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Artisanal Activism in the French Commune—Gould

*Women's Groups and the Transformation of U.S. Politics,
1890–1920*—Clemens

*Racial Differences in Household and Family Structure, circa
1900*—Morgan, McDaniel, Miller, and Preston

A Status-based Model of Market Competition—Podolny

An Evolutionary Ecological Theory of Expropriative Crime—
Vilà and Cohen

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WU- HO- 435- 574 P14

- 923 *Guerrillas and Revolution in Latin America: A Comparative Study of Insurgents and Regimes since 1956* by Timothy P. Wickham-Crowley
JEFF GOODWIN
- 924 *The Unquiet Woods: Ecological Change and Peasant Resistance in the Himalaya* by Ramachandra Guha
MICHAEL REYNOLDS
- 926 *Military Organizations, Complex Machines: Modernization in the U.S. Armed Services* by Chris C. Demchak
CHRISTOPHER DANDEKER
- 928 *Democracy at Work: Changing World Markets and the Future of Labor Unions* by Lowell Turner
MADELAINE GERBAULET-VANASSE
- 930 *Japan's California Factories: Labor Relations and Economic Globalization* by Ruth Milkman
JAMES LINCOLN
- 933 *Threads of Solidarity: Women in South African Industry, 1900-1980* by Iris Berger
SONYA O. ROSE
- 934 *Job Queues, Gender Queues: Explaining Women's Inroads into Male Occupations* by Barbara F. Reskin and Patricia A. Roos
DENISE D. BIELBY
- 936 *Gender Inequality: A Comparative Study of Discrimination and Participation* by Mino Vianello and Renata Siemieniska
JULIE ELWORTH AND SZONJA SZELÉNYI
- 938 *Women, Men and Time: Gender Differences in Paid Work, Housework and Leisure* by Beth Anne Shelton
JULIE BRINES
- 940 *The Overworked American: The Unexpected Decline of Leisure* by Juliet B. Schor
JOHN WILSON
- 942 *Making Fast Food: From the Frying Pan into the Fryer* by Ester Reiter

Dishing It Out: Power and Resistance among Waitresses in a New Jersey Restaurant by Greta Foff Paules
ROBIN LEIDNER
- 945 *Down the Backstretch: Racing and the American Dream* by Carole Case
JACK KATZ

K-P-14

150
Am 35

CONTENTS

- 721 Trade Cohesion, Class Unity, and Urban Insurrection: Artisanal Activism in the Paris Commune
ROGER V. GOULD

- 755 Organizational Repertoires and Institutional Change: Women's Groups and the Transformation of U.S. Politics, 1890-1920
ELISABETH S. CLEMENS

- 799 Racial Differences in Household and Family Structure at the Turn of the Century
S. PHILIP MORGAN, ANTONIO MCDANIEL, ANDREW T. MILLER, AND SAMUEL H. PRESTON

- 829 A Status-based Model of Market Competition
JOEL M. PODOLNY

- 873 Crime as Strategy: Testing an Evolutionary Ecological Theory of Expropriative Crime
BRYAN J. VILA AND LAWRENCE E. COHEN

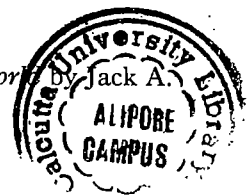
Book Reviews

- 913 *Feminist Methods in Social Research* by Shulamit Reinharz with Lynn Davidman
LAUREL RICHARDSON

- 915 *What's Wrong with Ethnography? Methodological Explorations* by Martyn Hammersley
DONILEEN R. LOSEKE

- 916 *Issues and Alternatives in Comparative Social Research*, edited by Charles C. Ragin
GILBERT ROZMAN

- 919 *Revolution and Rebellion in the Early Modern World* by Jack A. Goldstone
JOHN MARKOFF



- 946 *Equal Parenthood and Social Policy: A Study of Parental Leave in Sweden* by Linda Haas
MARJORIE E. STARRELS
- 948 *Raised in East Urban: Child Care Changes in a Working Class Community* by Caroline Zinsser
MARGARET K. NELSON
- 950 *Feeding the Family: The Social Organization of Caring as Gendered Work* by Marjorie DeVault
SARAH FENSTERMAKER
- 952 *Women with Alcoholic Husbands: Ambivalence and the Trap of Codependency* by Ramona M. Asher
NORMAN K. DENZIN
- 954 *Women, Violence, and Social Change* by R. Emerson Dobash and Russell P. Dobash
CHERYL HYDE
- 956 *On Divorce* by Louis de Bonald
ROBERT ALUN JONES
- 958 *The Resilience of Christianity in the Modern World* by Joseph B. Tamney
JAMES A. BECKFORD
- 960 *God's Warriors: The Christian Right in Twentieth-Century America* by Clyde Wilcox
ERLING JORSTAD
- 961 *Fundamentalisms Observed*, edited by Martin E. Marty and R. Scott Appleby
BENTON JOHNSON
- 963 *Representing Belief: Religion, Art, and Society in Nineteenth-Century France* by Michael Paul Driskel
JULIA BERNARD
- 965 *The Emergence of Liberation Theology: Radical Religion and Social Movement Theory* by Christian Smith
MARIE AUGUSTA NEAL
- 967 *Corpus Christi: The Eucharist in Late Medieval Culture* by Miri Rubin
MICHAEL P. CARROLL
- 969 *Gendered Spaces* by Daphne Spain
JOY CHARLTON

- 971 *Women Watching Television: Gender, Class and Generation in the American Television Experience* by Andrea L. Press
TAMAR LIEBES
- 973 *Media Events: The Live Broadcasting of History* by Daniel Dayan and Elihu Katz
DAVID I. KERTZER
- 974 *Sport in Australian Drama* by Richard Fotheringham
WILEY LEE UMPHLETT
- 976 *The Politics of Medical Encounters: How Patients and Doctors Deal with Social Problems* by Howard Waitzkin
DOUGLAS W. MAYNARD
- 979 *Inheriting Madness: Professionalization and Psychiatric Knowledge in Nineteenth-Century France* by Ian Dowbiggin
BARRY GLASSNER
- 981 *Melancholy and Society* by Wolf Lepenies
MICHAEL HAMMOND
- 982 *Pain as Human Experience: An Anthropological Perspective*, edited by Mary-Jo DelVecchio Good, Paul E. Brodwin, Byron J. Good, and Arthur Kleinman
RICHARD A. HILBERT
- 984 *Into the Valley: Death and the Socialization of Medical Students* by Frederic W. Hafferty
CHARLES L. BOSK
- 986 *Overcoming the Odds: High Risk Children from Birth to Adulthood* by Emmy E. Werner and Ruth S. Smith
JANET L. LAURITSEN
- 988 *Children in Poverty: Child Development and Public Policy*, edited by Aletha C. Huston
DANIEL T. LICHTER
- 990 *Hidden Anxieties: Male Sexuality, 1900–1950* by Lesley A. Hall
JOHN H. GAGNON
- 992 *Law, Sexuality, and Society: The Enforcement of Morals in Classical Athens* by David Cohen
CHRISTIE DAVIES
- 993 *Social Structure and Testosterone* by Theodore D. Kemper
LIONEL TIGER

IN THIS ISSUE

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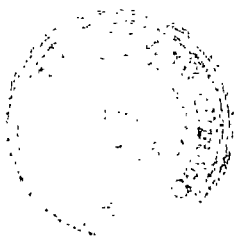
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Trade Cohesion, Class Unity, and Urban Insurrection: Artisanal Activism in the Paris Commune¹

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University of Chicago

Sociologists and historians generally agree that working-class protest in 19th-century France relied on the close-knit networks and corporate solidarity of artisanal trades. But urban uprisings invariably mobilized workers from a broad range of trades, a fact which some scholars have interpreted as evidence of growing class consciousness among French workers. This article shows that social organization within trade groups cannot account for insurgency in the Paris Commune: workers from close-knit occupational groups participated at lower rates than those in weakly organized trades. The reason was that Parisian workers were mobilized for insurgency through neighborhood networks, not through their membership in craft groups. The disappearance of trade boundaries during insurrections did not, therefore, reflect the emergence of class unity, but rather a shift from trade to neighborhood as the organizational framework for the mobilization of protest.

The revolutions of 1830, 1848, and 1871 in France have meant a lot to sociologists and historians studying working-class protest. The fact that the rank and file of these uprisings consisted of workers from a wide range of occupations has suggested to many that labor militancy underwent a transformation during the middle of the 19th century, as narrow, old-regime trade loyalties yielded to a generalized allegiance to the working class as a whole (Aminzade 1981; Katznelson 1986; Perrot 1986; Sewell 1986). According to this view, the participation of diverse trades in

¹ This research was supported by fellowship grants from the National Science Foundation, the Krupp Foundation, the Social Science Research Council, and the French Government's Chateaubriand Fellowship program. I thank Tom Ertman, Roberto M. Fernandez, Patrice Higonnet, Peter V. Marsden, Theda Skocpol, Harrison C. White, and the *AJS* reviewers for their helpful comments. Direct correspondence to Roger V. Gould, Department of Sociology, University of Chicago, 1126 East 59th Street, Chicago, Illinois 60637.

urban insurrections reflected—and also helped crystallize—the emergence of class consciousness among French workers.

This argument often appears in studies that provide convincing support for what Traugott (1985) has called the “thesis of artisanal activism”—that is, the thesis that craft communities played a pivotal role in the mobilization of working-class protest. The militants of 19th-century France (and other European societies as well) were not the industrial proletarians of *The Communist Manifesto* (Marx and Engels [1848] 1967) or *The Civil War in France* (Marx [1871] 1940), but rather skilled handicraft workers with long-standing corporate traditions.² The cohesiveness of these occupational groups constituted the principal source of solidarity in the French labor movement during the 1800s (Shorter and Tilly 1974; Tilly, Tilly, and Tilly 1975; Moss 1976; Hanagan 1980; Sewell 1980; Aminzade 1981). As a result, militant workers framed their struggle against capitalist development in terms of narrowly defined occupational identities: working-class organizations typically emerged during strikes and took the form of trade-specific mutual-aid societies and producers’ cooperatives (Perrot 1974; Sewell 1980; Tilly 1986; see also the short accounts of a large number of such episodes that appear in *Ministère du Commerce* [1904]). In the utopian vision that dominated French socialism from the 1830s until the end of the century, cooperative ownership of the means of production would gradually replace private property and forge a postcapitalist society governed by a council of worker-representatives from each trade (Moss 1976; Sewell 1980; see Freymond [1962] for evidence of the way this vision developed in the French section of the International Workingmen’s Association).

This leads to a puzzling question. If craft communities provided the key social foundation for labor militancy throughout the 1800s, how is it possible that during the same period craft loyalties gradually dissolved and were ultimately replaced by universal class consciousness as the basis for collective action?

I argue that it is not possible. If the cross-trade nature of urban insurrections had been the result of a decline in importance of occupational loyalties, then the dissolution of trade boundaries should have been apparent in smaller-scale forms of protest as well. Yet the labor movement that reemerged in the 1860s after a decade of repression under Louis Bonaparte was as segmented along trade lines as it had been before 1848. Labor protest following the Commune similarly failed to exhibit

² In this article, I shall use the terms “artisanal worker,” “skilled craft worker,” and “handicraft worker” interchangeably. The persistence of handicraft production in French industry in the late 19th century makes these terms largely synonymous (see Hanagan 1977).

significant signs of class unity. While workers' organizations occasionally contributed to each other's strike funds, these instances of cooperation were rare until the emergence of revolutionary syndicalism in the 1880s; intertrade antagonism was much more common, as when members of rival orders of *compagnonnage* brawled in the streets or skilled construction workers excluded unskilled excavators (*terrassiers*) from their unions (Perrot 1974; Moss 1976; Hanagan 1980; Price 1987; Gould 1990).

Conversely, if trade communities were a key factor in mobilizing artisanal participation in uprisings, why do we not observe the same kind of trade segmentation in these events that we see in industrial conflict? Why, for instance, were there no trade-specific barricades in 1848 or single-occupation National Guard units in 1871? Because the artisanal activism thesis focuses on the cohesiveness of craft groups in explaining the full range of protest activities in 19th-century Europe, it has difficulty making sense of this difference between insurgency and small-scale labor strife.

In this article, I offer an answer to this puzzle that avoids the claim that craft solidarity was declining at the same time that it provided an enduring organizational resource for activism. I argue instead that insurrections were able to bring workers from a variety of trades together because the mobilization of urban insurgency did not depend on craft-group cohesion. Using data on rank-and-file insurgents in the Paris Commune of 1871, I show that, while skilled craft workers did indeed play a prominent role in the uprising, it was not because they were integrated into cohesive occupational groups. In fact, the most organized and solidary crafts were systematically *underrepresented* in the ranks of insurgents. Net of other factors, the most active participants in the insurrection were artisanal workers from weakly organized trades.

This finding can only be understood, I maintain, by recognizing that the mobilization of urban insurgency relied on the social organization of neighborhoods—networks of social ties that cut across craft communities and thus provided an alternative framework for collective action. Close-knit trade groups participated in the Commune in proportionally smaller numbers because they were more weakly tied to neighborhood networks. In contrast, occupational groups with weak craft communities were more closely linked to the associational life of the urban neighborhood and were, therefore, recruited to the insurrection at higher rates. Consequently, it is the very fact that insurrections did *not* rely on trade networks that explains why the rank and file of the Commune included workers from a broad range of trades; and it was also for this reason that the insurgents of 1871 came predominantly from trades that the artisanal activism thesis would characterize as the most quiescent.

Following a brief historical overview and a description of the data

sources, I present documentary evidence of the connection between craft-group cohesion and shop-floor protest, with particular emphasis on two of the largest industries in 19th-century Paris: metalworking and construction. Archival material on strikes and workers' organizations shows that occupational groups with the most cohesive craft communities and the greatest control over skills were most successful in organizing shop-floor protest in the 1860s; yet arrest records from 1871 reveal that workers from these trades participated at consistently low rates in the defense of the Commune. Moreover, regression analysis shows that this pattern holds, controlling for other factors, across the whole range of artisanal occupations.

THE PARIS COMMUNE OF 1871

When Louis Bonaparte was taken prisoner by the Prussian army at Sedan in September 1870, crowds in Paris swept away the Second Empire in a matter of hours, and without a shot being fired.³ A group composed principally of republican legislators formed a provisional Government of National Defense, promising a Constituent Assembly and a successful conclusion to the Franco-Prussian War. But as Bismarck's forces closed in on the capital, it became clear to the people of Paris that the government, supported by the war-weary rural population, was preparing to capitulate. Increasing anger over this perceived betrayal, combined with long-standing resentment of limitations on the city's municipal liberties, fostered a revolutionary atmosphere in Paris during the four-month winter siege. Popular calls for a republican *levée en masse* prompted the government to arm the Paris National Guard, a militia force consisting—at least on paper—of 300,000 able-bodied men. But this measure only led to greater frustration when the city's military governor demonstrated his reluctance to send the guard into battle. Radical and socialist activists organized working-class guard battalions into a democratic National Guard Federation, which had close ties to the city's labor movement, above all to the Paris chapter of the International Workingmen's Association.

An armistice was signed in January 1871 and was followed by a humiliating peace treaty in February. But the end of the Franco-Prussian War only led to further conflict: under the terms of the armistice, the French authorities were responsible for disarming the Paris National Guard.

³ The following account is based primarily on the secondary accounts of Lissagaray ([1876] 1969), Horne (1965), Rougerie (1971, 1988), Edwards (1971), and Serman (1986). Other works on the Commune include Gaillard (1971), Greenberg (1973), and Tombs (1982).

Revolution came when the government, under the leadership of the conservative Adolphe Thiers, tried to confiscate the guard's artillery, which, during the peace negotiations, had become the chief symbol of Parisians' defiance toward the Prussians and toward their own rulers (now dubbed the "Government of National Treason"). On March 18, a dawn attack on the artillery parks of Montmartre and Belleville went awry when crowds of men, women, and children surrounded the government troops and convinced them not to fire their weapons. Thiers ordered the army to retreat to Versailles along with city officials, and the National Guard Federation's Central Committee took control of the city's public buildings.

Attempts at conciliation between the committee and the government at Versailles failed, and the following day the committee announced elections to the Commune, combining the symbolism of the Revolutionary Commune of 1789 with an allusion to the nationwide movement for municipal liberties—a movement that had already led to an uprising in Lyon and was about to spark communal insurrections in the cities of Marseilles, Toulouse, Saint-Étienne, and Le Creusot (Edwards 1971; Greenberg 1973). The Communal Council, inaugurated on March 26, quickly demonstrated its democratic socialist leanings by passing a series of progressive measures, including the establishment of workers' cooperatives, the creation of a labor exchange in each *mairie* (town hall), the separation of church and state, and the abolition of night work for bakers.

While the Commune declared its solidarity with the citizens of other French cities who had mobilized their own communal movements against the Versailles government and the "rural majority," it claimed no authority outside the walls of Paris. Still, Thiers would not tolerate what he saw as the rule of an undisciplined mob in France's capital city, and Paris was subjected to a second siege—this time by the French rather than the Prussian army—beginning on March 30.

The link between the National Guard and the Commune made this the first French uprising in which the insurgent forces constituted a genuine military organization. Even so, guard battalions were hardly a match for the larger, better-armed, and better-trained Versailles army. The government troops, led by the Third Republic's future president, Marshal MacMahon, entered the city's western gates on May 21; the seven days of barricade fighting that ensued have been known ever since as the *semaine sanglante*, or "bloody week." Modern historians estimate that over 20,000 Parisians died during the fighting, many of them in summary executions carried out as each barricade fell to the invading army (Horne 1965; Edwards 1971; Rougerie 1964*a*, 1964*b*, 1988; Tombs 1982; Serman 1986).

For thousands of other insurgents, however, the drama had not ended. The army arrested more than 36,000 people during the fighting and in the ensuing weeks; most of these detainees spent a year in temporary prisons awaiting preliminary investigations into their cases. Ultimately, 23,000 prisoners were released without trial, while the remaining 13,000 were tried by 26 *conseils de guerre*, or military courts. Approximately 2,500 were acquitted; 93 received death sentences, of which 23 were actually carried out; 4,500 were deported to penal colonies in New Caledonia; 6,000 were sentenced to prison or hard labor for periods ranging from a few months to 20 years. Nearly 3,000 other suspected insurgents—many of whom had died nameless deaths on the barricades—were tried and sentenced to deportation in absentia.

SOURCES

As an ironic result of the massive repressive effort, the Commune left abundant marks on the historical record. The data used in this article are drawn from a sample of the trial dossiers preserved in the Archives Historiques de l'Armée de Terre in Vincennes, France (hereafter, "Army archives"). An initial list of arraignees was drawn up by selecting every seventh name from the *Dictionnaire biographique du mouvement ouvrier français* (Maitron 1968).⁴ But because the *Dictionnaire biographique* is based on dossiers compiled at the Ministry of Justice when convicted prisoners applied for pardons, this initial list excludes anyone who was acquitted. A separate list of acquitted prisoners was therefore drawn directly from the records in the Army archives. Because of limitations on public access to the archives' inventory of dossiers, the only way to sample acquittals was by selecting two letters of the alphabet at random and consulting all the dossiers for acquitted prisoners whose names began with one or the other of these two letters. The final sample consisted of 1,740 insurgents who received guilty verdicts, 177 who were acquitted, and 283 who were tried and convicted in absentia. In the analyses reported here, the data are weighted to compensate for the overrepresentation in the sample of convicted insurgents.

While trial records constitute the only substantial source of information on the rank and file of working-class militancy, the data derived from such records are inevitably influenced in part by the biases and preconceptions of the officials in charge of the judicial process. In the analyses

⁴ Actually, the enumeration process was somewhat more complicated, since the *Dictionnaire biographique* (Maitron 1968) contains names of labor militants who were not connected with the Commune, and who were consequently not tried by the military courts. These entries were ignored in counting every seventh name.

presented below, controls are used wherever possible to correct for the bias introduced into the arraignee sample by prejudicial practices on the part of army officers and military courts. In particular, analyses of rates of participation in the Commune control for past criminal convictions and marital status, since the military courts accorded preferential treatment to detainees who were married and who had no criminal record. An extensive discussion of the nature, extent, and appropriate corrections for bias in the sample is available separately from the author.

Trial dossiers for each individual on the list were consulted at the Army archives and yielded basic demographic and occupational information as well as information concerning the defendants' activities under the Commune. Specifically, the data set used here includes each defendant's age, occupation, place of birth, address, marital status, number and type of previous encounters with the law, and formal rank (if any) in the Paris National Guard.

The analyses presented below focus on the determinants of insurgent participation across occupational groups; consequently, the data from the arraignee sample are initially classified into four broad socioprofessional categories: bourgeois/professionals, white-collar employees, skilled artisanal workers, and unskilled laborers (see table 1 below). In subsequent analyses that provide a more direct test of the artisanal activism thesis, skilled artisanal workers in the sample have been subdivided into 24 professional categories (see table 2 below). The other three broad socioeconomic categories—bourgeois/professionals, white-collar employees, and unskilled laborers—are not relevant for a test of whether craft social organization was related to insurgency and are therefore excluded from the regression analyses. Insurgent activity among the nonartisanal population of Paris was nonetheless far from negligible; for a study that includes these groups in an exploration of the social foundations of insurgent mobilization in general, see Gould (1991).

ARTISANAL ACTIVISM AND URBAN INSURRECTION

As Traugott (1985) has pointed out, past research on the participation of urban artisans in insurrections has typically demonstrated only their absolute numerical importance; since artisans also constituted the bulk of France's urban population during the 19th century, this leaves open the question of whether artisanal workers were disproportionately active in such events. Consequently, the first issue to address is whether artisanal workers were central to the 1871 insurrection in relative as well as absolute terms.

The figures reported below refer only to males, since the overwhelming majority of arrested insurgents were men. While this fact in part reflects

the assumption on the part of the authorities that men were more likely than women to have fought on the barricades, to a greater extent it results from the way in which participants in the insurrection were mobilized: most of the fighting both outside and inside the city was the duty of the National Guard. Some women did serve as nurses and provisioners to guard battalions in their areas, but only the men, who by decree of the Commune were required to serve if they were between the ages of 19 and 40, were supplied with arms. Thus for largely organizational reasons, the barricade fighting of 1871 was primarily limited to men.

Table 1 reports the distribution of male Parisians and arraignees falling into the categories bourgeois/professional, white-collar employees, artisanal workers, and unskilled workers. (Soldiers who were charged with participating in the insurrection because they had remained in Paris after March 18 are not included in this table; while a number of soldiers did indeed take part in the uprising, they were not part of the population of the capital and consequently do not appear in census figures.) It is immediately clear that artisanal workers are the only category that was overrepresented among arraignees: bourgeois/professionals and white-collar employees are present in the sample in proportions that are well below those for the Paris population as a whole, while the proportion of unskilled workers is roughly equal to that in the general population. In short, insurgency in 1871 was dominated by those from a working-class, and more particularly artisanal, social background. This result leaves little doubt that skilled handicraft workers were generally more active in the 1871 uprising than members of any other socioprofessional category. As far as 1871 is concerned, the broad empirical premise of the artisanal activism argument—namely, that artisanal workers were more militant overall than unskilled workers—appears to be confirmed.

TABLE 1
OCCUPATIONAL DISTRIBUTION OF PARIS POPULATION AND 1871 ARRAIGNEES

| Socioprofessional Categories | Paris Population* | % | Arraignees† | % |
|-------------------------------|-------------------|------|-------------|------|
| Bourgeois/professionals | 254,361 | 27.3 | 251 | 10.5 |
| White-collar employees | 144,007 | 15.4 | 272 | 11.4 |
| Artisanal workers | 396,131 | 42.5 | 1,521 | 63.6 |
| Unskilled workers | 137,948 | 14.8 | 347 | 14.5 |
| Total | 932,447 | 100 | 2,391 | 100 |

* Population figures refer to males only.

† Figures correspond to total number from each category in the arraignee sample, weighted to account for underrepresentation of acquittals.

But can this higher rate of activism among artisanal workers be attributed to craft social organization? To state the question another way, Did skilled workers form the core of the Commune's rank and file because they belonged to craft communities whose densely knit social structure helped to mobilize them into the insurgent effort? If this is the explanation, it should be possible to demonstrate that variation in participation rates across trades, but *within* the category of skilled workers, was itself positively related to trade-based social cohesion. If artisanal solidarity is to account for participation in insurgency, then we should find that artisanal crafts with close-knit professional communities exhibited higher rates of activism than crafts with weak social organization. Moreover, this relationship should hold net of other factors that might influence participation but which are not themselves connected to trade-based social cohesion. In the next two sections, I shall show that the contrary was the case: the most cohesive craft groups were most likely to have organized for shop-floor protest in the 1860s, but least likely to fight on the barricades of 1871.

Measuring Insurgent Participation

To explore the determinants of insurgent participation specifically among artisanal workers, two measures are derived from the data. First, the absolute number of people in the arraignee sample from each trade is used to estimate the number of actual participants that trade contributed to the insurrection; divided by the number of Parisians for each trade, this variable (arrest rate) estimates rates of participation for the 24 occupational groups shown in table 2.⁵

Interpreting arrest rates as participation rates requires two assumptions. First, it is assumed that there were no systematic differences between those killed during the fighting and those who survived; the essentially arbitrary nature of the mass executions strongly suggests that those killed had not, in general, fought more courageously or with more deter-

⁵ An ideal research design would involve a probability sample of Parisians, with a set of continuous variables measuring the extent of participation for each. As a compromise, I am forced to aggregate to the level of occupational categories and measure overall *rates* of participation. In one sense, however, this strategy simply makes the logic of my research more explicit than it would be in the case of an individual-level analysis: since the central question is whether trade cohesion affected insurgent mobilization, the principal variables of interest have to be measured on the level of trades. Thus, even if the individual were the unit of analysis, the key independent variables would still be defined on the group level. Also, since the independent variables are for the most part drawn from census data rather than the arraignee sample itself, selection bias should not pose a problem.

TABLE 2
MEASURES OF PARTICIPATION AND ORGANIZATIONAL RESOURCES
AMONG ARTISANAL TRADES

| Occupational Category | Population | Arrest Rate* | Officer Rate† | Concentration | Worker- Employer Ratio |
|-------------------------------|------------|-----------------|------------------|---------------|------------------------------|
| Bakers, pastry workers ... | 7,200 | 2.01 | .21 | .305 | 2.93 |
| Butchers | 4,800 | 1.59 | .48 | .300 | 1.67 |
| Distillers, other food | 2,240 | 2.03 | .00 | .344 | 3.58 |
| Painters, plasterers | 9,100 | 5.51 | 1.84 | .299 | 11.04 |
| Masons, roofers | 25,640 | 5.96 | .77 | .288 | 13.94 |
| Joiners, carpenters | 17,100 | 4.67 | 1.47 | .304 | 12.47 |
| Other construction | 17,500 | 5.99 | 1.26 | .286 | 9.42 |
| Furniture workers | 33,100 | 2.41 | .62 | .417 | 6.97 |
| Tailors | 14,000 | 2.28 | .60 | .413 | 7.28 |
| Shoemakers | 18,000 | 4.31 | .97 | .294 | 8.48 |
| Other clothing, textile | 14,400 | 4.38 | 1.11 | .331 | 11.72 |
| Machine builders | 13,700 | 4.44 | 1.05 | .307 | 17.15 |
| Foundry workers | 3,100 | 2.94 | .49 | .497 | 6.54 |
| Forgers, other metal | 22,100 | 4.13 | 1.41 | .365 | 9.68 |
| Jewelers | 15,000 | 3.34 | .96 | .536 | 6.52 |
| Instrument makers | 7,100 | 3.10 | .75 | .361 | 3.19 |
| Ceramics, chemical | 10,500 | 2.39 | .58 | .360 | 6.05 |
| Printers, bookbinders | 16,000 | 3.71 | 1.33 | .347 | 11.26 |
| Luxury-goods workers | 7,300 | 1.77 | .62 | .597 | 5.97 |
| Barbers | 2,100 | 5.07 | .00 | .307 | 1.52 |
| Carriage makers | 7,800 | 3.61 | .78 | .322 | 8.30 |
| Woodworkers | 8,000 | 5.03 | 1.33 | .401 | 6.27 |
| Tanners, other leather | 6,200 | 3.31 | .49 | .391 | 10.29 |
| Service workers | 13,400 | 3.46 | .91 | .351 | 3.45 |

* Total arraignees in trade *i* per 100 practitioners of trade *i*.

† Total arraigned National Guard officers in trade *i* per 100 practitioners of trade *i*.

mination than those who were arrested during the fighting but whose lives were spared. Second, it must be assumed that the repressive forces were not completely arbitrary in their decision to arrest and try some suspected insurgents and to release others. Analyses not presented here (see Gould 1990) show that the authorities used the insurrection as an opportunity to eradicate what they perceived as undesirable elements of the Parisian population; but these results also demonstrate that in deciding whom to release and whom to arraign the authorities made a genuine effort to distinguish willing participants in the fighting from those who had refused to take up arms or had been coerced into defending the Commune. Nonetheless, since investigating officers routinely used prisoners' past criminal records as evidence against them, and in various

other ways put into practice the dominant middle-class view that working-class protest was equivalent to working-class criminality, the proportion of each occupational group with one or more criminal convictions is included as a control in the analyses presented here.

The second measure is the rate at which each trade contributed officers to the Paris National Guard, which rallied to the cause of the Commune in the initial uprising of March 18, 1871, and which—as noted above—was principally responsible for the defense of the city against the Versailles government. Although the members of each battalion of the National Guard had elected their officers the previous autumn in preparation for the four-month Prussian siege of the capital, most units held new elections after the proclamation of the Commune (Clifford 1975). In general, then, men who remained or became officers in the guard after March 18 were perceived by their peers as worthy leaders of a largely revolutionary organization and were themselves willing to bear responsibility both for the pursuit of the Commune's opponents within the capital and the defense of the city against the French army. Again, this variable is expressed as the estimated number of National Guard officers from each trade, divided by the estimated total number of Parisian men practicing that trade (see table 2).

Independent Variables

Independent variables measuring various attributes of Paris occupational groups are derived from a 2% sample of voters in 1871 electoral lists (Archives de Paris, Listes Électorales), 1866 census data (Statistique Générale de la France 1869), and the insurgent sample. As individual-level information on the social characteristics of the population of Paris in this period is hard to come by, the sample of arrestees is an uncommonly rich source of data on the Parisian working class. Demographic variables derived from the arraignee sample are the proportion of each occupational group married at the time of the uprising and the proportion with one or more previous criminal convictions.⁶ For marriages, the overall percentages are nearly equal to those for the adult Parisian population (Loua 1873). With regard to the second variable, census data provide no assistance; but because of army officers' bias against releasing people with past convictions, the proportions observed in the arraignee sample

⁶ For the marital status variable, arrestees listed as *vivant en concubinage* (living with someone out of wedlock) were coded as married. Officers' reports in the trial dossiers clearly show that they viewed such liaisons as evidence of "immorality"; nonetheless, most of these arrangements were durable and stable, frequently included children, and therefore constituted marriages in all but the formal sense.

are likely to be higher than for the Paris population. Even if this is the case, however, relative differences between occupational groups will be preserved in the arraignee data. In any event, the proportion of arraignees with past convictions—about one in four—does not seem especially high for an urban working-class population, especially when it is taken into account that the majority of these convictions were for relatively minor offenses such as vagrancy, public drunkenness, and insulting police officers.

Finally, the key independent variables are measures of the extent of social organization within each occupational group. Typical measures of organizational strength, such as unionization rates, are unavailable for this period, and since most trades had organized some kind of association in Second Empire Paris, the mere presence of a formal organization is not an adequate measure. In any case, as I shall demonstrate below, what counted in determining whether members of a trade were successful in mobilizing resistance to employers was not the existence of organizations but the informal social solidarity of craft groups.

One characteristic of craft groups that profoundly influenced their social cohesion was residential concentration. Occupational groups that lived in tightly clustered communities were more likely to have dense intraoccupational social networks. Residential proximity facilitated the extension of corporate solidarity from the workplace into informal aspects of social life, epitomized in the occupationally segregated wineshop or "cabaret." These social centers facilitated the exchange of job information and occasionally even functioned as small banks (Agulhon et al. 1983). In more turbulent times, cabarets often provided the setting for the organization of protest: for instance, during the bitterly contested 1867 bronze-workers' strike, the Paris police prefect noted that "the Commission [of the Bronze-Workers' Mutual Credit Society] meets regularly at the establishment of the *marchand de vins* [wineshop owner] Pomey, 11 rue de l'Oseille, or that of Mogenier, *marchand de vins* at 5 rue St. Claude."⁷ Thus the geographical clustering of trade groups encouraged close-knit occupational networks, which in turn enhanced workers' organizational resources. (I shall discuss this process in greater detail below; for other discussions of the connection between residential concentration and solidarity, see Gaillard [1977], Hanagan [1980], and Scott [1974].)

Electoral lists from July 1871 were used to estimate the distribution of each occupational group among the 20 Paris arrondissements: for each of the 20 lists, occupation was recorded for every fiftieth name. Using

⁷ Archives Nationales, Paris (AN), Ser. F¹² 4652, Ministère du Commerce, Ministère de l'Intérieur.

the distribution across all 20 districts for each occupation, residential concentration was measured using a version of the spatial clustering index proposed by White (1986; see also Massey and Denton 1988). This measure is a refinement of standard measures of concentration in that it corrects for the "checkerboard problem" by taking into account the spatial distribution of districts.⁸ For example, an occupation whose members were divided evenly between just two out of the 20 districts would receive a concentration score of 1.0 (the theoretical maximum) if the two districts were geographically contiguous, but 0.5 if they were not; if the group were divided among three noncontiguous districts, the score would be .33, and so forth. Concentration scores are reported in table 2.

A second crucial factor affecting a craft group's organizational capacity involves the degree of control workers have over the production process. There are, of course, many ways in which this variable might be measured; however, the best choice in terms of measurability across a wide range of trades is the number of workers per employer in each trade. As numerous scholars have observed, one of the central elements of artisanal solidarity throughout the 19th century was the familiarity and interdependence that emerged among artisans working together in teams or in small shops and over long periods (Scott 1974; Hanagan 1980; Aminzade 1981). Conversely, increases in the size of industrial establishments were related to the intensified division of labor, and resulting economies of scale, brought about by capitalist production (Sewell 1980). Since the division of tasks made training easier, workers in trades such as shoemaking and garment manufacture gradually lost their control over skills at the same time that the work process lost its collective quality. Thus, generally speaking, the organizational advantages of workplace autonomy dwindled as employers built larger plants, increased the division of labor, and tried to enforce factory discipline (Shorter and Tilly 1974; Perrot 1986).

Worker-employer ratio should not be confused with shop size, since the latter measure would mask the profound effects on worker autonomy of *marchandage* (subcontracting) or *confection*, a manufacturing system in which semiskilled outworkers, many of them women, produced standardized parts for shoes or garments that were later assembled in factories. The large number of employees working at home, in near isolation,

⁸For each occupational group, the measure is calculated as

$$\sum_i p_i \sum_j c_{ij} p_j,$$

where p_i is the proportion of the group in district i , p_j is the proportion in district j , and $c_{ij} = 1$ if districts i and j share a border, and $c_{ij} = 0$ otherwise.

presented an even greater obstacle to organization than a simple increase in the size of the average workshop. Again using 1866 census data, then, trades are characterized in the analyses below according to the ratio of male and female workers to employers.

Clearly, residential concentration and worker-employer ratio do not measure organizational strength itself, but rather two characteristics of craft groups that can be expected to have contributed to their solidarity and organizational potential. Before proceeding to the quantitative analysis of insurgent participation, therefore, it is worth demonstrating that these two factors were in fact intimately related to the effectiveness with which various occupational groups struggled against employers during the period leading up to the Commune. Because systematic information on working-class protest in this period is unavailable, I will rely primarily on qualitative evidence concerning the metalworking and construction industries.

CRAFT COMMUNITIES AND LABOR PROTEST UNDER THE SECOND EMPIRE

In 1864, as part of a series of liberalizing reforms, Louis Bonaparte pushed through France's parliament a law making strikes legal. This new freedom was highly circumscribed, however: workers were permitted to go on strike as individuals but forbidden to form unions or to interfere with anyone's "right to work." In other words, workers were legally entitled to refuse to work but could be imprisoned for trying to coerce others into joining their strikes. The only workers' organizations permitted under the new law were producers' cooperatives and mutual-aid societies; even these presumably innocuous bodies were required to solicit charters from the government and clear their choices of officers with the Ministry of the Interior.

Nevertheless, handicraft workers all over France took full advantage of the 1864 decree, and within a few months the country had been seized by a strike wave that seemed to many to threaten social collapse. By November of 1864, the Paris prefect of police had already reported serious citywide strikes organized by bookbinders, armchair makers, iron founders, copper founders, bronze workers, zinc workers, and others.⁹ In the course of many of these strikes, workers founded trade-specific fraternal societies (*sociétés de secours mutuels*), cooperatives, and even clandestine trade unions that were to last through the decade.

But the success of strikes and the strength of organizations were by no means uniform across trades. While it is fair to say that almost every

⁹ From AN, Ser. F¹² 4651, Ministère du Commerce.

moderate to large occupation formed some kind of association during the 1860s, the durability of these attempts at organization depended largely on the strength of the informal craft communities in which they emerged. A more focused examination of the experiences of workers in the metal and construction industries—which together employed nearly one-third of the Parisian work force—illustrates how closely the success of protest was linked to the social cohesion and workplace autonomy of trade groups.

Metalworking

Of all the trades practiced in France's capital, metalworking was the one that had changed the most as a result of industrial capitalism. The development of railroads, the increasing use of iron in construction, and the growing demand for steam-driven machinery in industries like textiles and sugar refining fueled a dramatic expansion in metalworking under the Second Empire. The most visible sign of this process was the growth in size of metallurgical plants, particularly in the northern industrial regions and in Alsace and Lorraine (Clapham 1968; Duveau 1946; Price 1987). Although it was by no means the center of heavy industry in France, Paris too was home to numerous foundries and machine-building factories, some of them employing hundreds of workers. Many of the new factories were built in the suburbs, in part to evade the *octroi* tax levied within the city walls. Still, the two largest engineering and machine-building enterprises were located inside Paris: the Gouin locomotive factory, in the seventeenth arrondissement, employed 1,000 workers, while the Cail metal works, in the fifteenth, numbered 2,000 (Duveau 1946; Gaillard 1960).

Yet the appearance of large factories did not by itself signal a decline in the artisanal character of work in the metal trades. The impact capitalist development had on workers' capacity for protest varied across specific occupations in the metalworking industry and depended on the extent to which technical innovations and changes in the organization of production had eroded workers' autonomy and control over skills.

In machine construction, the demand for standardized machinery on the part of national railroad companies, textile manufacturers, and sugar refineries led industrialists to introduce growing numbers of professionally trained engineers into the production process. At the mammoth Cail works, this move led to a managerial revolution that, spurred by its spectacular effects on productivity, quickly spread across France (Nakajima 1985; Edmonson 1987). By the 1860s, most machines were designed, not by artisans using empirical, trial-and-error methods in the workshop, but by white-collar employees in a formally and physically separate draft-

ing room (*bureau d'études*). Engineers and draftsmen presented shop foremen with a completed machine design, deviations from which required formal approval by management. This change in turn facilitated the division of machine construction into a set of independent and reproducible tasks, enabling employers to train new personnel quickly (and making it commensurately more difficult for workers to maintain their hold on skills). Finally, the standardization of designs and tasks facilitated cost control through the use of rationalized accounting methods, further undermining the autonomy of foremen and the machine builders (*mécaniciens*) who worked under them. Machine building increasingly became a semiskilled occupation, with shorter training periods and higher labor turnover (Gaillard 1960; Rougerie 1971). In short, even before the Commune, industrial development was already transforming the *mécanicien* into the *métallo* of the 20th century (for a discussion of the low levels of organization among such workers in the early 1900s, see Shorter and Tilly [1974]).

In contrast, workers in some branches of metallurgy—that is, in the production of metal as a raw material—retained their hold on skills well past the turn of the century. Despite a number of advances in the efficiency of iron and steel production (of which the development of the Bessemer converter is perhaps the best known, but by no means the most central, 19th-century example), European foundries continued to rely heavily on the judgment and informal knowledge of highly skilled artisans. Chemists had achieved only a rudimentary understanding of the complex reactions that occurred inside smelting furnaces, a problem that was exacerbated by the wide range in the chemical composition of iron ore from different regions.¹⁰ As a result, employers were unable in this period to find a technological replacement for the skills of puddlers and iron founders: experience, rather than science, told the ironworker how much coke to add, what areas of the molten ore needed to be exposed to the oxidants, and when the metal had reached the proper consistency for casting into molds or for hammering into wrought-iron blooms (Daumas 1968; Vial 1967).¹¹ It was, of course, only through long apprenticeships that new workers could be trained in foundry work, making founders

¹⁰ For instance, the Bessemer conversion process proved a disaster in areas where iron ore contained even moderate amounts of phosphorus; Bessemer himself had not encountered this problem because of the exceptionally low phosphorus content of the iron he had been using (Daumas 1968).

¹¹ Indeed, the decline of puddling and foundry work was ultimately the result, not of mechanization in iron production, but rather the increasing cost-efficiency of steel. The key innovation that brought about this change in steel manufacture was the Siemens-Martin open-hearth oven, which permitted continuous casting (Vial 1967; Landes 1968; Hanagan 1980).

correspondingly difficult to replace. The combination of experience, strength, and finesse this occupation required epitomized artisanal work in 19th-century France.

Control over skills meant control over who could enter the trade, with corresponding effects on the cohesiveness and social organization of occupational groups. The resulting difference between foundry workers and machine builders is reflected dramatically in their residential patterns: 1871 electoral lists show that 71% of iron founders lived in just four adjacent districts (the tenth, eleventh, twelfth, and twentieth arrondissements), while machine builders were fairly evenly scattered across the city (cf. the residential concentration indices reported for these two occupations in table 2). Thus the autonomy foundry workers continued to enjoy in the workplace contributed directly to their cohesion as a social group: residential clustering and stability put iron founders in a much better position than machine builders to maintain the rich associational life (e.g., in trade-specific cafes and bars) that scholars have repeatedly tied to artisanal activism (Shorter and Tilly 1974; Hanagan 1980; Sewell 1980; Aminzade 1981).

While comprehensive strike statistics do not exist for the Second Empire, the details available on trades like metallurgy and machine building leave little doubt as to the relationship between workplace autonomy, craft-group cohesion, and successful working-class mobilization. As noted earlier, iron foundry workers were among the first groups in Paris to go on strike in 1864, and the clandestine union or "resistance society" (*société de résistance*) that emerged during this strike remained active throughout the 1860s (Ministère du Commerce 1904; Gaillard 1960). Indeed, during the national upsurge in strikes in 1869 and 1870, foundry workers organized the largest strike in the capital: beginning as a wage dispute in the Cail works, the strike quickly spread to small workshops throughout the city and stopped work for three months. The strikers' demands ultimately included the abolition of piece-rates and subcontracting, extra pay for overtime, and even worker approval of foremen nominated by management (Chambre de Commerce 1875). Foundry workers were clearly aware that the stakes included not only the rate of compensation, but the crucial organizational resource they used to collectively defend their interests against employers: control over the work process.

What did not happen during this strike was as noteworthy as what did happen. Many of the workers at Cail were not founders, but machine builders as well as forgers, turners, fitters, and others specializing in the manufacture of finished products rather than casting. Despite their support for the foundry workers, these groups did not stop work themselves; thus, even as it became citywide among iron founders, the strike failed

to spread to the machine shops within the Cail works. In fact, machine builders failed to organize a single large-scale strike during this period: none of the three machine-building strikes that occurred between 1864 and 1871 involved more than one employer (Ministère du Commerce 1904).

The example of the Parisian metal trades makes a compelling case for the thesis of artisanal activism as it applies to shop-floor protest: to the extent that employers succeeded, through technical and administrative innovations, in dismantling the control artisanal workers exerted in the workplace, they were able to push capitalist development even further by dampening worker militancy. On the other hand, where limitations on technology and administrative rationality left workers with tight controls over skills and training, as in foundries, capitalist development was impeded by the resulting persistence of cohesive craft communities.

Construction

Early in the 19th century, the building trades in France were still extensively organized by *compagnonnage*, the clandestine journeymen's associations whose three main "sects" (the Enfants du père Soubise, the Enfants du Maître Jacques, and the Enfants de Salomon) maintained chapters in cities and towns all over the country. Masons, joiners, carpenters, and other artisans in construction or related crafts typically developed their skills through the institution of the Tour de France, which entailed traveling from town to town over a period of several years to acquire a thorough knowledge of regional specialties and trade secrets. Upon arriving in a new town, the *compagnon* would go directly to the *mère* (literally, "mother") or local headquarters of his sect, where fellow members would house him temporarily until they could find him employment. All three sects had chapters in Paris in the 19th century; consequently, itinerant construction workers had an elaborate system of social support available to them when they worked in the capital, either as part of the tour or on a seasonal basis.

Even when *compagnonnage* began to decline in importance, the common provincial origins of construction workers helped them to avoid an isolated existence during their annual migrations to Paris. While in the capital, workers in these trades remained together, working at the same construction sites and lodging in the same transient hotels. Gaillard (1977, p. 201), writes: "These professions helped to transport provincial cohesion and custom to the capital: the masons, for example, who lived in a group under the leadership of one of their number and ate their meals together in the same lodging-house, re-created their province inside Paris."

After the 1840s, however, this pattern began to change, as construction workers started moving to Paris permanently with their families. At one time most masons stayed in hotels in the fourth or fifth arrondissements, in the area surrounding the Place de Grève, where subcontractors traditionally gathered to assemble work teams (Rudé 1959; Chevalier 1958). But the massive expansion of construction during the Second Empire resulted in the proliferation of such gathering places as well as the establishment of subcontracting enterprises throughout the city; combined with the increasing number of workers taking up permanent residence in Paris, this trend helped to disperse the seasonal enclave around the Place de Grève (Gaillard 1977). Electoral lists show that, by 1871, only 28% of masons in Paris lived in the fourth and fifth arrondissements, with the remainder distributed more or less evenly throughout the city (see the residential concentration figures for various construction trades in table 2). The growing population of building workers not affiliated with *compagnonnage*, and the division of *compagnonnage* itself into rival sects—a rivalry that frequently led to violent street fights (Ministère du Commerce 1904)—only served to fragment construction workers further.

These changes had a significant impact on construction workers' efforts to win concessions from employers through collective protest. The most common type of workers' organization to emerge in the building trades in the 1860s was the producer's cooperative. Although they were the cornerstone of the Proudhonist socialist vision, these associations rarely contained more than a few dozen members and never became the focus of resistance to capitalist employers. In fact, one of the largest and most successful cooperatives of the period, the Fraternal Association of Masonry Workers and Stonecutters, sided with other employers during the 1865 stonecutters' strike, thereby contributing to the strike's failure.

Workers in some of the building trades did manage to establish mutual-aid societies—the organizational form that, because of its emphasis on the creation of a common assistance fund, would have been most useful for mobilizing and sustaining strikes. But even these organizations were ill-suited to provide the basis for an energetic challenge to capitalist employers in construction. At its peak, the painters' mutual-aid society included less than one-fifth of the work force. The stonecutters' society began admitting workers from other professions soon after it was founded and within a few years had completely lost its trade character (Ministère du Commerce 1904). Even more telling, the Fraternal Society of Carpenters, founded in 1857, was completely co-opted from the outset. In their initial request for government approval, the carpenters assured the authorities that they had no plans to use their society to antagonize employers by organizing strikes; indeed, one of their chief goals was to "maintain the order so often disturbed by certain men who . . . resort to violent

means and try to take work away from those who will not adopt their ways. The founders of this society have no interest either in speculation or in conspiracy: its prosperity will benefit all its members equally, and the society undertakes not to involve itself in any matter foreign to its statutes."¹²

The carpenters were true to their word. From 1857 until its demise in 1876, most of the society's officers were employers and foremen, not wage-earning carpenters. The first of its three presidents was a former carpenter turned cafe owner, and the last was a shop foreman (the occupation of the second president, who held office from 1865 to 1868, does not appear in the Interior Ministry's files). The carpenters' society remained uninvolved in strikes or other labor struggles during its 19-year existence, and the competition for members between this organization and the local chapters of *compagnonnage* prevented Parisian carpenters from mobilizing effectively against employers.

The documentary record shows that organizational fragmentation and residential dispersion among construction workers precluded the emergence of any significant challenge to employers in the Paris building trades during the 1860s. The integration of building workers into the urban environment diluted the solidarity that common regional origins had once provided; and the prevalence of subcontracting in the Paris construction industry prevented the emergence of that mainstay of artisanal social organization, the durable work team. In the absence of vibrant craft communities, workers' organizations failed to transcend their legally prescribed roles as providers of social insurance and, more important but less explicit, as instruments of social control in the service of the paternalistic imperial order.

This discussion could easily be extended to other industries. Tailors, in contrast to shoemakers or other clothing workers such as dyers or shirt-makers, still worked in small, unmechanized establishments and lived in a relatively concentrated area; they mounted the only major strike in the Parisian clothing trades between 1864 and 1871. Similarly, furniture workers (including the bronze workers mentioned above, but also cabinetmakers, chairmakers, and the like) maintained a close-knit artisanal community in the area surrounding the Faubourg St. Antoine and steadfastly resisted capitalist development through strikes, cooperative associations, and resistance societies (Ministère du Commerce 1904).

But it is not necessary to probe further into the stories of these industries to see that the indirect measures of craft social organization proposed above—residential concentration and worker-employer ratio—are closely related to the characteristics of handicraft trades that social histo-

¹² From AN, Ser. F¹² 5390, Ministère de l'Intérieur.

rians have associated with artisanal solidarity. The occupational groups that mounted significant citywide strikes in the 1860s all exhibited a combination of high residential concentration and low worker-employer ratios relative to the other trades in table 2.

Overall, then, these examples establish—as the artisanal activism thesis predicts—that the success of shop-floor protest in Paris in the 1860s depended heavily on the cohesiveness of craft communities. Craft-group organizational strength derived from two related sources: the density of intraoccupational social relations (as reflected in residential clustering) and the extent of worker control over the production process and the transmission of skills. The experience of metalworkers shows that, when capitalists were successful in using technology to attack workers' control over skills, the resulting shrinkage of training periods and increase in labor turnover eroded the craft basis of occupational communities. Similarly, the enormous growth in the building trades under the Second Empire, together with the dispersion of locales for subcontracting and the obsolescence of *compagnonnage*, made it more difficult for workers to establish long-term social relations at work. At the same time, these changes in the labor market led construction workers to settle permanently in Paris, contributing to the dissolution of provincial enclaves that had governed their residential habits in the middle of the century. Thus, ironically, construction workers became less cohesive as an occupational group even as they became more thoroughly integrated with the urban community of the capital. These transformations combined to prevent significant labor struggles in the building trades during the 1860s.

None of these findings will be surprising to proponents of the prevailing view on artisanal activism. What is surprising is the following: the very trades that can be identified as cohesive, and consequently successful in mobilizing protest, joined the ranks of the Commune in disproportionately small numbers. As the data in table 2 indicate, iron founders, furniture workers, tailors, jewelers, and others who worked in small artisanal shops and lived in tightly clustered craft communities exhibited decidedly low rates of participation in the insurrection. In contrast, the highest arrest rates and officer rates are found primarily in construction, textiles (other than tailors), and machine building—the very trades described above as weakly organized.

Inferences based on this sort of visual inspection of data are risky, above all because other factors might account for the apparently negative relationship between insurgent participation and the indirect measures of craft-group cohesion used here. For one thing, the evidence just presented on metalworking and construction showed that residential clustering and capitalist control over the organization of work were themselves related. Before attempting an explanation of the anomaly of high rates

TABLE 3
ZERO-ORDER CORRELATIONS OF OCCUPATION VARIABLES ($N = 24$)

| | Officer Rate | Residential Concentration | Worker- Employer Ratio | Native of Paris | Married | Criminal Record |
|------------------------------------|--------------|------------------------------|------------------------------|--------------------|---------|--------------------|
| Arrest rate | .596 | -.462 | .559 | -.113 | .193 | .140 |
| Officer rate | | -.151 | .616 | .323 | .212 | -.109 |
| Residential concentration | | | -.201 | .546 | .391 | -.267 |
| Worker-employer ratio | | | | .095 | .258 | -.069 |
| Native of Paris | | | | | .202 | -.565 |
| Married | | | | | | -.454 |

of insurgency among weakly organized trades, therefore, I show in the next section that this pattern holds systematically across Parisian occupations even when multivariate controls are introduced.

RESULTS

The bivariate correlations reported in table 3 reveal several striking relationships among the measures of participation, craft social organization, and other variables. Most notable is the high positive correlation between rate of participation and worker-employer ratio. Similarly, there is a strong correlation between worker-employer ratio and the percentage of each group serving as National Guard officers. This result contrasts with Traugott's (1985) finding that participation in the 1848 insurrection was not related to the ratio of workers to employers.¹³

Table 3 also reveals negative correlations between residential concentration and both measures of participation, though in this case the correlation is fairly weak for the officer rate. Consistent with table 2 data, then, it appears that occupational groups with superior organizational

¹³ Tilly and Lees (1975) report that trades characterized by large workshops were more active in the insurrection of June 1848; however, Traugott (1985) shows that this result is probably a methodological artifact attributable to the inclusion of women workers in estimates of the number of potential participants in each trade (as noted earlier, women have not been included in the estimates used here). The comparison is further complicated by the fact that Tilly and Lees use actual shop sizes, not worker-employer ratios; thus trades with large numbers of domestic workers are assigned small shop sizes in their study, even though these were precisely the trades with the most workers per employer. Since Traugott did not collect new data on insurgents in the June uprising, it is clear that the research on 1848 cannot, in its present state, be compared directly with the result on worker-employer ratios reported here.

TABLE 4
REGRESSION OF ARREST RATES ON OCCUPATION-LEVEL VARIABLES

| Variable | Parameter Estimate | β | Parameter Estimate | β |
|------------------------------------|-----------------------|---------|-----------------------|---------|
| Constant | 2.079 | . . . | 1.785 | . . . |
| Criminal record | 8.713** (3.849) | .488 | 7.360* (3.810) | .412 |
| Married | 6.850*** (2.038) | .648 | 5.325** (2.194) | .504 |
| Native of Paris | 5.073** (2.308) | .501 | 3.758 (2.380) | .371 |
| Residential concentration | -14.002*** (3.343) | -.858 | -11.315*** (3.653) | -.694 |
| Worker-employer ratio | . . . | . . . | .092 (.059) | .278 |
| Adjusted R^2 | .435 | | .475 | |
| N | 24 | | 24 | |

NOTE.—SEs are reported in parentheses.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

resources were actually less likely to participate in the insurrection of 1871 either among the rank and file or as officers. These patterns of covariation warrant the provisional conclusion that insurrection and economically motivated labor protest, such as strikes, differed not simply in scale or intensity, but also with respect to the kinds of groups that participated in them. In addition, the weak negative correlation between residential concentration and worker-employer ratio is at least consistent with my argument that residential patterns were themselves affected by the extent to which capitalist growth had eroded workers' control over skills.¹⁴

Tables 4 and 5 present multiple regression results for arrest rates and officer rates, respectively.¹⁵ Along with residential concentration and

¹⁴ Measuring change in worker-employer ratios over time would be preferable here. However, variations over time in the way establishments were classified and aggregated into trades make this task impracticable.

¹⁵ Since the dependent variables in these regressions are proportions, they were also estimated using (1) raw numbers of arrestees using population as an additional control and (2) logits. The inferences do not change across these alternative specifications; I report the proportions for greater ease of interpretation and to avoid inflating the variance explained.

TABLE 5
REGRESSION OF OFFICER RATES ON OCCUPATION-LEVEL VARIABLES

| Variable | Parameter Estimate | β | Parameter Estimate | β |
|------------------------------------|----------------------|---------|--------------------|---------|
| Constant | .015 | . . . | -.137 | . . . |
| Criminal record | 2.482 (1.490) | .397 | 1.780 (1.393) | .284 |
| Married | 1.830** (.789) | .494 | 1.038 (.802) | .280 |
| Native of Paris | 2.921*** (.894) | .823 | 2.238** (.870) | .630 |
| Residential concentration | -3.929*** (1.295) | -.687 | -2.534* (1.336) | -.443 |
| Worker-employer ratio | . . . | . . . | .048** (.022) | .412 |
| Adjusted R^2 | .311 | | .429 | |
| N | 24 | | 24 | |

NOTE.—SEs are reported in parentheses.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

worker-employer ratio, the regressions include as controls the proportion of workers in each trade with one or more past convictions, the proportion married, and the proportion born in Paris. The first two variables are necessary to control for selection bias in the arraignee sample, because army officers tended to favor married arrestees and arrestees with no criminal record in the decision about whom to hold for trial (Gould 1990). In addition, however, all three variables measure the extent to which *individuals* were embedded in sets of social relations that integrated them more or less closely with the urban community. They are included in the regression model because a given occupational group can have high or low values on these dimensions of social integration independent of any variation in the organizational capacity of the group as a whole.¹⁶ The

¹⁶ In fact, of course, these rates are simply aggregations of individual-level variables. This means that an effect of proportion married on arrest or officer rates does not reflect a causal process on the level of the occupational group, as long as one assumes—reasonably, I think—that these rates do not exert contextual effects on participation. Instead, it demonstrates the effect of being married on any individual's likelihood of participating in or leading the movement, independent of the occupational group he or she belongs to. Because the occupational groups exhibit variation in the propor-

effects on insurgency of specifically trade-level factors cannot be evaluated accurately without controlling for individual-level forms of social integration that may have affected participation.

Regression results for both arrest rates and officer rates generally confirm the impressions given by the zero-order correlations. Contrary to what the artisanal activism thesis would predict, concentration has a significant negative effect on both measures of participation, indicating that workers in residentially dispersed trades were more likely to take part in the uprising. When worker-employer ratio is included in the model without controlling concentration (results not shown here), its coefficient is positive and significant; when both are included, the effect of each is attenuated (though the effect of concentration remains statistically significant at the .01 level), which indicates that their effects on participation were intertwined. This finding supports the argument that residential clustering was itself affected by changes in labor markets and the organization of work.

Note that the positive effects of proportion married and proportion native to Paris do support the individual-level side of the artisanal activism thesis, that is, occupational groups whose members had personal ties that rooted them more firmly in the community *as individuals* were more likely to participate in collective protest, contrary to the "dangerous classes" perspective of earlier scholars (Chevalier 1958). To the extent that an occupation was integrated as a craft, then, it provided relatively few insurgents; but to the extent that its members were themselves integrated with the urban community, they were more likely to participate in the insurrection. The effect of proportion married, which is significant at the .05 level in three of the four models, is particularly noteworthy given that married arrestees were more likely to be released without trial because of leniency on the part of the military courts (Gould 1990); without this confounding factor, the effect of this variable would probably be larger.

As expected, the proportion of a trade with past criminal convictions has a positive, though relatively weak, effect on arrest rates. However, the coefficient for criminal convictions is not a significant predictor of officer rates, indicating that marginal individuals were less likely to be elected as officers by their battalions. These effects are consistent with the argument that arrestees' criminal records exerted an influence on the military court's decision to try or release a suspected insurgent. Prisoners who had been arrested in the past, most often for crimes such as va-

tion of their members who were married or who were born in Paris, however, the aggregate variables need to be included to avoid incorrect estimates of the effect of true group-level variables.

grancy, drunkenness, and petty theft, were more likely to be held for trial and are thus overrepresented in the arraignee sample; if this bias were removed, it is likely that the coefficient for crime would be negative, at least for officer rates.

Overall, the results for the two sets of regression models are similar in terms of the direction of the variables' effects, with some differences in the relative sizes of these effects. Most important, worker-employer ratio and residential concentration are strong predictors of arrest rates and officer rates, but in a surprising direction: residentially cohesive craft groups working in small shops (or in trades with limited putting-out production) were, relatively speaking, absent from the ranks of the insurrection. But, while cohesive trade groups participated at lower rates in the uprising, the positive effects of proportion married and proportion native to Paris show that insurgents were more likely to be socially integrated as individuals.

DISCUSSION

These results send a strong and, from the point of view of the orthodox perspective on artisanal activism, troubling message. If the artisanal activism thesis were correct with respect to working-class involvement in urban insurrection, we would have observed that the most active trades were those with the greatest residential cohesion and lowest ratios of workers to employers. Instead, the data demonstrate the contrary: craft-group organizational capacity, as measured through these two variables, was negatively related to participation in the 1871 insurrection.

In the words of Shorter and Tilly (1974, p. 273), the essence of the dominant view on artisanal activism is that "the motors of militancy are set in motion not by the marginal, the unintegrated and the recently arrived, but by workers who belong to firmly established networks of long standing at the core of urban industrial society." As elaborated by numerous later writers, this view identifies these "firmly established networks" as craft communities, groups of workers held together by a shared professional identity and traditions of corporate solidarity. This solidarity, along with the control over skills and the production process that artisanal workers maintained, is thought to have furnished the organizational resources that were crucial to working-class protest in 19th-century Europe.

As the experiences of foundry workers, tailors, and furniture workers demonstrate, this account is well suited to explain patterns of labor protest in Paris in the 1860s: workers in trades with the best-preserved artisanal communities were the most successful in organizing strikes and winning concessions from employers (see also Moss 1976). But the data

presented here make it clear that the same logic cannot explain militancy in the Paris Commune: insurgents were most likely to belong to trades—such as construction, machine building, and shoemaking—with weaker organizational resources and a correspondingly limited level of strike activity.

This finding does not, however, warrant a return to the idea that insurgents were recruited disproportionately from among marginal elements of the urban population (see Moss [1976] for an example of this argument). As table 1 showed, artisanal workers overall were more likely candidates for insurgent participation than unskilled day laborers, who were decidedly less integrated into the urban life of the capital. And within the artisanal category, occupational groups whose members tended to be married or were native to Paris were more heavily represented in the insurgent ranks.

What, then, explains the finding that trade-based social organization, which constituted the foundation of industrial protest, did not facilitate the mobilization of an uprising that was populated in general by socially integrated people? In the introduction, I hinted at an answer by observing that the urban insurrections that occurred in France during the 19th century differed critically from institutionalized protest insofar as they exhibited none of the customary signs of trade divisions.¹⁷ Indeed, even the Lyon uprising of 1834, which is usually referred to as an uprising of the silk weavers, was really a movement of workers in general: despite the fact that the events leading up to the insurrection involved a specific conflict between silk weavers and silk merchants, the arrested insurgents were drawn from the entire range of Lyonnais trades (Bezucha 1974; McDougall 1978). This feature of urban uprisings suggests strongly that insurgency relied on a different social-structural foundation from that which underlay industrial protest: if this had not been the case, insurgency would have been subject to the same trade cleavages that characterized strikes, mutual-aid societies, and political demonstrations.

The different social foundation in question was the neighborhood. In an earlier paper (Gould 1991), I showed that insurgent mobilization in the 1871 uprising was intimately tied to the dense system of social relations that characterized working-class neighborhoods. Urban insurrections throughout the 1800s, both in France and elsewhere in Europe,

¹⁷ To be precise, it was really only the barricade fighting that showed no traces of trade consciousness. In both 1848 and 1871, workers' political activity and social experiments such as producers' cooperatives displayed a very strong trade orientation. It would consequently be misleading to speak of the uprisings in 19th-century France as episodes in which trade consciousness suddenly vanished, only to reappear after the revolutions had been crushed. Of course, this fact only makes the irrelevance of trade boundaries to revolutionary street fighting all the more noteworthy.

were organized around the construction of barricades to seal off the popular quarters from the forces of order; thus it is not surprising that insurgent mobilization should have depended on neighborhood rather than trade solidarity. In 1871, the importance of the neighborhood, or *quartier*, was enhanced, and to some extent altered, by the dominant role of the Paris National Guard in the insurgent effort; nonetheless, since the National Guard recruited according to residential area, its organizational strength was itself dependent upon the social structure of the *quartier*. Social pressure to report for guard duty derived from the fact that one's fellow battalion members were also one's neighbors.¹⁸ Failure to participate in the insurgent effort was construed as a betrayal of loyalty to the neighborhood and was sanctioned accordingly. For instance, one of the Commune's police commissioners, in filing one of his weekly reports, recounted the following street conversation between a priest and a National Guard sergeant who had acknowledged that he had the resources to flee Paris if he wished:

"What!" said the priest, "you have twelve thousand francs in income, and you go along with these people?" [indicating the other guardsmen in the battalion]. "I can't leave," said the sergeant, "because what would my comrades from the *quartier* say?" [Quoted in Dauban (1873), p. 104]

This theme also runs through the testimony of arrestees, whose references to neighborhood pressure to join the insurgent effort occur too often to be dismissed as mere fabrication. Take the case of Alexandre Beaumont, a young shoemaker:

Q: Why did you join the guard?

A: I had run out of money. Not only that, in my building there were a lot of *fédérés* [men belonging to battalions in the National Guard Federation] who had already threatened to make me sign up.¹⁹

Similarly, Joseph Lasserre, a 49-year-old mason living in the twentieth arrondissement, claimed that he tried to stay home during the "bloody week" but could not.

On May 21, the day the Versailles army entered Paris, I was in my neighborhood. The next morning I stood guard at the barricade on the corner of the chaussée Ménilmontant and the rue Puebla, where I stayed until the 26th. Then I went home, but on the morning of the 27th I went back to the barricade, having been forced by the National Guards of the *quartier* who had come to get me at my house.²⁰

¹⁸ It is instructive to note the high arrest rate for barbers, who were inescapably tied to neighborhood life by virtue of the role their workplace played as a center of sociability. I am indebted to an anonymous reviewer for this observation.

¹⁹ Army archives, 19th *conseil de guerre*, dossier 371.

²⁰ Army archives, 7th *conseil de guerre*, dossier 440.

Whether the social sanctions encouraging participation are characterized as “solidary incentives” (McAdam 1982) or as coercion, the role of neighborhood social ties was pivotal. But this does not explain why trade solidarities did not also contribute to insurgent mobilization. After all, just because insurgency was mobilized along neighborhood lines does not mean that social cohesion within craft groups could not have provided a secondary basis for recruitment to the insurrection. The various dimensions along which social relations were patterned could simply have worked together to promote participation in collective protest.

The reason they did not work together is that the social organization of the Parisian working-class *quartier* and artisanal craft communities did not simply coexist but rather competed with each other as bases for social integration. The more cohesive a craft group was, the easier it was for its members to associate in occupation-specific bars and cafes located near their places of work, as the bronze-workers’ case shows; and this associational life facilitated industrial protest, which in turn strengthened trade identities and solidarities even further. Workers in residentially (and thus socially) dispersed trades were, in contrast, more likely to establish links to their neighborhoods in general. This pattern is best illustrated by the transformation of construction workers from a close-knit seasonal group clustered near the Place de Grève to a permanently established population scattered across a broad range of neighborhoods. Likewise, the emergence of large machine-building firms in a variety of areas outside the traditional artisanal districts—older metalworking shops were concentrated in the east of Paris, in the area known as Folie-Méricourt (Nakajima 1985), while Cail was located in a southwest neighborhood and Gouin was in the north—spread workers around the city, breaking up the social ties that constituted the craft group. In the clothing trades, a different process produced the same result: the increasing use of domestic outwork in the manufacture of shoes, shirts, dresses, and the like reduced the residential clustering of workers in these trades and, even more important, replaced social interaction in the workplace with interaction at home. In all these cases, then, erosion of trade-specific social networks opened the way for social integration with the neighborhood as such.²¹

This competition between trade and neighborhood was the crucial fac-

²¹ Note that even those industries with the highest levels of residential concentration, such as jewelry manufacture, coexisted geographically with a multitude of other crafts; no Parisian neighborhood could be identified with a single trade. In this respect, Paris and other large French cities were quite different from smaller single-industry communities, such as mining towns, where neighborhood and trade networks (and thus identities) very much overlapped. Even in Lyon, which was famous as a center of silk manufacture, silk workers accounted for only about one-third of the working population (Bezucha 1974).

tor preventing the cohesion of craft groups from serving as an additional social-structural basis for the mobilization of insurgency. Specifically professional solidarities could not contribute to a recruitment process that framed participation (Snow et al. 1986) in terms of the collective identity of neighborhood rather than trade: if two shoemakers happened to defend the same barricade, they did so as neighbors, as inhabitants of the same street or building, not as practitioners of the same craft. And, to the extent that workers in the most cohesive trades were less closely tied—socially and therefore cognitively—to the *quartier*, they were less responsive to a mobilization effort based on neighborhood loyalty. This explains why workers in close-knit crafts participated in the Commune at rates that were not only no higher, but actually lower, than the rates for other workers.

It is worth noting, finally, that the findings observed here cannot be the result of ideological opposition to revolution on the part of workers who had experienced success in implementing the cooperative socialist program. As many scholars have pointed out, the Commune represented exactly the form of government cooperative socialists wanted: a “democratic and social republic” dedicated to the emancipation of the working class through cooperation (Edwards 1971; Gaillard 1971; Rougerie 1971; Moss 1976; Serman 1986). By establishing a Ministry of Labor and Exchange (chaired by the Internationalist Leo Frankel) and authorizing workers’ organizations to convert abandoned workshops into producers’ cooperatives, the Communal Council placed itself squarely in the French socialist tradition. Indeed, many of the capital’s best organized trades, such as the tailors, expressed public support for the Commune’s policies.

On an ideological level, then, the Commune could not have been any more appealing to workers in the metal and construction trades than to leather workers, bronze workers, or tailors. It is the fact that trade-based protest and insurgency relied on distinct networks of social ties, coupled with the fact that the Parisian trades were differentially linked to these two types of networks, that explains the observed variation in levels of insurgent participation.

The observation that insurgent mobilization depended on neighborhood ties thus explains simultaneously why neighborhoods provided the social-structural foundation for urban insurrections and why trades did not. Social ties among neighbors were crucial to insurgency because barricade fighting was by its very nature framed as an expression of neighborhood solidarity and because workers in weakly knit occupational groups were more tightly integrated into neighborhood networks. The apparent class unity of the 1871 uprising was essentially an artifact of the neighborhood basis of mobilization: trade boundaries dissolved on the barricades

because insurgents fought together as neighbors, not as members of specific trades.

It would, of course, be foolish to conclude that the experience of battling the forces of repression alongside workers from a broad range of trades did not *contribute* to class consciousness for many insurgents. While trade divisions persisted in the labor movement after 1871, there is little doubt that the violence of the Commune pushed some workers to view the world in the stark terms of a struggle between capital and labor. But this should not obscure the point that class unity fails as an *explanation* for the temporary disappearance of trade boundaries during insurrections. What tied workers from different occupations together in the Commune were the tangible bonds they experienced as neighbors, not the abstract bonds of joint structural position in the capitalist mode of production.

CONCLUSION

The central lesson of this research is that the insurrection of 1871—and possibly France's other urban uprisings as well—cannot be understood in the same terms as the less intense and more economically oriented kinds of activism that characterized most of the 1800s.²² Insurrection and industrial militancy were, of course, closely related in a number of respects; above all, both were driven in large part by a cooperative socialist vision of the future, and artisanal workers as a general category predominated in both strike activity and insurgency.

At the same time, however, certain crucial differences make it necessary to reevaluate the dominant view on artisanal involvement in social protest. As the thesis of artisanal activism argues, craft-group social organization was fundamental to the mobilization of small-scale industrial protest, giving this side of the labor movement a pronounced trade character. But these same social-structural resources dampened the participation of the most solidary trades in the 1871 insurrection by limiting the degree to which workers in these trades were integrated into a distinct network of social ties—specifically, the ties that constituted Parisian neighborhoods. As a result, the rank and file of the Commune consisted

²² It seems probable that, if Traugott's (1985) and Tilly and Lees's (1975) data on 1848 were reanalyzed, using worker-employer ratio *and* residential concentration as independent variables, one would still find that trade cohesion did not contribute to insurgent participation. However, the magnitude of the negative effect of trade cohesion might be quite different, since this effect derives from the historically specific fact that industrial development in midcentury Paris set neighborhood and trade networks at odds with each other.

disproportionately of skilled workers whose trade-based organizational resources had been insufficient to mount a successful challenge to capitalist development in the workplace. In short, it is precisely because the artisanal activism thesis provides an accurate analysis of shop-floor protest that it cannot make sense of insurgent participation in 1871.

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Organizational Repertoires and Institutional Change: Women's Groups and the Transformation of U.S. Politics, 1890–1920¹

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Although social movements are often presumed to cause change, the dominant theoretical accounts lead to the opposite conclusion. To explain how challenging movements do produce institutional change, this article introduces the concept of organizational repertoires. Groups marginalized by existing political institutions have an incentive to develop alternative models of organization. These alternative models, in turn, are more likely to be adopted by other political actors to the extent that they embody familiar, but previously nonpolitical, forms of organization. This argument is illustrated with an analysis of political innovation by women's groups in the United States at the turn of the century.

Although we commonly think of social movements as agents of change, the dominant accounts of the relation of movements to politics lead to the opposite conclusion. While movements are often credited with limited substantive achievements—the passage of legislation or the defeat of a particular politician—at an organizational level we have come to expect co-optation, conservative goal transformation, and the “iron law of oligarchy,” all operating to minimize differences between a challenging social movement and existing political institutions (Jenkins 1977; Michels [1911] 1962). Even in the case of social revolution, as Tocqueville ([1856]

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1955) argued, insurgent movements may only intensify already emergent patterns of state authority. In each of these contests between established political institutions and oppositional social movements, the existing institutions endure even if the substance of policy is altered. But analytically, we are left with the question of how political institutions change. What accounts for transformations in the basic models or conventions that inform political organization and action?

Such a broad question is most easily approached in a specific context. In the decades immediately before and after the turn of the century, the institutions of American politics underwent "one of the more significant governmental transformations in American history—the emergence of meaningful regulatory and administrative policies" along with "a series of lasting changes in the nature and structure of political participation; party voting declined and interest-group politics became more important" (McCormick 1986, p. 83). These changes in the basic models of political participation came in the wake of efforts by agrarian groups and organized labor to secure greater leverage in a polity where the formal equality of white male citizens seemed increasingly irrelevant. At the same time, women mobilized to secure the vote for the one half of the adult population that was formally disenfranchised.² But the connections between changing political institutions and the wave of popular political mobilization remain unclear.

In the scholarly division of labor, these two problems have been addressed by separate literatures: the study of social movements and the history of party systems or electoral regimes. Of late, however, women's historians have questioned this partitioning of inquiry, arguing that as more "is learned of the magnitude and centrality of women's contributions in these years, the more likely it seems that understanding them will provide a basis for the comprehensive analysis of progressivism that has eluded historians until now" (DuBois 1991, pp. 162–63). In order to arrive at such an analysis, however, two theoretical assumptions that have led us to discount social movements as sources of change must be reconsidered.

First, some movement organizations may be comparatively immune to pressures to adapt to the existing institutional environment. To establish this possibility, I will identify the logics of political incorporation implicit in the classic models of political sociology—specifically those of Robert Michels and Max Weber—and then apply them to a set of social movement groups known collectively as the "woman movement" of the late

² Southern blacks were the great exception to this expansion of the polity as poll taxes and restricted registration were successfully used to remove African-American males from the electoral rolls (McAdam 1986, pp. 68–69).

19th and early 20th centuries in the United States. This movement was rooted in the antebellum proliferation of female benevolent societies and abolitionist activities. When the Civil War amendments failed to provide for their enfranchisement, women gradually regrouped around the causes of temperance and woman suffrage, while constructing an impressive network of nationwide, federated women's organizations (Scott 1991). By the 1880s, women's organizations and causes were established alongside, but largely apart from, the nation's formal political institutions. The next decades saw increasing political mobilization of women as well as a series of legislative gains that compared favorably not only with the successes of women in other nations but also with the victories of labor and agrarian insurgents in the United States (Clemens 1990a; Skocpol and Ritter 1991). The ability of women's groups to enter the political arena without being fully co-opted suggests that processes of conservative organizational transformation are conditioned by both the social identity of those organized and the character of existing political institutions.

Second, at least some of the interactions between social movements and existing political institutions must be capable of producing changes in those conventions that inform political action and organization. After presenting an alternative model of the interaction of movements and institutions, I will argue that the organizational dynamics of the American woman movement help to explain one of the most important institutional changes in U.S. political history: the shift from the 19th-century "state of courts and parties" to a political regime grounded in legislative activity and interest-group bargaining (Skowronek 1982; McCormick 1986). While internal struggles and electoral tactics were central forces in the decline of the parties and the preeminent position of electoral politics (McGerr 1986; Shefter 1983), voluntary associations played a key role in elaborating a new style of politics focused on specific issues, interests, and legislative responses. A rapidly growing literature now documents the widespread involvement of women's groups in a political project that moved from the "municipal housekeeping" of the 1890s to the development of formidable state and national lobbies during the 1910s and the 1920s. While rarely producing a pure expression of womanhood, these efforts did span lines of race, ethnicity, class, and region (Baker 1991; Frankel and Dye 1991; Muncy 1991; Scott 1991). Women's groups were not alone in this organizational innovation, but because of their marginal position with respect to electoral politics, their efforts to create an institutional alternative are particularly clear.

The central point, then, is to replace the focus on bureaucratization that characterizes work in the Michels-Weber tradition with a recognition that the social world offers multiple models of organization as well as conventions concerning who may use what models for what purposes.

Models of organization comprise both templates for arranging relationships within an organization and sets of scripts for action culturally associated with that type of organization. Thus, models may be thought of as being intermediate to abstract dimensions of organizational form (e.g., degree of hierarchy) and to examples of specific organizations. Models can refer to "organizations of that type" or to "organizations that do that type of thing."³ Mention of either an attribute or an action may invoke a shared model or form of organizing.

Women's groups, along with others, were politically successful insofar as they adapted existing nonpolitical models of organization for political purposes. Rather than adopting a single bureaucratic form, these groups made use of multiple models of organization—unions, clubs, parliaments, and corporations—each of which articulated in different ways with existing political institutions. This finding requires that the scope of the standard Michels-Weber account of social movement development be delimited by a more elaborated analysis of social organization. Drawing on current debates in organization theory, I will argue that our understanding of the relation of social movements to political change has been handicapped by the twin assumptions that the choice of organizational form is governed primarily by considerations of efficacy and that classic bureaucratic hierarchies are the most effective form for achieving political goals. The choice of organizational models may also be governed by "logics of appropriateness" (March and Olsen 1989, pp. 23–24) or institutional norms (DiMaggio and Powell 1983) and, given variations in environment, composition, and organizational goals, bureaucratic forms may well prove less effective than network arrangements, solidary groups, or other conceivable alternatives (Powell 1990). Finally, when deployed in novel ways by unfamiliar groups, even the most familiar organizational models can have unsettling consequences for political institutions.

The set of organizational models that are culturally or experientially available may be thought of as an "organizational repertoire." This concept integrates the theoretical vocabulary of organization theorists sensitive to diversity of form with the cognitive or cultural framework of

³ For the sake of terminological clarity, I have used "organizational model" to refer to the cognitive or "blueprint" definition of "organizational form." But whereas Hannan and Freeman (1986, p. 56) have argued against this approach on the grounds "that blueprints for organizations are not observable," during periods of organizational change debates over the choice of appropriate models do provide evidence of the basic templates that constitute a group's repertoire of organization. Institutional change reflects the interplay of organizational diversity at the cognitive and material levels: "When efforts to implement novel forms succeed, they can result in a blurring of the boundaries among a set of forms or in the rise of a distinctly different form" (Hannan and Freeman 1986, p. 63).

"repertoires of collective action" put forward by social movement scholars attuned to historical variation (Tilly 1978, 1986). As in social constructionist accounts, institutionalization, and by extension institutional change, is understood as the product of habitualization, the self-reproduction (or failure thereof) of a particular social pattern (Jepperson 1991). But rather than focusing on a "shared history" of interaction as the primary source of reciprocal typifications (Berger and Luckmann 1966, pp. 53–67), this analysis argues that consensus may also result as actors make use of a common, culturally available (rather than situationally constituted) repertoire of alternative models for interpreting a situation or acting in it. By deploying multiple organizational models in diverse institutional fields, social movements can be a source of institutional change even if they themselves undergo transformations of a more or less conservative nature.

In developing a model of institutional change, this argument draws on contemporary organization theory while requiring a shift in focus away from the centers of institutionalized organizational fields and toward their peripheries. Rather than attributing the disruption of organizational fields to various exogenous shocks, this account suggests that strategic political action as well as the search for collective identities produces migrations of organizational models and, potentially, the disruption of organized fields of action. The institutions of modern society are understood to be "potentially contradictory and hence make multiple logics available to individuals and organizations" (Friedland and Alford 1991, p. 232). These movements of organizational models are patterned in at least two ways: by the distribution of multiple memberships in organizational fields—the Simmelian web of group affiliations—and by the cultural logics informing the deployment of organizational repertoires. In developing a theory of organizational choice, James March pointed to the role of imitation as a component of "sensible foolishness," arguing that "in order for imitation to be normatively attractive we need a better theory of who should be imitated" (1979, pp. 75–76). Earlier generations of political actors found just such a theory embedded in their repertoires of organization, the cultural understandings linking organizational models to actors and purposes.

THE WOMAN MOVEMENT: SCOPE AND SOURCES

The potential of social movements or voluntary associations to transform institutional politics is evident in a striking—although, until recently, underappreciated—case of political organization by a comparatively disadvantaged group. The American "woman movement" of the late 19th and early 20th centuries drew together women who were relatively privi-

leged in terms of economic standing and education (Blair 1980; Sklar 1985), yet suffered from formal and informal exclusionary practices that limited their ability to cultivate political skills or to exercise those skills if they were somehow acquired. Notwithstanding their formal disenfranchisement in much of the nation (the Nineteenth Amendment was not ratified until 1920), middle-class and upper-middle-class women constructed an impressive array of voluntary associations that were a significant force in the public life of the nation. Eighteen years after its founding in 1874, the Women's Christian Temperance Union (WCTU) numbered 150,000 members (Bordin 1981, pp. 3–4) and exerted influence on legislation ranging from temperance to woman suffrage. The General Federation of Women's Clubs (GFWC) was founded in 1890 and had perhaps 500,000 members by 1905 and over 1 million by the end of the decade.⁴ In addition to these groups, associated charities, civic clubs, auxiliaries to fraternal orders, and suffrage associations filled out a dense network of women's organizations. As a key element of the era's social reform constituency, these groups contributed to the founding of America's distinctively "maternalist" welfare state, a policy regime emphasizing programs such as mothers' pensions rather than unemployment and old age insurance (Gordon 1990; Skocpol 1992).

At an institutional level, women's groups were central to a broader reworking of the organizational framework of American politics: the decline of competitive political parties and electoral mass mobilization followed by the emergence of a governing system centered on administration, regulation, lobbying, and legislative politics. This change involved the invention of new models of political participation outside the established parties and the articulation of interests and demands that could be addressed by legislation and the active intervention of state agencies. Although the invention of modern interest-group politics may not have been intended by women activists, it was one of the most important consequences of this period of experimentation with political organization.

Although women's organizations of the period realized that some form of political action would be needed to advance many of their causes, politics as usual was out of the question. In addition to their formal exclusion from electoral activity, women's associations joined in a broader cultural attack on political methods. According to Mrs. Croly, the first president of the GFWC: "If I were to state what seems to me to be the great hindrance to club life and growth, it would be the employ-

⁴ The calculation of exact membership figures is impossible since totals were typically given by clubs and some women held multiple memberships (Wilson 1979, pp. 100–101; Wood 1912, pp. 131, 154).

ment of political methods, of political machinery and wire-pulling to bring about results. Politics can never be purified until its methods are changed, while its introduction into our club life subverts the whole intention and aims of club organization" (Croly 1898, p. 128).

Politics itself was not rejected, only the existing forms of political organization, the models of the electoral party and patronage machines. To construct an alternative, women's groups drew on models of organization that were culturally or experientially available in other areas of social life. Borrowing from this broader repertoire of social organization, these groups helped transform the repertoire of political action in the Progressive Era.

No formal listing of such repertoires exists, but the range of culturally available models of organization can be reconstructed from the debates that groups conducted over what sort of organization they wanted to be. Convention proceedings are a rich source of this information, since any change in organization or strategy usually entailed formal motions complete with statements of the facts (the "whereas" clause) and an argument for some alternative model for group action. For example, factional divisions might be explained in terms drawn from the worlds of business and political reform. In 1911, one Wisconsin suffrage group proclaimed its intention to "bust the suffrage trust" (the national suffrage associations) and to found an alternative organization "with a commission form of government" (*Milwaukee Evening Wisconsin*, October 10, 1911, Wisconsin Woman Suffrage Scrapbooks).⁵ Business methods also defined new political strategies. Discouraging the use of public debates—a centerpiece of 19th-century political life—one California suffragist argued in 1913 that "I think we must frankly acknowledge that people are not all convinced through reason, and that although the proposition that women should vote is seriously and profoundly true, it will, at first, be established with this class of people much as the virtues of a breakfast food are established—by affirmation" (California Equal Suffrage League of Northern California [hereafter CESLNC] 1913, p. 11).⁶

⁵ The Wisconsin Woman Suffrage Scrapbooks include clippings (primarily from Wisconsin newspapers) as well as souvenirs, telegrams, and other documents related to the campaign for suffrage in the state. Most of this material is from 1900 to 1920. Citations are given to the original newspaper source. The scrapbooks are in the collections of the Wisconsin Historical Society, Madison, Wisconsin.

⁶ Although the influence of business on politics has been a central point of contention within the historiography of the Progressive Era, these arguments have typically taken an instrumentalist form: bureaucratic models are introduced by professionals (Hofstadter 1955; Wiebe 1968) or corporate elites (Berkowitz and McQuaid 1980; Kolko 1963; Weinstein 1963) to serve their own interests. What has gone unnoticed is the extent to which models of organization associated with business shaped the

To reconstruct the repertoires of organization employed during this period, this study draws on an extensive reading of the proceedings of women's organizations. The material in this analysis is drawn primarily from a comparative study of political organization and strategies of labor, agrarian, and women's groups in three progressive states (California, Washington, and Wisconsin) between 1890 and 1920 (Clemens 1990a). Because this analysis is concerned with innovation in political organization, these cases were selected from those states recognized for innovation with respect to political procedure and for their substantive social policies. While a number of eastern states did pass progressive social legislation, on the whole they were significantly slower to adopt procedural reforms such as the initiative, referendum, and recall that undermined the power of party organizations to control political agendas and outcomes (Phelps 1914, p. xlv; Shefter 1983). Struggles within national organizations also frequently took the form of a regional split between an eastern leadership committed to established methods and midwestern or western factions more open to direct political action or state intervention. In addition to the convention proceedings and official publications of various state federations, I draw upon organizational debates as they were reported and analyzed in women's papers and in the histories published by and about different associations (e.g., CESLNC 1913; Croly 1898; Gibson 1927; Park 1960; Ruddy 1906; Simpson 1909, 1915; Spencer, n.d.; Williamson 1925; Winter 1925; *Womans [sic] Parliament of Southern California* 1892; Wood 1912).⁷ Throughout these accounts, the extent of awareness and mimicry among groups is clear. Rather than asserting that political innovation was grounded in the distinctive characteristics of individual states, this argument takes the process of organizational imitation and innovation as central to an understanding of the institutional changes of the period.

Focusing on repertoires of organization, the analysis seeks to establish

strategies of those most opposed to corporate power even prior to the decline of patronage politics. As one California Granger argued: "It stands us in hand, as Patrons, to study well the lesson which these great corporations that dominate the country are teaching, not only the farmer, but mankind, viz. That in unity, in intelligent cooperation, there is strength, there is power; but in divided effort, there is weakness, there is disintegration. . . . We must do as the corporations are doing—meet combinations of capital and brains with like combinations" (California State Grange 1889, p. 43).

⁷ Among the women's papers are the *Wisconsin Citizen* (monthly, 1887–1900, Wisconsin Historical Society, Madison), *Club Life* (published monthly in San Francisco, 1902–6, Library of Congress), the *Western Woman Voter* (Seattle, 1911–13, available on microfilm at the Bancroft Library, University of California, Berkeley), and the women's page of the *Wisconsin Equity News* (biweekly, 1911–17, Wisconsin Historical Society and University of California, Berkeley).

mechanisms for such changes by locating the interaction of social movements and politics within a broader social system that embraces alternative models of organization and multiple institutions that may promote organizational conformity or isomorphism (DiMaggio and Powell 1983). While applications of the neo-institutionalist model that examine a single focal institution (usually the state or the professions) tend to emphasize how isomorphic processes promote stability and homogeneity, the opposite outcome is also possible. When familiar organizational forms are deployed in unfamiliar ways, insurgent groups may well destabilize existing institutions and ultimately contribute to the institutionalization of new conventions for political action. Organizational heterogeneity is reflected in the repertoires of organization; this is an account of cultural "tool kits" whose potential to create change flows from the complex organization of modern societies rather than from "unsettled" times (Swidler 1986). Rather than rejecting the classic Michels-Weber model, I will use the case of the woman movement to identify scope conditions for that model and, by extension, to provide criteria for identifying the types of movements most likely to contribute to significant institutional change in the political arena.

THE IRON LAW RECONSIDERED

Political organization can have profoundly ironic effects. As Robert Michels argued in his classic study of the Social Democratic Party in Wilhelmine Germany, "Organization is the weapon of the weak in the struggle with the strong." But this leverage is gained at a high price: "Organization is . . . the source from which the conservative currents flow over the plain of democracy, occasioning there disastrous floods and rendering the plain unrecognizable" ([1911] 1962, pp. 61–62). Hierarchical bureaucratic organization is necessary to compete effectively in the formal political arena, yet the processes of competition and organization distance the leadership from the interests of their followers and from the organization's initial commitment to the transformation of the political system.

Although the inevitability of such conservative transformations has been challenged within the literature on social movements, immunity is associated with forms of insulation—economic independence, membership exclusivity, and ideological or professional purity (Jenkins 1977; Selznick 1960, pp. 23, 77, 153; Zald and Ash 1966). Furthermore, these studies have tended to focus on the internal dynamics of voluntary organizations rather than on their interactions with political institutions. For Michels, however, these may well be the exceptions that prove the rule. In his original analysis, ongoing interaction of radical political organizations with the institutions of formal politics and the economic environ-

ment was the primary engine of conservative transformation. Thus, we are left with a paradox, one that has been central to critiques of political pluralism (McAdam 1982, pp. 5–6, 18–19). To the extent that organized groups committed to political change seek to secure change through political processes—the give-and-take of coalition building and electoral mobilization—they seem doomed to fail as their goals of social change are sacrificed to the constraints of political process.

In this article, my central claim is that turn-of-the-century women's organizations—along with other associations—did cause substantial changes within American politics. Therefore, my first task must be to explain why women's organizations were comparatively immune from the logics of conservative transformation described in Michels's analysis. In *Union Democracy* (1956), the famous deviant case analysis of the International Typographical Union, Lipset, Trow, and Coleman argued that the "internal politics" of the union (the organization of work, the strength of locals, and a distinctive history of political conflict) contributed to its relative immunity from Michels's iron law. To understand the transformative potential of the women's groups of the Progressive Era, however, three aspects of external politics must be examined. The first two concern the relation of an organizational model to broader social structures. Michels's argument builds on two distinctive logics of incorporation—economic and political—that assume an organizational membership of enfranchised heads of households. Consequently, the effects of organization may vary with the social identity of those who are organized. The third point involves the symbolic rather than instrumental aspects of organizational models; the appropriation of an established model by marginal groups may have consequences that are less than entirely conservative. Given the economic and political situations of their members, women's groups were less likely to be drawn into established forms of political organization. Insofar as they did adopt these established models, however, heightened contradiction rather than effective co-optation was often the result.

The Logic of Economic Incorporation

One of the rules of investigative journalism is to follow the money. This rule also plays an important part in political sociology, receiving its classical form in Weber's analysis of the routinization of charismatic authority. The "administrative staff" of a movement or organization have: "an interest in continuing it in such a way that both from an ideal and a material point of view, their own position is put on a stable everyday basis. This means, above all, making it possible to participate in normal family relationships or at least to enjoy a secure social position in place

of the kind of discipleship which is cut off from ordinary worldly connections, notably in the family and in economic relationships" (Weber 1978, p. 246).

This familiar passage identifies two distinct mechanisms by which economic imperatives shape the relation of the administrative staff to the organization: the interest in the continuation of the organization itself and the assurance of their own economic situation "making it possible to participate in normal family relations." Both of these imperatives play a prominent role in Michels's account of the iron law of oligarchy. The administrative staff will be unwilling to challenge the leadership from within: "Financial dependence upon the party, that is to say upon the leaders who represent the majority, enshackles the organization as with iron chains" (Michels 1962, pp. 140, 138). The status of party officials as breadwinners for their families also pushes the organization's goals in a conservative direction. For that fraction of the bourgeoisie who cast their lot with the working class, "no backward path is open. They are enchained by their own past. They have a family, and this family must be fed" (Michels 1962, p. 208). Family responsibilities and social position produce similar effects on leaders recruited from among the workers, who will be reluctant to abandon their improved status as officials of a labor organization (1962, p. 259). This account is generalizable, therefore, only to the extent that alternative forms of organizational support (e.g., personal or institutional patronage, see Jenkins [1977]) are unavailable and that staff or members are responsible for their own economic well-being and that of their dependents.

These dynamics continue to figure prominently in contemporary discussions of social movements, although a conservative outcome is no longer assumed to be inevitable. Analyses of funding stress the dangers of co-optation posed by a reliance on outside funding or patronage, while noting the limitations of resources within various communities marginalized by established political institutions (Jenkins 1977; McAdam, McCarthy, and Zald 1988). The constraints of everyday life have also been recognized in less narrowly economic terms as "biographical availability" (McAdam 1986); career and family responsibilities may constrain one's ability or inclination to participate in politics.

As I will argue below, the impact of these resource constraints is mediated by the form of organization. The more obvious point, however, is that the Michels-Weber account overlooks the gendered division of labor within households. Many of the leading figures in the woman movement were subject to a different set of constraints. At the most general level, changes in the division of labor and the growth of a market for consumer commodities left middle-class and upper-middle-class women of the late 19th century with increasing amounts of time free from household respon-

sibilities (Wood 1912, pp. 24–25). Among those most active in the women's associations, many were supported financially by their husbands, and others came from families of considerable wealth and education (Blair 1980; Sklar 1985). The imperatives of the breadwinner, so central to both Weber and Michels, were clearly less relevant for these groups. For the less privileged, the women's groups themselves might provide a community—such as the settlement houses (Deegan 1988, pp. 40–45)—or activist careers might be funded by either broad constituencies or prominent patrons. The fact that many educated women remained single or childless—along with many mothers active in these associations having the help of a household staff—lessened the constraints of biographical availability.

The Logic of Political Incorporation

Just as the logic of economic incorporation is mediated by the forms of social organization and family life, the logic of political incorporation is structured by specific institutions. In Michels's terms, political participation leads to the conservative transformation of oppositional organizations by way of the system of electoral competition and the identification of movement leaders with the political establishment. Foreshadowing Anthony Downs's analysis of party systems (1957), Michels argued that by competing in elections, oppositional parties would be drawn toward the political center and, thereby, moderate their radical goals (1962, pp. 334–35). But if electoral competition does not produce favorable outcomes, even formal political parties may adopt alternative organizational models and strategies that emphasize ideological purity or solidary incentives rather than electoral advantage (Kitschelt 1989, p. 41). Furthermore, sheer numbers are less closely linked to victory in other systems of political contestation. For a revolutionary party, the requirements of training cadres may well outweigh the advantages of a large membership. For lobbying groups, as for all those engaged in "symbolic politics," resources, status, and style may matter more than numbers.

Women's formal disenfranchisement obviously distanced them from the logic of electoral incorporation. Although their groups frequently sought to persuade male voters to support woman suffrage or temperance or some other cause—and often moderated the radicalism of these claims for strategic ends—these efforts were only loosely coupled to the internal life of most women's associations. Barred from efforts to mobilize members directly as a voting bloc—with the consequent need to acquire majority, if not unanimous, consent to the organization's goals—women's associations typically developed internal "departmental" structures that allowed individual members to focus their efforts on a variety of goals,

from visiting the sick to agitating for social legislation.⁸ Combined with a political mission summed up in the WCTU motto "Do Everything!" the departmental structure allowed factions or local organizations to experiment in advance of any consensus by the national membership. The organizational arrangements of the woman movement often freed it from the conservative consequences of consensual decision making (Baker 1991, pp. 20–21); when the Woman's Joint Congressional Committee was established in 1920, "procedure held that whenever three of the WJCC's organizations voted to support or oppose a piece of legislation, they formed a subcommittee to do their lobbying" (Muncy 1991, p. 103). Such systems of "loose coupling" (Thompson 1967) provided an additional source of insulation, buffering the internal life of the organization from both environmental constraints that might favor centralized bureaucratic forms directed toward discrete instrumental goals and from the veto of conservative factions within a given group.

The Instrumental Claim for Bureaucratic Organization

The claim for the efficacy of hierarchical bureaucratic organization was the first substantive argument made by Michels (1962) and remains widely accepted, even among his critics:

It is indisputable that the oligarchical and bureaucratic tendency of party organization is a matter of technical and practical necessity. It is the inevitable product of the very principle of organization. Not even the most radical wing of the various socialist parties raises any objection to this retrogressive evolution, the contention being that democracy is only a form of organization and that where it ceases to be possible to harmonize democracy with organization, it is better to abandon the former than the latter. Organization, since it is the only means of attaining the ends of socialism, is considered to comprise within itself the revolutionary content of the party, and this essential content must never be sacrificed for the sake of form. [P. 72]

While it is possible to imagine circumstances in which oppositional groups will be immune from the economic and political logics of conservative incorporation, this claim is the bedrock of Michels's assertion

⁸ The unfamiliarity of women's groups with "politics as usual" played a role in the split between the National American Woman Suffrage Association and the more radical Congressional Union. During the 1916 election, the Congressional Union rejected traditional strategies of persuading male voters in order to mobilize women in suffrage states to use their votes to "punish the party in power," a model of organized action developed by British suffragettes in the context of a parliamentary system (Van Voris 1987, pp. 120, 131). While much of the dissension centered on issues of nonpartisanship and bipartisanship, it was equally true that strategies of mobilizing their members as voters were unfamiliar to many women's organizations.

that "who says organization says oligarchy" and, by extension, that organization entails both internal and external conservative transformations. In its most simple form, the argument runs as follows: (1) hierarchical, centralized bureaucracies are the most effective form of organization; (2) consequently, existing political parties and institutions have adopted this form of organization; (3) in the course of pursuing their ends, oppositional parties will adopt the same organizational form for strategic reasons, even at the expense of their ideological commitments; (4) therefore, growing organizational isomorphism will lead oppositional parties to become like established political groups, precluding the possibility of meaningful political change.

This argument may be challenged on at least two points. I will discuss the first, the assumption that there is a single form of bureaucratic hierarchy toward which all organizations evolve in the pursuit of efficacy, in the context of an alternative model to be developed below. The second, however, concerns the assumption that the adoption of existing organizational forms by an oppositional group necessarily has a conservative or moderating influence on its critical stance toward existing political institutions and endangers "internal" democracy. Since politics is structured by intersecting rules—how to organize, who should organize, and for what—it is possible that the adoption of conventional organizational models may be destabilizing as it exposes contradictions within the existing system.

Many early efforts by politically active women accepted both the institutionalized models of American politics and the traditional cultural division of the social world into "separate spheres" for men and women. Yet this borrowing revealed incompatibilities between the organizational systems of politics and gender. Attempts by women to use recognizably political methods eventually necessitated a denial of the dual system of separate spheres. Initially, women's political institutions were largely self-directed. In 1848 the Women's Rights Convention at Seneca Falls issued a formal "Declaration of Principles" and Elizabeth Cady Stanton declared that "woman herself must do this work; for woman alone can understand the height, the depth, the length, the breadth of her degradation" (quoted in Flexner 1970, p. 77). By the second half of the century, women's organizations were promoting "Women's Parliaments" as forums in which women could debate and publicize their views. Through the rest of the 19th century, women drew on this model of separate politics parallel to the separate spheres. In 1892, for example, the Southern California Womans Parliament met for "the full and free discussion of reforms necessary to the progress of women's work in the church, home and society" (1892, p. 1). Women's work was redefined in public terms rather than traditional religious terms, but retained its identity as

a distinct feminine realm. Yet by the end of the century, the appeal of this form of political organization had been undermined, in part by its own success. By establishing a common ground for public involvement and domestic ideals, the parliaments helped to make the idea of political involvement by women more widely acceptable. But because the parliaments were so easily linked with the highly contentious issue of woman suffrage, women frequently faced less opposition when they pursued their goals through organizational models that were not derived directly from the existing political system.

But what would such organizational models be like? For women's associations, and for the suffrage organizations in particular, the immediate dilemma was that any instrumental advantages of hierarchical, centralized bureaucratic form counted against one of the central goals of these groups: the demonstration that women were capable of being independent citizens rather than subject to undue direction by priests, husbands, or other authorities.⁹ Consequently, it was not enough to replace hierarchical organizations headed by men with bureaucratic voluntary associations led by elite women. From the level of local parlor meetings to national conventions, attention was paid to the form—if not always the substance—of participatory organization and proper procedure.¹⁰ In San Francisco, the Richelieu club was “mostly composed of the presidents of other clubs. . . . The club's aim is to carry on the drill of parliamentary usage with a view of having a more accurate knowledge of one's rights upon the floor and one's duty in the chair of an assembly” (*Club Life*, October 1902, p. 3). Women's papers regularly featured drills on parliamentary procedure. Although some organizations were dominated by a single national figure (such as the WCTU under Frances Willard), the GFWC regularly elected new presidents and, in almost all cases, the federated structure and complex departmental divisions of

⁹ This was a particular concern in giving the vote to women in communities of recent immigrants. Suffragists and voters of Anglo-Saxon stock were unsure whether these groups would vote in the “true interests” of women and children or be swayed to oppose temperance and other social reforms: “Is it probable that the wage slaves of our great department stores, our shirt-waist and clothing manufacturing establishments and other industries employing large numbers of women and votable girls at almost starvation wages would *risk the loss of their job and the mean pittance it returns to them* by voting against the wishes of their employers?” (*Wisconsin Equity News* vol. 4, no. 18 [January 25, 1912]; emphasis in original).

¹⁰ Even the decision to organize or not could carry symbolic weight. The National Woman's Rights Convention of 1852 had defeated a proposal to form a national association, rejecting any type of formal organization in the same language used by radical abolitionists and trance-speaking spiritualists: secular authority was an affront to the self-sovereignty of the individual and his or her commitment to higher authorities (Braude 1989, pp. 162–63; Perry 1973, p. 114).

these associations provided for considerable opportunity and flexible participation within the woman movement. Although an organizational chart of any of the major women's groups would possess many traits of a classical bureaucracy, individual careers rarely involved a regular progression through a hierarchy of offices, suggesting that these groups accommodated the shifting familial and economic obligations of activists (Clemens and Ledger 1992). While embracing the internal specialization of the modern corporation, women were much more ambivalent about establishing a clear hierarchy of authority. Although studies of interest-group politics and social movements have both viewed organization in terms of its contribution to instrumental goals of policy change and legislation, the advantages of centralization and specialization may be tempered by the meanings attributed to organizational models and the differing ability of organizational forms to sustain involvement.

This brief overview of women's organizations does, however, establish the premises upon which an alternative account of political organization and institutional change must be fashioned. The first point is that the choice of organizational models is not governed solely by instrumental considerations. Cultures have rules about who should organize in what way and for what purposes; consequently, the choice of a conventional model by an unconventional group may produce neither the efficacy nor the conservative transformations suggested by Michels's analysis. Second, complex societies present many possible models of organization—multiple combinations of hierarchy, centralization, authority, and exchange—which may be simultaneously “legitimate” and incompatible. Americans of the late 19th century were familiar with both the centralized hierarchical forms of the patronage party and the modern corporation, yet the period's politics are often portrayed as a conflict between these two models of organizing public life. To the extent that a challenging group is immune to the processes of incorporation discussed above, both its potential to cause institutional change and the character of that change will reflect the range of alternative models available in its members' repertoires of organization.

REPERTOIRES OF ORGANIZATION: AN ALTERNATIVE APPROACH

For much of political sociology, organizations matter as resources; they make coordinated action possible and success more likely. The role of organizations in blocking action has also been demonstrated; prior commitments and established networks can make new patterns of mobilization difficult (Connell and Voss 1990). But organization has consequences beyond the process of mobilization itself. As a group organizes in a particular way, adopts a specific model of organization, it signals its identity

both to its own members and to others. Models of organization are part of the cultural tool kit of any society and serve expressive or communicative as well as instrumental functions. In addition, the adoption of a particular organizational form influences the ties that an organized group forms with other organizations. The chosen model of collective action shapes alliances with other groups and relations with political institutions. At both cultural and institutional levels, models of organization and collective activity are central mechanisms in the transformation of political systems. Once organizational form is viewed as being simultaneously a statement of identity and constitutive of broader institutional fields, social movements appear as not only vehicles of preexisting interests and causes of specific political outcomes, but as critical sources of institutional change.

In order to make sense of such change, the language of cultural analysis is helpful. If a society's cultural heritage constitutes a set of "models of" and "models for" action (Geertz 1973), an organizational repertoire, the appropriation of each of these possible models is not equally probable. Instead, certain models are privileged by the existing distributions of power, status, and wealth as well as by established institutional arrangements. At the most basic level, there are certain advantages to familiarity: "Even government officials and industrial managers of our own time generally behave as though they preferred demonstrations and strikes to utterly unconventional forms of collective action" (Tilly 1986, p. 391). Organization theorists have emphasized this process, exploring the ways in which conformity to institutional rules produces increasing homogeneity within organizational fields (DiMaggio and Powell 1983; Meyer and Rowan 1977). But if one recognizes an established *repertoire* of acceptable forms instead of a single institutional rule, processes of institutional isomorphism can also promote change within a social system.

Consider the following possibilities. A group finds itself in a situation where the established models of organization and paths of action are inadequate. This may occur for many reasons: existing models may be tactically or culturally discredited (as wire-pulling and other "methods of politics" were for Mrs. Croly); existing models may be unavailable or off-limits (as electoral politics were for most American women); or no established models of action may be associated with that situation (e.g., methods of holding legislators accountable for specific votes prior to the development of preelection pledges and legislative roll-call reporting). Confronted with analogous situations, organization and decision theorists have developed accounts of search procedures. These accounts typically establish some relation—either purposive or stochastic—between an environment that provides multiple potential "solutions" and some selection criteria or procedures (e.g., Heclo 1974; Kingdon 1984; March and

Olsen 1989; Meyer and Rowan 1977). Once selected, the choice of a particular solution or organizational model then has consequences for both the environment and the system of relations among organizations. Certain choices may produce organizations that make sufficient demands on their environment to induce new organizations to form (Westney 1987). At the same time, the choice of a model may draw an organization closer to some groups while weakening other interorganizational ties. For example, teachers may choose to affiliate with an occupational group based on the model of the labor union or one modeled on the professions. This difference, in turn, sets up distinctive alliance patterns with other organizations such as the AFL-CIO or the American Medical Association (Hess 1990).

Applied to the study of political change, this account highlights the significance of the available set of organizational models and the process of selection. The repertoire of organization both reflects and helps to shape patterns of social organization. As it is mastered by any given individual or group, a repertoire is largely constituted through experience or awareness of existing forms of social organization. We know what it is to be part of a committee or a commune or a platoon because we have participated in, observed, or heard of these different forms of organization.¹¹ Similarly, we know what different organizational models signify with respect to the expectations and behaviors of members as well as the collective identity presented to others. Thus, the initial use of familiar forms by unfamiliar groups will have a destabilizing effect on existing conventions of organization. For example, the clubwomen, now quaint and moderate figures, named themselves in violation of established feminine conventions. While the term "club" was rejected by some as a "masculine" label, more daring groups such as the New England Women's Club "deliberately chose *club* to indicate a break with tradition; it did not want to be associated with good-works societies" (Martin 1987, p. 63; emphasis in source; see also Ruddy 1906, p. 24). For outsiders, organizational form was a signal of these groups' novel qualities and aims. "What is the object?" was the first question asked of any

¹¹ In this sense, the concept of a repertoire of organization synthesizes the cognitive or cultural "tool kit" model of recent work in social movement theory (Tilly 1978, 1986) with current institutional accounts in organizational theory. As distinct from the new institutionalism of economics and political science, with their concerns for transactions costs and rule-governed decision procedures (Williamson 1985; Shepsle and Weingast 1987), in the context of sociology and organization theory, "institutions" are those conventions that "take on a rule-like status in social thought and action" (Meyer and Rowan 1977; DiMaggio and Powell 1991). Insofar as the concept of organizational repertoire implies multiple sets of conventions, however, this "rule-like" status is necessarily contextual and subject to change.

organization of women, and if it was not the making of garments, or the collection of funds for a church, or philanthropic purpose, it was considered unworthy of attention, or injurious doubts were thrown upon its motives" (Croly 1898, p. 9).

By distancing themselves from religious associations and charitable societies, women's clubs constituted themselves as "absolutely a new thing under the sun" (Wood 1912, p. 188). And, in defining itself through the appropriation of organizational models not traditionally associated with female groups, the women's club movement is a clear example of innovation grounded in the materials at hand. This process of organizational change through the rearrangement of existing repertoires characterized the woman movement as a whole.

Once one group has pioneered the use of an organizational model in a new arena, that model may then be adopted for use by other groups. Although the rationale for adoption may flow from momentary strategic advantages, widespread adoption is a source of fundamental change in the organizing categories of the political system. Returning to the specific case of political change in the United States, I argue that the shift from the electoral regime of highly competitive parties to the legislative and administrative focus of interest-group bargaining can be understood by examining the organizational experiments of groups that were comparatively disadvantaged under the first of these regimes. The subsequent shift in the available repertoire of organization—the recognized set of political options—then gave way to a system in which this form of mobilization became part of the taken-for-granted. Writing in 1907, John R. Commons, a prominent economist and social reformer, observed that "there is no movement of the past twenty years more quiet nor more potent than the organization of private interests. No other country in the world presents so interesting a spectacle" ([1907] 1967, p. 359). With respect to the conventions of political action and organization, this new system entailed a focus on legislative rather than electoral politics and a consequent organization on the basis of stakes in particular issues rather than broad political philosophies. As one commentator complained by the 1920s:

The present unionized era of leagues, societies, alliances, clubs, combines and cliques offers confederation for mutual support of almost any interest conceivable except for the diversified interests of the humble in the application of general law. With united front the bankers, the brokers, the dairymen, the detectives, the sportsmen, the motorists, the innkeepers, the barbers, the mintgrowers, the Swiss bell ringers, *et al.*, may and do present their complaints to the legislature for adjustment. [Wisner 1928, p. 172]

Although manufacturers organized nonpartisan trade associations in both the United States and Europe, only in the United States did "inter-

est"—rather than party, class, language, or religion—become the primary idiom of political life, a *legitimate* if not necessarily welcome form of political organization (see also Clemens 1990a; Maier 1981; McCormick 1986; Reddy 1987; Rodgers 1987). The making of specific claims on legislatures was not in itself new, but previously took the form of petitions, private bills directed at individuals, or the considerable bribery of the Gilded Age (Thompson 1985). What was new was the exertion of issue-specific pressure through political education, public opinion, expert testimony, and the increasingly sophisticated legislative tactics of issue- or constituency-based organizations.

This new system of political organization grew out of an eclectic process of reorganization. Rather than accepting a single model for political action, groups drew on both traditional models and the most modern good government groups as well as imitating what worked for their frequent opponents, the corporations and political machines. By the 1890s, for example, temperance associations and women's groups had set up their own precinct organizations and departmental organizations to exert focused pressure on specific government institutions (Bordin 1981; Kerr 1980). In California, the Women's Christian Temperance Union "exerted an influence out of proportion to its size, because of its strong church support, its unique ability to cooperate easily with all other temperance organizations, and because, unlike the fraternal societies, it devoted its energies almost entirely to agitation and reform work. On the model of the national W.C.T.U. the state organization set up many departments, thirty in all, each concentrating on a separate phase of temperance work" (Ostrander 1957, p. 58).

Recognizing the need to create a political counterweight and a vehicle for shaping public opinion, the liquor industry responded by appropriating an organizational model then unfamiliar in business politics: "The liquor dealers formed an organization which, to outward appearances at least, was based on an idea new to California. They formed what was publicized as a liquor-men's temperance association. Following the example of many temperance organizations before them, the liquor dealers patterned themselves after Masonic design. The new organization was called the Knights of the Royal Arch, and in April, 1902, the Grand Lodge of the Knights of the Royal Arch was formed in San Francisco" (Ostrander 1957, p. 100).

Complete with exotically titled officials, and, no doubt, secret handshakes, this innovation by the liquor interests appears retrograde, a move away from the modern politics of interest based on public opinion and expertise rather than fraternal solidarity. But to conceive of institutional change as the product of novel applications of existing organizational repertoires is not to claim that all such applications will be effective. If

California's liquor dealers engaged in a retrospective form of organizational borrowing, temperance advocates and especially women's groups helped to transform the meaning of corporate political models such as the lobby. In the process, a model that had been associated with corrupt practices was now transformed and legitimated as a taken-for-granted component of political action.

This account of changes in the forms of political organization generates a series of propositions quite different from those associated with the Michels-Weber model. *First*, rather than identifying a unilinear trend toward hierarchical, bureaucratic forms, this alternative account suggests that we should expect to find a lot of cultural work around the questions of What kind of group are we? and What do groups like us do? The links between organizational and cultural analysis are clear; models of organization are not only conventions for coordinating action but also statements of what it means for certain people to organize in certain ways for certain purposes (Kanter 1972). *Second*, we should expect that both the substance of these debates and the subsequent patterns of mobilization should vary by the set of organizational models that are culturally and experientially available to a given group at a particular point in time. *Third*, patterns of organization in response to novel or ambiguous situations should be shaped by a group's existing or desired ties to other groups committed to a particular model of organization. The selection of a specific organizational form should then strengthen ties between some organizations while weakening others.

In the next section of the article I will explore each of these propositions, using examples from the turn-of-the-century woman movement. I am not claiming that women's groups were unique in adopting new models of organization (indeed my argument suggests that available models of organization are likely to be noticed and used by multiple groups), only that women's groups were particularly well placed—both constrained by a lack of viable alternatives and relatively immune to the logics of incorporation discussed above—to explore the potential of organizational models newly introduced to the political arena.

GENDER AND POLITICAL ORGANIZATION

Over the course of the 19th century, the role of gender in defining political identity intensified. In the French "Declaration of the Rights of Man and Citizen" of 1791 and in the Fourteenth Amendment to the Constitution of the United States, voting was defined as the exclusive prerogative of adult men (Catt and Shuler 1926, pp. 32–45; Landes 1988, p. 122). In the United States, these formal exclusions were reinforced by a dense web of political association. To the extent that political associations were

based on workplace identities, the differing patterns of men's and women's involvement in the labor market ensured that these associations would be primarily single-sex groups. Similarly, insofar as turn-of-the-century political mobilization built on the fraternal organizations of 19th-century America, it perpetuated that period's distinctively male and female political cultures (Baker 1984; Clawson 1985). While organizations such as the Good Templars and the Patrons of Husbandry (also known as the Grange) did embrace the moral issues of concern to women and at times allowed women to join, in all cases the political organization of men at the end of the 19th century powerfully influenced the participation of women.

But women's politics were not entirely derivative. Opening her massive overview of the women's club movement in the United States, "Jennie June" Croly proclaimed "when the history of the nineteenth century comes to be written, women will appear as organizers, and leaders of great organized movements among their own sex for the first time in the history of the world" (1898, p. 1). In part, women's models of political action derived from their history of public participation, the revivalism of the Jacksonian era was echoed in the fervent calls for reform made by the Women's Christian Temperance Union (Epstein 1981; Smith-Rosenberg 1985, pp. 129-64) and social reformers drew on a legacy of friendly visiting and personal service to the poor (McCarthy 1982, pp. 3-24). Middle- and upper-class women, in particular, adopted the models of the parlor meeting and charitable society, gradually adapting them as vehicles for a greater role in public affairs. Without directly challenging the fundamentally fraternal character of political life, women drew on domestic and religious models of action to begin to craft a public role for themselves. But the long-standing political exclusion of women meant that their activism would be particularly disruptive to the political order. The puzzle, then, is to explain how women's groups were able to transform their public identity in a way that largely sidestepped the culturally embedded equation of the political with masculinity.

In this enterprise, women activists drew on an organizational repertoire familiar to them as members of American society if not necessarily through their direct experience in women's groups: "The woman's club was not an echo; it was not the mere banding together for a social and economic purpose, like the clubs of men. It became at once, without deliberate intention or concerted action, a light-giving and seed-sowing center of purely altruistic and democratic activity" (Croly 1898, p. 13). Having appropriated a "male" model of organization, the clubwomen continued to transform their movement through organizational imitation. One of Mrs. Croly's successors as president of the General Federation of Women's Clubs explicitly celebrated the innovation of the movement,

while acknowledging its basis in imitation. Her "little book," *The Business of Being a Clubwoman*, "does not deal with purposes or programs, but with *ways* of running our affairs. We must learn to avoid our old mistakes and gain our ends by more direct paths. We can learn out of our own past. And no one but ourselves can give us much help. Colleges and social scientists and experts of various kinds can help us in the *matters* upon which we are working, but as to the *ways* of working we have to blaze our own trail" (Winter 1925, p. vi; emphases in original).

The need to invent new "ways of working" was a product of the organizational repertoire that these economically privileged women inherited from the 19th century combined with their double exclusion from both male organizations and partisan politics. Faced with these constraints, the clubwomen and their social peers were pushed to innovate.

While club life was the form of many women's initial entry into public affairs, three other models of organization were central to the development of a more explicitly political strategy for women. First, women increasingly drew on corporate forms and cash exchanges to replace personal service as the preferred medium for social action. In addition, women's groups internalized the bureaucratic forms and methods of the modern state as they turned with greater frequency to state and federal governments, rather than to their own communities, as the appropriate arenas for political action (Baker 1991). The final development involved the positive cultural revaluation of the model of the lobby. If these three organizational shifts are taken to an extreme, one arrives at a picture of the modern-day interest or issue group, with its use of educational literature and expert testimony to secure federal funding for some sort of program. As timeless as this form of political action may now seem, it was an invention of the Progressive Era, and women's groups played a central part in its elaboration and legitimation.

Organization as Business

All of these efforts at organizing women and their political activities took place during a period now widely recognized as an organizational revolution. Consequently, rather than confining the analysis to the relations between women's groups and politics, we must consider the influence of sweeping changes in the broader repertoire of social organization in 19th-century America. Of particular importance was the expansion of the market and the development of new forms of commercial relations (Chandler 1977). While the hierarchical relations of the corporation had mixed implications for women's self-presentation as independent citizens, the market appeared as unambiguously modern.

For women's groups, however, the adoption of "business methods"

had consequences that were not apparent in the activities of men already immersed in a system of market relations. Part of becoming a modern organization involved substituting cash exchanges for personal service. Speaking on "Woman's Work in the Church" to the 1892 Southern California Womans Parliament, the Rev. Lila F. Sprague declared that "the woman of to-day is inaugurating an epoch of belief; a belief that it is better in every way, for all concerned, to give five dollars in cash to the needs of the church, rather than ten dollars in poor cake, and poorer pie, which may, with a big crowd and plenty of hard work, yield a net return of one or two dollars" (as quoted in Womans Parliament of Southern California 1892, p. 8).

Throughout the Progressive Era, the conflict between traditional and activist women's groups continued to be expressed in terms of the role of money. The call for cash, however, had to confront the fact that many women had only limited access to funds of their own. Consequently, a reliance on the domestic production of baked goods and bazaar items continued alongside an effort to extract funds on the basis of a more "modern" female identity, that of the consumer. Mainstream suffrage associations sponsored "consumer fasts" in which women promised to forgo cosmetics and other luxuries for a week or so, sending the money saved to fund the fight for the vote. More radical organizations tried to sever the link between fund-raising and women's traditional roles as either charity workers or consumers. In Wisconsin, the leader of the Political Equality League declared "that she will not conduct sales of cookbooks or postcards to raise a campaign fund and further declares that if it is to be that sort of a campaign she will seek a cool spot near Lake Superior and retire there. Promises, Miss Wagner points out, do not pay the bills and begging for money is humiliating" (*Racine News*, July 11, 1911, Wisconsin Woman Suffrage Scrapbooks).

The shift to business methods characterized the delivery of services as well as their underwriting. Whereas personal contact between charitable women and their poor clientele had once been viewed as central to the project of moral uplift, this "friendly visiting" was increasingly under attack. From the perspective of scientific charity, this form of aid was inefficient; from the perspective of the disadvantaged and their advocates, it was frequently demeaning. In response to these complaints, volunteers in numerous cities sponsored Women's Industrial Exchanges where working women could sell homemade items, baked goods, and needlework, while charities joined together as "Charity Organization Societies" seeking to coordinate both fund-raising and the delivery of services. Indeed, part of the mission of the new scientific charities was to move completely beyond a reliance on volunteers.

The convergence of these trends toward cash support and paid sup-

porters is seen in a novel program established by the Associated Charities of San Francisco, an organization in which women held considerable power. In the wake of the 1906 earthquake, the Associated Charities was the main conduit for both federal aid and private relief funds raised across the country and, consequently, developed the habit of operating on a cash basis. Later, in response to the situation of destitute women and children, these women established what one report described as "the most important sociological innovation ever made in San Francisco."¹² Continuing an earlier effort to deinstitutionalize the state's orphans, the Associated Charities simply boarded out babies for \$12.50 a month. Like the later mothers' pensions, this program provided women with the money to support their own children. Unlike the mothers' pensions, however, this was money that the women earned by entering into an explicit employment relationship with a public agency. For the charity volunteers of San Francisco, the potential of moral suasion or social control rapidly played next to the demonstrated power of spending.¹³

Spending was also a sign of full political citizenship. Recounting their contributions to the San Francisco Panama-Pacific Exposition of 1915, the Woman's Board explained that:

Woman's co-operation in other world expositions has necessarily included an accounting of funds drawn from official sources. That is not the case with the Woman's Board which helped in the creation of San Francisco's Dream City of 1915 and in bringing it to a picturesque and notable obligation of stewardship; it financed all its own undertakings as well as those undertakings which it cheerfully assumed at the request of the Exposition directorate. [Simpson 1915, p. ix]

The importance of this claim is underlined by its presence in the second paragraph of the preface, the first substantive claim in a book-length account. The political significance of financial autonomy is explained in the paragraph that follows:

There are in mind the men and women everywhere who may be interested in these achievements not merely for their intrinsic worth, but also for the

¹² Anna Pratt Simpson, "The Story of Associated Charities since the Fire of 1906." These articles originally appeared in the *San Francisco Call* in 1909 (Bancroft Library, University of California, Berkeley).

¹³ The influence of this model of employment is highlighted by a comparison with the organizational consequences of relief efforts after the Chicago Fire of 1871. Whereas the San Francisco Associated Charities established contractual relations with its clients who were thereby empowered as self-supporting workers, the industrialist-dominated Chicago Relief Association used its authority over the disbursement of relief funds to control the charity organizations of the city through a philanthropic version of the trust, demanding representation on boards in return for a share of the donations (McCarthy 1982).

reason that they bear eloquent witness to the success of a great human cause, for the reason that they are, in some sort, the first fruits of woman's emancipation in a state newly made politically free, a practical thank-offering of woman's pride and woman's patriotism. [Simpson 1915, pp. ix-x]

By adopting "business practices," these activists diminished the role of distinctively gendered organizational forms in constituting their public identity. This adoption of business practices and cash exchanges simultaneously undermined the forms of intimacy, solidarity, and community traditionally associated with women's groups of the 19th century. In promoting the organization of working women, for example, elite clubs no longer sought to maintain the personal, albeit supervisory, ties of friendly visiting. In San Francisco, the prestigious California Club first sponsored the working-class Porteous Club and then left it to support itself. Once again, business practices signaled civic maturity:

That the little club is capable of managing its own affairs in a small way is sufficiently evidenced by the concert it gave in the early part of the year, when it was practically, though not yet nominally, on its own resources. . . . None the less was the affair organized and carried through in all business details by the Porteous members themselves, and so well did they manage that they netted \$108.04 as their profit from the entertainment. After such a result none can doubt the business capacity of the baby Porteous. [*Club Life*, May 1902, p. 4]

Such shifts toward business practices were in tension with the familial models of sisterhood and mother-daughter relations that had shaped both the organization and self-image of women's organizations of the 19th century (DuBois 1991). But if these changes made it possible for women's organizations to take public actions not directly linked to the domestic, the questions of how such actions could be made politically effective remained.

From Community to Bureaucracy

While the organization of activity around cash exchanges signaled personal dignity and political maturity, this move away from personal service as a primary public activity also had consequences for the ability of women to enter into politics as usual and to stimulate the expansion of state agencies. To the extent that their activities were constructed around a cash nexus rather than around personal service, experimental private programs could be adopted more easily by state agencies, once sufficient public support had been generated. Without directly confronting the gendered opposition of home and electoral politics, the oblique embrace of

business forms of organization resulted in an increasing isomorphism between women's associations and state agencies.

Yet if the reliance on cash facilitated such transfers, it was not their sole cause. Charitable programs for men had experienced much the same development; indeed, the reliance on work-based systems centered on employment exchanges or coalyards was much more usual for impoverished men. But there was a danger in distributing public moneys to poor men, a danger beyond the threat that dependency posed to their moral character. Any such distribution might be easily turned to political purposes, to partisan advantage. Yet the same was not true for impoverished women. Unable to vote themselves, and with their lack of attachment to adult men as the very condition of their eligibility for aid, widows and unmarried women could be the targets of public aid without the funding of those programs being attacked as politically corrupt.¹⁴ Given their relative immunity from the logic of political incorporation, the disenfranchised were culturally privileged as recipients of public largesse.

The distinctive position of women with respect to the state reflected the dynamics of organizational isomorphism along two dimensions. With respect to the organizational field of electoral politics, women's formal disenfranchisement insulated their associations from electoral incorporation, the onslaught of "predatory politics" that had undermined both agrarian and labor movements. But by adopting business practices as models of organization, women's groups could then accommodate themselves to the generic bureaucratic procedures common to both corporations and state agencies.

These agencies were central to the new politics of interest groups and social programs. In many cases, women's groups played an important role in the establishment of these institutions. In the United States, many women demanded the vote not as a natural right but in order to secure specific reforms—child-labor laws, temperance, and protective legislation for working women. Frequently, women's groups not only demanded state intervention, but initiated it by providing funds for kindergartens, probation officers, and health inspectors and other services later provided by public agencies (Gibson 1927, pp. 214, 216; Williamson 1925, p. 40). Arguing that working women were without the protection of union contracts—due to their incapacity for forming contracts, the nature of the labor market, or the neglect of union organizers—women's labor reform groups called for state intervention to control both hours

¹⁴ This characterization of aid recipients was, however, often merely a convenient construction since aid to indigent women could be used as a way of securing the electoral support of male relatives who might otherwise be called upon for financial aid.

and wages for women, actions that often drew criticism from organized labor. These demands for economic regulation and social services mean that women's politics are of particular importance for understanding both the beginnings of the American welfare state and the entrenchment of interest-group politics.

The creation of state agencies also had important implications for the future of women's politics in the United States. For just as the business transformations of women's organizations allowed for a measure of political success in the decades surrounding the turn of the century, these changes also undermined the strength of these organizations. By shifting to cash transactions, away from personal service and the creation of community, these organizational innovations eroded the personal networks and commitments that so often account for the success of a social movement.¹⁵ The political effects of adopting business practices worked in opposite directions. The implementation of this model first served as a path by which women could sidestep the clearly gendered forms of the 19th-century polity. But once established in the polity, women's groups adopted increasingly hierarchical models of professionalism and expertise that ultimately made the widespread political mobilization of women less relevant for the determination of policy outcomes (Muncy 1991). Although elite women reformers were much more likely to attend to and rely on their constituencies than were male bureaucrats, more and more often a few well-placed experts and political insiders could accomplish as much as mass rallies and petition drives.

In this respect, Michels's prediction of the emerging gap between leaders and members was fulfilled.¹⁶ Insofar as women's groups had both created and captured state agencies, a smaller set of reformers and activists could successfully promote a political agenda that had once required a mass movement. For example, in carrying out its infant health and birth-registration campaigns, the newly established Children's Bureau

¹⁵ Prior to the recruitment of a cadre of wealthy supporters, for example, "the suffragette style drew on the militant traditions of the labor movement, and its protest tactics, such as outdoor rallies, were suitable to a constituency with little money." As money replaced effort as the medium of mobilization, suffragists were increasingly divided by class and pushed toward passive roles as donors and audiences: "Meetings went inside once there was money to hire halls" (DuBois 1991, pp. 169, 173).

¹⁶ This distance did not necessarily translate into a conservative goal transformation because of the distinctive organizational context in which the leaders found themselves. During the 1920s, the leadership of the woman movement—particularly that of the suffragists—embraced the international peace movement and, consequently, endured attacks and red-baiting from conservative women's groups and politicians (Van Voris 1987, pp. 189–97).

relied on the cooperation of 1,500 clubwomen by 1914 and on 11 million by the national Children's Year of 1918 (Ladd-Taylor 1991, p. 117; Muncy 1991). But the leaders of agencies were no longer leaders of movements and, as the woman movement was absorbed by parties and new voluntary associations during the 1920s, the new women bureaucrats were left vulnerable to conservative attacks and the dismantling of programs and agencies. But, in the meantime, women's groups had helped to legitimate models of extraparty politics.

Politics without Parties: The Art of Lobbying

Because American women initially pursued their political goals without the benefit of the vote, they developed methods of influence distinct from the electoral context of partisan politics. One opening was found in lobbying, the unsavory practice of the Gilded Age, that Thompson (1985) described as "a constitutionally guaranteed right of all citizens . . . that nonetheless has no respectability unless it masquerades under euphemistic aliases." Unlike the vote, the right of petition was available to all—"minors, minorities, aliens, women, even idiots have always been able to employ it" (Thompson 1985, p. 27; see also Herring [1929] 1967, p. 36).

In constructing their own "euphemistic alias," one which would distinguish them from aliens, idiots, and others with no claim on citizenship, women combined the tainted model of the lobby with educational strategies more conventionally associated with 19th-century women's organizations. As one clubwoman (Wood 1912, p. 26) observed, "the earliest form of the woman's club was the study club, the 'Middle-aged Woman's University.'" But women did not immediately recognize education as a strategy of political influence. As of the First Biennial of the General Federation of Women's Clubs in 1892: "The educating of public opinion as the only permanent basis for welfare work seems not at the time to have become a part of the inner consciousness of the average General Federation worker" (Wood 1912, p. 50). Over the next decade, the links between women's politics and "educational strategies" were firmly secured. The woman suffrage movement embraced the model of education as a form for both internal mobilization, through a "Course of Study" on political science, and for external cultivation of sympathizers among the male electorate (Van Voris 1987, pp. 43–44). Other women's groups linked education to social policy demands: "The method employed by the Consumers' League to better conditions invariably followed this rule: obtain facts through investigation, acquaint the public with the facts, and after educating public opinion, secure legislation" (Nathan 1926, p.

78).¹⁷ This last step, the merging of educational and legislative strategies in a new model of political organization, proved most difficult.

The politics of education and public opinion ran into difficulty when women attempted to translate research and expertise into political influence. Having crafted a political role from a strategy associated with corporate bribes and illicit interests, women were vulnerable since this political role could be easily attacked. During the debate over the woman's eight-hour bill in California, Maud Younger, a San Francisco union organizer, encountered this dilemma:

The rule against lobbying while the houses were in session was strictly enforced. Miss Younger's efforts in behalf of the Eight-Hour Bill had earned her the name of lobbyist. Having business with Senator Caminetti one forenoon, she ventured on the floor of the Senate while that body was in session. Her presence was observed and objection made to lobbyists being permitted on the floor.

When Caminetti understood that Miss Younger was the "lobbyist" referred to, he became furious even for him. . . .

"This young woman is assisting me in my work. She is not a lobbyist, she is acting as my clerk. She will not leave my desk."

Still the objection was made. [Hichborn 1911, pp. 246-47]

The rule against lobbyists on the floor had been passed only two years earlier by antimachine progressives, yet was frequently used by machine politicians against reformers (Hichborn 1909, pp. 219-25). Then, as now, the line between legitimate and illegitimate representation of interests was unclear.¹⁸

Despite these dangers, lobbying was one of the few models of political influence available to women, and they worked to legitimate this model of activity by linking it to the conventions of professionalism and expertise. Prior to securing the right to vote, for example, members of the California State Federation of Women's Clubs replaced a loose Legislative Committee composed of six district representatives with one headed by a more powerful Legislative Chairman. While the initial results were

¹⁷ The political appropriation of education had been pioneered by the major parties themselves in an effort to cultivate the small but decisive group of independent voters that developed in the late 19th century (McGerr 1986). Within the woman suffrage movement, the political appropriation of education also resulted in demands for an educational requirement for the vote.

¹⁸ Franklin Hichborn, a journalist closely allied with the progressive forces in the state, noted this difficulty in his account of the last machine-dominated California legislature: "The problem of drawing the line between legitimate and reprehensible lobbying has perplexed wiser men than sat in the California Legislature of 1909." Hichborn himself accepted the propriety of lobbying when one had a "legitimate interest" in a bill, but the criteria for legitimacy remain unclear (Hichborn 1909, pp. 226, 228).

somewhat disappointing, within two years the clubwomen claimed some credit for the passage of the eight-hour law for women, the employers' liability law, and bills pertaining to child labor. By 1912, following the suffrage victory:

The California Federation of Women's Clubs, through its Chairman of Legislation, invites all State organizations of women to cooperate with it in forming a central committee, a State Legislative Council of Women. . . . The purpose of this body will be to prevent duplication of this work and expense; to bring together experts from each society who can plan mutually for better work than would be possible alone; to decide how much legislation and what is wise to ask for, and to see that this is being prepared by responsible organizations; also to select a small committee to be in Sacramento during the session of the Legislature to look after all interests involved in such legislation.

One reform-minded journalist proclaimed that the California women had invented "the Scientific Management of Club Influence" (Gibson 1927, pp. 181, 185, 188).

The women's associations of California may have been somewhat precocious in establishing a formal lobby, but they were not alone. In Washington State, women's groups affiliated with the nonpartisan Joint Legislative Committee and eagerly joined in campaigns for labor legislation as well as women's issues more narrowly defined, forging important political alliances and developing considerable political skills (Tripp 1973, pp. 85–86). Even the women's groups of Wisconsin, constrained as they were by nativism, dry sentiments, and a general cultural conservatism, were willing to venture into this new field of endeavor. The Milwaukee Consumer's League gave its support to women's hours legislation, although the primary advocates of these bills were the city's Social Democratic legislators (Schmidt 1933, pp. 187–88). The State Federation of Women's Clubs passed resolutions favoring mothers' pensions and child-labor laws, sent officers to testify on bills, and created a Legislative Committee whose members "are finding the work decidedly educational, and are acquiring a knowledge of the methods used to pass or defeat bills never dreamed of in the philosophy of women's clubs" (Wisconsin Federation of Women's Clubs 1909, p. 83).

Having mastered these methods, women lobbyists effectively supported a wide range of legislation that has secured the United States a reputation as a "maternalist" welfare state (Skocpol and Ritter 1991; Skocpol 1992). Tensions between the models for feminine and political identity lingered; women found that their choice of political techniques continued to draw comment. As Maud Wood Park of the National American Woman Suffrage Association wrote: "The Front Door Lobby was the half-humorous, half-kindly name given to our Congressional Com-

mittee in Washington by one of the press-gallery men there, because, as he explained, we never used backstairs methods" (Park 1960, p. 1). Once women had secured bases of power within government, they sought to implement programs that would serve their constituencies but were increasingly constrained by their obligations to carry out programs promoted by other political coalitions. The women who led the Children's Bureau, for example, were torn between an infant health program favored by working women and the enforcement of child-labor laws that threatened the tenuous family economies of these same women: "Female reformers in government functioned both as advocates for poor mothers and as administrators of the (sometimes injurious) policies that affected them, and their contradictory role made conflict with grass-roots mothers inevitable" (Ladd-Taylor 1991, p. 123). These organizational accomplishments won a begrudging acceptance for politically active women, but they also transformed the relation between activists and the members of their self-adopted constituency.

Organizational Repertoires and Cross-Class Alliances

While these changes may have helped to undermine the solidarity of the woman movement—once rooted in the relatively intimate networks of clubs and parlors—they also made it possible to negotiate other class-based divisions among women. So long as privileged women drew on those models of organization closest to their own experience, their differences with working-class women and men were emphasized. But as their repertoire of organization expanded, so did the possibilities of cross-class alliances among women. The significance of changing organizational models is suggested by a comparison of the political development of privileged women with the experience of working-class women. Similarly excluded from formal political participation, working-class women confronted this situation with a different repertoire of organization. Although women rarely had equal standing within the labor movement, within two years of dropping its commitment to secrecy in 1878, the Knights of Labor did authorize the initiation of women (Delzell 1919, p. 10; Foner 1979, pp. 186–87; Levine 1983, pp. 324–25).¹⁹ In 1882, at the second convention of the Federation of Organized Trades and Labor Unions (the

¹⁹ The reasons that women could not join the Knights earlier were rooted in cultural beliefs about the feminine character. The founder of the Knights, "though far in advance of many members of the early Knights, was so obsessed with the value of secrecy and with the sexist view that women could not keep secrets that, while he favored the inclusion of all male workers and mentioned women, he did not advocate opening membership to women" (Foner 1979, p. 185). The model of organization thus identified those who could and should be organized.

forerunner of the AFL), women were invited to participate. Thus, like the Patrons of Husbandry, these male-dominated organizations did not formally exclude women in the manner of the multiplying fraternal societies of the time (Clawson 1989, pp. 180–87). But unlike the Patrons of Husbandry, who often found themselves claiming “some earnest Grangers and good workers, especially the sisters, who seem to take more interest in the grange than the brothers” (California State Grange 1887, p. 110), the large 19th-century labor unions remained clearly the province of men. Women rarely accounted for more than 10% of the membership in the Knights of Labor and a smaller proportion yet in the AFL (Foner 1979, p. 188; Levine 1983, p. 325).

While this degree of openness compares favorably with that encountered by middle- and upper-middle-class women who sought to work alongside their “fathers, husbands and brothers,” labor organizations were not free from more general cultural beliefs that women were not as amenable to organization as men. Reflecting on recent state-level suffrage victories, one Socialist organizer asserted: “Many have contended that the work of propaganda among women requires essentially different methods than those used among workingmen. So it was said shortly ago that the work among agrarian populations must be carried on differently than for the town proletariat. This has been proven erroneous, and the same principles have been found to apply in both cases” (Simons 1912? p. 6).

Insofar as the labor movement was concerned, however, the conditions of employment of most working women made it difficult for them to organize effectively within the framework provided by the AFL. Although skilled women had joined craft unions in the late 19th century (Eaves 1910, pp. 314–15; Matthews 1913, pp. 40–50), by the turn of the century, the fastest growing group of women workers were unskilled operatives working in industries such as textiles, garment manufacturing, and electrical goods. In these occupations, men and women alike proved difficult to organize; for women, this situation was aggravated by the AFL's repeated failure to hire women organizers (Dye 1980, pp. 13, 80). But even these failures helped to reinforce the primacy of class rather than gender as the organizational basis of public identity for working women.

The tensions between models of political organization based on class solidarity and gender became evident in the policies of the Women's Trade Union League (WTUL), a cross-class alliance of wealthy, educated reformers and women activists who had made their way up through union movements and strike organizing. For the first decade of its existence, the WTUL adhered to the definition of working women as workers, overriding the organizational experience that its elite sponsors

brought from years of participation in the woman movement. Since the labor movement was technically open to women, an approach that emphasized craft-based organizing protected the sponsors from class-based attacks on their motives and the working women from charges of dual unionism. Dependent on the AFL for both financial support and legitimation as a labor organization, the WTUL was constrained from pursuing industrial models of labor organization or taking a more explicitly political approach that might aggravate the tensions between the AFL's official bipartisan stance and the independent electoral efforts of the socialist wing of the labor movement. Given these struggles over organizational models and strategies within the dominant labor federation, a craft union strategy seemed preferable to more "feminine" organizational forms that often resembled the political strategies of the Socialists. "In its first years . . . the league did not explore alternatives for women's unions. In the early twentieth century, the AFL represented the only model for successful unionism, despite its severe limitations for female industrial workers. For the league, it was *the* labor movement, and the WTUL saw no alternative but to carry on its organizing campaigns within the AFL's framework" (Dye 1980, p. 87).

With the victory of craft unionism and the hegemony of the AFL only recently secured, any women's experiments in union form would inevitably challenge their most powerful allies. Only after years of disappointments and declining financial support from the AFL did the WTUL begin to draw on the models of political action developed within the middle-class bastions of the woman movement. Legislative campaigns began to replace union organizing and industrial strikes.

This shift in strategy depended upon the ongoing evolution of organization and strategy among middle- and upper-middle-class women. During the same years that the women's club movement began to grow rapidly among the middle class, women philanthropists had sponsored "Working Girls Clubs" offering classes and lectures (Montgomery 1987, p. 146).²⁰ Although the clubs enjoyed considerable popularity in the Northeast, these attempts at cross-class alliances relied on the public but prepolitical models of the women's club and were often a source of tension with working-class women hostile to any patronizing, however well-intended. This hostility was echoed by the men of the labor movement. After certain "ladies of a philanthropic and religious turn of mind" established a

²⁰ A decade or more later, the WTUL appealed to immigrant women by setting up clubs parallel to the men's fraternal benefit clubs, arguing that unionization could grow out of social solidarity. But, reflecting the ambiguous role of women in the work force, these clubs were caught in a controversy over whether to distribute marriage benefits or strike benefits (Dye 1980, pp. 112-13).

"Girls' Union" in San Francisco in the late 1880s, the *Coast Seamen's Journal* complained that "while we believe they mean well, their mode of procedure is not such as will emancipate our sisters from the slavery and socially degrading position which they at present are placed in" (quoted in Matthews 1913, p. 5). Club life did not appear to offer a path to significant economic improvement.

With the increasing turn of the women's club movement toward legislative activity, however, new parallels emerged between working-class and middle-class women's movements. Like their middle-class counterparts, working women risked censure when they ventured into the male world of politics. Surveying women's union activities in San Francisco, Lillian Matthews (1913) noted that:

The waitresses as a body and individually exhibit considerable more interest in municipal politics than do the women of other trade unions. This gives rise to many rumors that the waitresses include within their membership women who serve, from time to time, at least, in the type of cafe and resort which is always a factor in the darker side of municipal corruption. It is reported from numerous sources also that politicians of a certain class make use of the favor of waitresses because the publicity of their work throws them in contact with people whom they wish to influence. All this is mere rumor, however. But, whatever the reasons and whatever conclusions it may suggest, it is undoubtedly true that the waitresses mix into municipal politics, and that during some administrations they have received marked favors in the way of municipal positions. [P. 81]

In a city governed by the allegedly corrupt Union Labor party, participation in politics tarnished the reputation of the unionized waitresses at the same time that their activities suggested a further corruption of politics, a proletarian version of the "cunning spider-lobbyist" of the Gilded Age and her illicit mixing of the domestic and the political (Herring 1967, p. 36).²¹ But if the waitresses were censured for their alleged appropriation of the clearly masculine model of patronage politics, other models of political action were culturally available. Faced with defeat in organizing

²¹ The reputation of the waitresses' union was also attacked because the organization flaunted standards of feminine propriety in order to secure the financial basis of a solid union: "The waitresses raise most of their funds for relief and sick benefits from their large annual ball. This ball provokes considerable disapproval. One of the main features is a bar, and from the sale of drinks the receipts are \$600 to \$800. The sum thus derived goes into the benevolent fund." With these ill-gotten gains, the waitresses provided death benefits, supported a paid staff which was "not customary in other unions," established a minimum wage scale, and "molded together a class of workers who are notably hard to weld." Despite all these accomplishments, the culturally questionable methods of the waitresses caused "the personnel of the Waitresses' union [to receive] more criticism than is accorded to the women in the other unions" (Matthews 1913, pp. 78-81).

workers, both the Knights of Labor and the AFL had sometimes turned to lobbying or legislative strategies (Montgomery 1987, pp. 164–69) and, at least potentially, this path was also open to working women and their more privileged allies.

This shift in strategy is evident in the organizational development of the Women's Trade Union League. Between 1913 and 1915, the New York League switched its resources from supporting strikes to working for the passage of legislation and, above all, a woman suffrage bill: "The league's commitments to suffrage and protective legislation suggest that members had begun to cast their lot with women's organizations and feminist issues rather than with the male-dominated labor movement. . . . WTUL women also increasingly viewed their difficulties with organized labor as a fundamental conflict between men and women rather than as a conflict between workers and a predominantly upper-class organization". (Dye 1980, p. 123).

This shift required the organizational experiences and expertise that middle-class women had acquired over two decades of involvement, first in the club movement and charitable associations, then in the politics of suffrage and social reform. As the contrasting fates of middle-class and working-class women's organizations demonstrate, the relationship between gender and politics is mediated by organizational form and capacity. For working-class women, the comparative openness of male unions to their participation lessened the incentive for organizational innovation at the same time that the hardships of their lives limited the time and resources available for independent organizing. For middle-class women, by comparison, the sharp ideological delineation between the separate spheres pushed them to invent new organizations.

The organizational developments within the far-flung woman movement support each of the three propositions set out above. Rather than displaying a regular evolution toward more bureaucratic forms of organization in order to promote efficacy, women's groups were constantly engaged in debates over the meaning of different organizational forms. Even when business models were adopted, this was often not out of instrumental concerns but as an effort to demonstrate the status of the membership as autonomous, rational citizens eligible for an equal place in the American polity. Second, the organizational repertoires at stake varied across both time and social position. While an early generation of middle- and upper-middle-class women used the club to distance themselves from traditional female models of association, later generations contrasted the solidary and still distinctively gendered woman's study club with organizational models and practices (departments, cash exchanges, professionalism, and expertise) appropriated from business and bureaucracy. Over the same period, working-class women were fre-

quently represented in—or at least by—work-based organizations promoting identities based on employment or economic sector (e.g., the Knights of Labor and the Patrons of Husbandry).

This class-related divergence of organizational repertoires created conditions in which the third proposition may be demonstrated: patterns of organization in response to novel or ambiguous situations should be shaped by a group's existing or desired ties to other groups committed to a particular model of organization. Faced with the task of creating organizations that would include working women, elite female reformers had to choose between work-based and gender-based models of organization. For so long as they were allied with and financially dependent on the AFL, the WTUL pursued organizationally conservative forms of workplace organizing. As this relationship became strained, the WTUL reconstituted itself around a more explicitly political model, strengthening its ties to other major women's associations. Through these efforts, the distinctively gendered models of organization dominant in the 19th century disappeared, only to give way to a political system in which the bureaucratically organized representation of women by women (experts and activists, usually privileged) emerged as the leading edge of social policy expansion in the United States. But even though the reorganization of gender and political identity was incomplete, these changes had far-reaching effects on the institutions of American politics.

ORGANIZATIONAL REPERTOIRES AND INSTITUTIONAL CHANGE

Through ongoing processes of organizational innovation—the constant search for political advantage or shared identity by trying something new, adopting some alternative model of organization—women's groups helped to create a new system of political institutions. In the place of a political system in which voting had been the central act and identity was grounded in the solidary networks of community and workplace, the beginning of the 20th century saw the rise of a political regime in which groups claiming to represent categories of persons presented specific demands to legislatures, using the leverage of public opinion, lobbyists, and expertise rather than sheer numbers of votes. Although scholars may well differ over the conservative nature of these developments and their normative status, these changes stand as an important example of profound institutional transformations stemming from regular—not revolutionary—political processes.

To understand how such change occurs, it is necessary to abandon the assumptions of unilinear development and institutional homogeneity that have dominated theorizing about the relation of social movements to political institutions. Instead, recent developments in organization theory

and social movement studies point to the importance of a multiplicity of organizational models, a repertoire of organization.²² This variety forces qualification of the classic models of the relation between movements and political institutions. First, different models of movement organization (and differences in the identities of those organized) mean that some movements may be more susceptible to the logics of incorporation that characterize a specific political regime. Second, in order to circumvent the disadvantages imposed by a specific regime, movement groups may import models of organization that are already culturally legitimate although not previously recognized as political. By using models of organization that are simultaneously familiar and novel, social movement groups may bring about changes in the taken-for-granted rules about what political organization is and what it is for.

In the United States, the loosely knit "woman movement" of the turn of the century provides an important example of this type of institutional change. Although the women's parliaments and parties did demonstrate the isomorphism with existing political institutions predicted by the classic model of Michels, the majority of organizational activity by women's groups involved a much more eclectic process of copying and transforming multiple models of organization: "Colleges and social scientists and experts of various kinds can help us in the *matters* upon which we are working, but as to the *ways* of working we have to blaze our own trail" (Winter 1925, p. vi; emphases in original). Limited by their exclusion from the organization and practices of electoral politics, women's groups were particularly motivated to discover or invent new channels for their political activities. By drawing upon available alternative models of organization—business methods, state bureaucracy, and lobbies, along with models drawn from education and the professions—women's groups helped to pioneer a distinctively nonelectoral style of social politics. The success of their experiments was such that this new style was quickly appropriated by other political actors and the historical origins of this model of political organization have been forgotten as the system of interest-group bargaining is taken as natural, indeed as constitutive of American politics (Moe 1980, p. 2).

Women's groups were a source of political change because they were marginal to the existing electoral system, but not so marginal that they were ignored by other political actors. Together with the assumption of organizational heterogeneity—the assumption that a *repertoire* of organi-

²² A similar assumption is found in much neo-Marxist work on modern society that focuses on the contradictions between institutions held to have different "logics" (e.g., Alford and Friedland 1985; Block 1977; O'Connor 1973).

ization exists—the presence of differences in political power is fundamental to this account of institutional change. The potential of a challenging group to produce changes in existing institutions is a joint product of the incentives to innovate produced by relative marginality *and* its visibility within the political arena, as well as the acceptability of those innovations to other political actors. Framed in these terms, this process of institutional change is a recurring element in the political history of this country. The civil rights movement, for example, was grounded in both the decreasing political marginality and increasing organizational innovation of blacks in the United States. The Great Migration out of the South increased black voting strength just as the shift of blacks to the Democratic party contributed to their leverage over the administration. Profiting from this growing visibility, activists synthesized models of organization grounded in religion with the strategies of nonviolent resistance and court-centered contestation to create a style of oppositional politics now shared by both the Left and the New Right (McAdam 1982). Similarly, the antiwar movement of the 1960s sought to exploit new relations with the mass media and, thereby, helped to usher in a political system in which a central role is played by the access of challenging groups to television coverage and the ability of established elites to control the terms of that coverage (Gitlin 1980). Nor is this dynamic limited to the United States. Describing the rise of ecology parties in Western Europe, Herbert Kitschelt (1989, p. 3) argues that “Left-libertarians have engaged in protest movements with loose alliances of federated, egalitarian organizations with little hierarchy or formalization of decision-making procedures. They have attempted to build their parties in the same mode” in the hope of ultimately “creating a more decentralized, libertarian and participatory society with less emphasis on economic competition and growth.” In those nations where it is marginalized by the political entrenchment of social democratic parties, the libertarian left has used the relatively high economic and educational capital of its membership to make visible a logic of politics defined by the representation of alternative life-styles or values rather than by electoral competition and compromise.

This account of institutional change does not imply that challenging groups achieved all that they desired, that oppositional intentions were not co-opted. Indeed, the enduring power of Michels's analysis stems from the broad scope of the logics of incorporation in modern societies. But not all social groups and organizational forms are equally susceptible. Given the organizational heterogeneity of modern society and its consequences for the repertoires of organization that inform political life, the very process of challenging political institutions can change the rules of political action, if not necessarily the substance of political outcomes.

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Racial Differences in Household and Family Structure at the Turn of the Century¹

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Using recently available data drawn from the 1910 census manuscripts, this article documents sharp racial differences in family and household structure at the turn of the century. Compared with those of native whites, African-American households were less likely to be nuclear and more likely to be headed by women. Further, African-American women were much more likely than white women to have surviving children who were not living with them at the time of the census. Because such historical differences parallel contemporary ones, the authors call for greater attention to *persistent* structural, cultural, and demographic factors that affect racial differences in family structure.

INTRODUCTION

Contemporary racial differences in American families are striking. Compared with white women, African-American women are more likely to become parents, bear children as teenagers, have out-of-wedlock births, remain single, experience marital disruption if ever married, and remain unmarried if separated or divorced (see Chen and Morgan 1991; Espen-shade 1985; Farley and Allen 1987). As a result, African-American women are much more likely than white women to head families and households. Conversely, African-American men are less likely to head

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households than white men. Finally, African-American children are much more likely than white children to live in female-headed households or in a household without either parent (Espenshade 1985).

The causes and consequences of these racial differentials have been at the heart of academic and public policy debates for over five decades. Many of their themes can be traced to the Moynihan report, which labeled racial differences in the family as evidence of "pathology" and "disorganization" in the African-American community (Moynihan 1965). Higher rates of nonmarital childbearing and female-headed families are illustrative of family behaviors that some researchers claim have generated a vicious cycle of disadvantage. Moynihan saw the African-American family "as the fundamental source of weakness" in the black community (p. 5). Strengthening the black family was identified as an appropriate public policy and as a precondition for the success of other policy interventions.

The Moynihan report generated a firestorm of controversy. One critique, commonly referred to as "blaming the victim," accepted much of the Moynihan description but challenged his causal arguments (see Rainwater and Yancey 1967). Specifically, this critique maintained that different family structures among blacks did not cause their disadvantaged position but instead the disadvantaged socioeconomic position of blacks produced the observed high prevalence of female-headed families and nonmarital childbearing. Thus the Moynihan position amounted to "blaming the victim" and directed attention away from the underlying causal forces, such as poverty, discrimination, and segregation.

A second critique, one closely related to our work, centered on Moynihan's characterization of the history of black families. Relying heavily on the classic works of E. Franklin Frazier (1932*a*, 1932*b*, 1939), Moynihan had traced the "unstable and disorganized black family" to the ravages of slavery and its aftermath and to the disruptive effects of urban migration. According to Moynihan, this historical experience had produced a "weak," "disorganized," and "matrifocal" family (p. 48). Herbert Gutman (1976) aimed his classic work, *The Negro Family in Slavery and Freedom: 1750-1925*, at correcting this view of African-American family history. Using census data on families from a number of different locations, as well as such other historical documents as diaries and letters, Gutman argued that African-American families had been strong and resilient. In the 50 years after emancipation, he wrote, most African-American families were headed by a husband and wife, most women eventually married, and most children lived with both parents.

Our work relates to this second critique, especially to the work of Gutman and its use by others. For instance, in *The Truly Disadvantaged*, William Julius Wilson (1987, p. 64) states that Gutman's and others'

historical research "demonstrates that neither slavery, nor economic deprivation, nor the migration to urban areas affected black family structure by the first quarter of the twentieth century." This interpretation allows Wilson to move directly to postwar factors in accounting for racial differences in family structure in large urban areas: deindustrialization, job loss for less skilled males, and the selective out-migration of more successful blacks. But Gutman never claimed that black families were unaffected by their historical experiences or that adequate understanding of African-American families could begin by studying only the developments in the second half of the 20th century. "Strong and resilient" does not imply that black families in the past were just like white ones. In fact, where Gutman did compare whites and African-Americans, he frequently found substantial differences (Gutman 1976, pp. 445-47; 1987).

Our research uses the recently released Public Use Sample (PUS) from the 1910 U.S. Census of Population (Strong et al. 1989) to document more precisely than was previously possible the extent of racial difference in family and household structure near the turn of the century. We examine whether these differences were principally due to African-Americans' higher mortality, greater levels of economic stress, or to different patterns of urbanization.

PREVIOUS STUDIES

Only a few national quantitative studies (Engerman 1977; Farley and Hermalin 1971; Gordon and McLanahan 1991; Gutman 1976; Sanderson 1979; Walker 1986) have focused on African-American family structure in the period prior to 1940. With the exception of that of Gordon and McLanahan, these studies suffer from a dependence on published data and can examine only a narrow range of household/family variables. Farley and Hermalin (1971), for instance, use available census and vital registration data to examine four statuses indicative of family stability: current marital status of adults, female headship of families and households, nonmarital childbearing, and the living arrangements of young children. With available data they could trace only the first and last back further than 1940 and could find substantial racial differences at the turn of the century only for the last, the living arrangements of children. Most children under five years of age lived with mothers but this was more true for white children (96.5%) than for African-American children (86.9%) (see Farley and Hermalin 1971, p. 13). Using the 1900 census Public Use Sample, Gordon and McLanahan (1991) show that fewer African-American children lived with both parents than did white children (75.1% vs. 88.6%) and that this difference was greater in urban than rural areas. But Gordon and McLanahan could not provide

much additional detail because of the small sample size provided by the 1900 PUS.

In addition, a number of local studies have examined family and household structure in the decades around 1900. The works of Frazier and Gutman focused on selected counties or cities. Other local studies used computerized data taken from original census manuscripts (Burton 1985; Furstenberg, Hershberg, and Modell 1975; Harris 1976; Shiflett 1975). Consistent with the national studies, these studies produce little evidence of differences in the proportion married but report substantial differences in female headship and the living arrangements of children.

While existing evidence points toward substantial racial differences in past levels of female headship and in the living arrangements of children, the significance and interpretation of these differences are frequently questioned. Specifically, it is commonly argued that mortality accounts for most of the observed racial differences (e.g., see Gordon and McLanahan 1991, p. 106, or Furstenberg et al. 1975, p. 230). Women cannot live with husbands who have died, and children cannot live with deceased parents. Thus, mortality eliminates the need for alternative cultural or structural explanations. But Preston, Lim, and Morgan (1992) show that the 1910 reports of widowhood are much too high to be believed and, as a result, large racial differences in female headship cannot be attributed to mortality alone. In general, Preston et al. (1992, p. 18) argue that "marriage was more fluid and ambiguous than the [census] categories suggested." Similarly, we will present evidence here that differential mortality alone cannot account for the racial differences in children's living arrangements.

DATA

The only pre-1940 national samples of census manuscripts are the Public Use Samples from the 1900 and 1910 U.S. Census of Population.² Gordon and McLanahan (1991) have recently studied children's living arrangements using the 1900 PUS. Here, only data from 1910 are examined. The 1910 PUS (which is 3.7 times larger than the 1900 PUS), coupled with supplementary African-American samples, allows for much more detailed study than is possible with the 1900 PUS.

All analyses in this article combine blacks and mulattoes into an African-American category. Race was assigned by the census enumerator. The instructions for enumerators specify that "the term 'black' includes all persons who are evidently full-blooded negroes, while the term 'mulatto' includes all other persons having some proportion or perceptible

² A public use sample of the 1880 census will soon be available.

trace of negro blood" (U.S. Bureau of the Census 1910, p. 28). While blacks and mulattoes differed on some characteristics, such as literacy,³ the household and family structures of blacks and mulattoes were similar in comparison with native-born whites (Miller, Morgan, and McDaniel 1993).

The 1910 Public Use Sample.—The 1910 PUS is a self-weighting, 1/250 sample of households drawn from microfilms of the 1910 census (see Strong et al. 1989). In 1910, census manuscripts were filled out by enumerators who visited each dwelling. Information collected on each individual included age, sex, race, marital status, relationship to household head, place of birth and mother tongue, and place of birth and mother tongue of parents. We used place-of-birth information to select only African-Americans and native-born whites whose mothers were also native-born. This selection provides a rough control on international migration, one demographic factor that influences household/family structure. Native-born and foreign-born whites differ substantially on some household/family characteristics (Miller et al. 1993).

The 1910 African-American oversamples.—Two oversamples were drawn to produce sufficient samples for certain state-specific estimates of African-American mortality (Ewbank 1987).⁴ The supplementary samples' features make them valuable for a range of research projects. More specifically, southern states with the largest numbers of African-Americans (much of Georgia and all of South Carolina, Mississippi, and Alabama) were not oversampled because the 1/250 PUS contains substantial numbers of African-Americans from these states. Oversample 1 roughly doubles the number of African-Americans in Virginia, North Carolina, Florida, Tennessee, Arkansas, Louisiana, and Texas. Oversample 2 adds more urban blacks by sampling Atlanta and by disproportionate sampling of Maryland and Kentucky, states where larger proportions of African-Americans lived in urban areas. Thus, the supplementary samples "add cases" where they are most needed, that is, outside the states that have the densest concentration of rural African-Americans.

Table 1 shows unweighted individual and household *N*'s for native whites and African-Americans by sample (the 1910 PUS and the African-American supplementary samples). African-American sample 1 is a 1/200 additional sample and sample 2 is a 1/100 sample.

³ Mulattoes are much more likely to be literate than blacks (Miller 1991, chap. 2).

⁴ The oversample data (including the intrahousehold links) have been prepared as a public use tape and are publicly available (see Hereward, Morgan, and Ewbank 1990).

TABLE 1

NUMBER OF INDIVIDUALS AND HOUSEHOLDS IN VARIOUS 1910 SAMPLES BY REGION, STATE, AND RACE

| REGION AND STATE | 1/250 PUBLIC USE SAMPLE | | | | AFRICAN-AMERICAN OVERSAMPLES ^c | | | |
|-----------------------|-------------------------|-----------|--------------------------------|-----------|---|-----------|------------|-----------|
| | Whites ^a | | African-Americans ^b | | Sample 1 | | Sample 2 | |
| | Individual | Household | Individual | Household | Individual | Household | Individual | Household |
| Northeast | 10,064 | 2,877 | 258 | 76 | ... | ... | ... | ... |
| Mid-Atlantic: | | | | | | | | |
| New York | 12,382 | 3,198 | 478 | 157 | ... | ... | ... | ... |
| New Jersey | 3,863 | 918 | 283 | 82 | ... | ... | ... | ... |
| Pennsylvania | 16,474 | 3,849 | 762 | 203 | ... | ... | ... | ... |
| North Central | 62,081 | 14,582 | 2,023 | 622 | ... | ... | ... | ... |
| South Atlantic: | | | | | | | | |
| Delaware | 557 | 138 | 113 | 25 | ... | ... | ... | ... |
| Maryland | 3,135 | 678 | 900 | 191 | ... | ... | 2,050 | 489 |
| Washington, D.C. | 646 | 173 | 364 | 84 | ... | ... | ... | ... |
| Virginia | 5,247 | 1,140 | 2,859 | 587 | 2,265 | 528 | ... | ... |

| | | | | | | | |
|------------------------------|---------|--------|--------|-------|--------|-------|-------|
| West Virginia | 4,081 | 858 | 225 | 54 | ... | ... | ... |
| North Carolina | 5,917 | 1,210 | 2,864 | 587 | 3,310 | ... | ... |
| South Carolina | 2,504 | 522 | 3,459 | 766 | ... | 695 | ... |
| Georgia | 5,596 | 1,139 | 4,836 | 1,140 | ... | ... | ... |
| Florida | 1,319 | 305 | 1,200 | 297 | 1,411 | ... | 259 |
| Other South: | | | | | | 401 | ... |
| Kentucky | 7,414 | 1,598 | 1,142 | 256 | ... | 2,028 | 565 |
| Tennessee | 6,769 | 1,439 | 1,822 | 444 | 2,240 | 546 | ... |
| Alabama | 4,453 | 884 | 3,313 | 808 | ... | ... | ... |
| Mississippi | 2,908 | 595 | 3,703 | 898 | ... | ... | ... |
| Arkansas | 4,085 | 807 | 1,666 | 399 | 1,762 | 441 | ... |
| Louisiana | 3,071 | 625 | 2,720 | 648 | 2,126 | 512 | 609 |
| Oklahoma | 5,130 | 1,107 | 554 | 125 | ... | ... | ... |
| Texas | 10,017 | 1,975 | 2,664 | 642 | 3,153 | 747 | ... |
| Mountain/Pacific | 13,902 | 3,985 | 178 | 70 | ... | ... | ... |
| All regions and states | 191,616 | 44,602 | 38,086 | 9,161 | 16,267 | 3,870 | 1,922 |

^a Native-born whites whose mothers were also native-born.

^b African-American category includes black and mulatto. (See data section for definitions.)

^c A few whites are included in these samples. An occasional white (7 in all) is included because the sample is of African-American-headed households.

Linking coresident spouses and linking children to mothers.—Ever-married women were asked how many children they had ever borne and how many of these were still living. Using this information, the recorded "relationship to household head," and other variables, one can attempt to identify which woman is mother to which coresident children. Spouses can also be linked. (See Strong et al. [1989] for details of these linking procedures.) This linking establishes kinship relations from a set of coresidents. Such linking is crucial for determining whether mother-child(ren) groups are living with their husband/fathers or whether children are living with their mothers. We will discuss the importance of these linking variables in the following section.

MEASURING HOUSEHOLD AND FAMILY STRUCTURE

When discussing differences in household structure it is common to argue that the crucial differences in America are between male- and female-headed households (Billingsley 1988). Particularly in studies of the African-American family, it is often assumed that male(couple)-headed households are more stable and wealthier than female-headed ones. Studies show that most African-American households during the 20th century have been male-headed (e.g., see Agresti 1978; Farley 1971; Farley and Hermalin 1971; Gutman 1976). But a focus on headship alone ignores other dimensions of variation among African-American families and across racial groups.

In this article we present data on two features of households: headship and extendedness. Two procedures followed by census enumerators allow these variables to be defined (see U.S. Bureau of the Census 1910). The enumerator first listed individuals by household, beginning with the head of household, and then recorded each individual's relationship to the household head. Thus, household structure can be described in a straightforward manner.

Households can be headed by a couple (e.g., a married man with spouse present), a man, or a woman. Extendedness is represented by four contrasts: (1) nuclear family members only (nuclear);⁵ (2) at least some nonnuclear kin, but no nonrelatives (extended); (3) at least some non-kin (primarily boarders, lodgers, and employees), but no nonnuclear relatives (augmented); (4) both nonnuclear kin and nonrelatives (extended/augmented).

We focus most attention on racial differences in family structure, but defining family structure is not straightforward. To begin with, given census procedures, only coresident persons can be considered part of the

⁵ Nuclear members include husband, wife, and children (including adopted children and stepchildren).

family. Next, one must decide how to assign coresidents to families. Criteria for family membership are culturally and temporally specific. Rather than assigning all persons to families, we focus on two central aspects of nuclear family structure (Hareven 1976; Laslett 1972): (1) living arrangements of children and (2) living arrangements of mothers. We expect that, if the fundamental residence rules of the nuclear family are adhered to, children will live with their mothers, and mothers will live with their husbands.

To measure adherence to these nuclear family norms, we construct four variables using the intrafamily links constructed from the manuscript records. First for each child we ask ourselves whether he or she lives with his or her mother (the answer is either yes or no, i.e., linked or unlinked to a mother). We also construct a second measure of coresidence with either parent: for each child does he or she live with mother or father. A third measure of child-mother coresidence is calculated for mothers: for those with only young children, we ask whether all of their surviving children live with them (yes or no). We have examined the census's internal consistency between the number of children that mothers report as surviving and the number enumerated in the census. We conclude from this exercise that the 1910 census provides a sound basis for examining the living arrangements of children. (A detailed description of this process is available from the authors on request.)

The fourth variable measures husband-wife coresidence. We select women who have one of their own children (less than 15 years old) living with them and then search the household roster to see whether any person listed is likely to be the woman's spouse. The resultant variable is spouse present, yes or no.

RACIAL DIFFERENCES IN HOUSEHOLD STRUCTURE

Table 2 shows the number of households (part 1) and racial odds (African-American/white, in part 2) by household structure and household headship. Clearly, nuclear family structure is the prominent form; only 16% (9,216 out of 56,378) of households are classified as extended, 10% as augmented, and 3% as augmented/extended. Nuclear households have been found to predominate even in societies where extended residence is the "preferred" type (see, e.g., Burch 1967; Hsu 1943). Further, extended residence in the 1910 U.S. context seldom implies that households contain more than one married couple. Only about one in 100 married couples lived in a household that contained a second married couple. Typically extendedness was produced through coresidence of a single relative or by a few relatives with a nuclear family. Coresiding nuclear families are very rare.

TABLE 2
NUMBER OF HOUSEHOLDS AND RACIAL ODDS (AFRICAN-AMERICAN/NATIVE-BORN WHITE) BY HOUSEHOLD STRUCTURE
AND HOUSEHOLD HEADSHIP

| HEADSHIP | NUMBER OF HOUSEHOLDS | | | | RACIAL ODDS | | | |
|--------------|----------------------|----------|------------------------|--------|---------------------|----------|------------------------|-------|
| | Household Structure | | | | Household Structure | | | |
| | Nuclear | Extended | Augmented/ Extended | Total | Nuclear | Extended | Augmented/ Extended | Total |
| Couple | 29,460 | 6,540 | 4,093 | 41,214 | .23 | .32 | .19 | .24 |
| Man | 5,380 | 1,212 | 656 | 7,494 | .28 | .24 | .54 | .29 |
| Woman | 4,820 | 1,465 | 984 | 7,669 | .33 | .53 | .52 | .40 |
| Total | 39,660 | 9,217 | 5,733 | 56,377 | .25 | .34 | .27 | .26 |

SOURCE.—Weighted household data, 1910 PUS, and African-American oversamples. White sample is self-weighting; the black sample is weighted to produce national estimates for the African-American population (i.e., the mean black weight equals 1.0 and weights are inversely proportional to the sampling fraction).

Table 2, part 2 shows the odds (or ratio) of African-American households to white households by extendedness and headship. If there were no racial differences in household structure or household headship, then each odds in the table would be equal to .26 (total number of African-American households/total number of native white households). Thus observed values greater than .26 indicate a disproportionate number of African-American households; lower values indicate a relative surplus of white households.

African-American households were disproportionately represented among extended households (.34) and underrepresented among nuclear households, especially those headed by a couple (.23). If we focus on couple-headed households, black households were 1.4 times (.32:.23) more likely to be extended than were white households. Among extended households, those headed by females were twice as likely to be African-American (vs. white) as those headed by men. Whites were disproportionately represented among couple-headed households. This overrepresentation is greater for those with nuclear (as opposed to extended) structures and greater still for those with augmented structures.

Nuclear, extended, and augmented household structures may represent various phases of the family's developmental cycle (Berkner 1972, 1975; Vinovskis 1987). The census provides only a cross-sectional view of households. When census results show a low incidence of extended families, it is tempting to conclude that extended family living was experienced by only a small minority of families. However, it is possible that most families were extended at some point but that the duration of coresidence was short. If so, cross-sectional data may show only a minority of families as extended, even though the large majority of people live in extended households at some point in their lives. To provide a rough control on the developmental cycle of the family, we focus on ever-married men and examine extended residence patterns by age.

Table 3 shows the odds of extended residence (extended or augmented/extended) for ever-married men by race and age. Extended residence was more common for young and older men. African-American men were more likely than white men to live in extended households at all ages, but this was especially true at the oldest ages.⁶ If these odds remained stable as a cohort aged from 18 to 60, then on average African-American men would live 11.6 years in extended residences compared with 9.9 years for white men. This racial difference is modest in size but consistent with the greater extendedness of African-American families documented

⁶ The 1.36 racial differential at age 41+ is significantly greater than the other ratios. Specifically, a model allowing for age, race, and a heightened race effect at age 41+ fits the data very well ($\chi^2 = .6$ with 3 df).

TABLE 3
ODDS THAT EVER-MARRIED MALES LIVED IN AN EXTENDED (EXTENDED OR
AUGMENTED/EXTENDED) HOUSEHOLD BY RACE AND AGE

| ODDS OF LIVING IN AN EXTENDED HOUSEHOLD* | | | |
|---|------------------------------|-------------------|-------------------------------|
| AGE | African-American Odds (1) | White Odds (2) | Ratio of Cols. (1) and (2) |
| 18-25 | .35 (1,291) | .32 (3,489) | 1.06 |
| 26-30 | .29 (1,559) | .27 (5,293) | 1.06 |
| 31-35 | .30 (1,323) | .27 (5,305) | 1.12 |
| 36-40 | .28 (1,367) | .25 (5,356) | 1.12 |
| 41+ | .44 (4,081) | .32 (20,135) | 1.36 |
| YEARS IN AN EXTENDED HOUSEHOLD (AGES 18-60) | | | |
| | African-American (1) | White (2) | Ratio of Cols. (1) and (2) |
| | 11.6 | 9.9 | 1.17 |

SOURCE.—Weighted individual data, 1910 PUS, and African-American supplementary samples.
NOTE.—Nos. in parentheses are *N*'s.
* Versus other household types.

for the contemporary period (Farley and Allen 1987; Angel and Tienda 1982).

Neolocal residence implies that ever-married men will head their own households. Headship odds (the odds that persons of a specific age or sex group are household heads) are, in effect, an inverse measure of household complexity. The greater the tendency for males to head their own households, for instance, the fewer other adult males are available to produce more complex forms (see United Nations 1980, p. 100). To what extent do African-American and white men differ in their headship patterns? Table 4 shows the odds of heading a household and the odds of heading a nuclear household by race and age. African-American males are less likely than whites to head a household at all ages under 40 years old.⁷ These differences at older and younger ages cancel one another when we calculate years heading a household. African-American men

⁷ These odds are well fit by a model that allows for age and race effects and an interactive effect of race for those aged 41+ ($\chi^2 = 4.8$ with 3 *df*). Note this model constrains the ratio at ages less than 41 to be equal.

TABLE 4

ODDS THAT EVER-MARRIED MALES HEADED A HOUSEHOLD OR NUCLEAR HOUSEHOLD
BY RACE AND AGE

| AGE | HEADING A HOUSEHOLD* | | | HEADING A NUCLEAR HOUSEHOLD† | | |
|----------------------------|---------------------------|----------------|----------------------------|------------------------------|----------------|----------------------------|
| | African-American Odds (1) | White Odds (2) | Ratio of Cols. (1) and (2) | African-American Odds (3) | White Odds (4) | Ratio of Cols. (3) and (4) |
| 18-25 | 3.4 | 3.8 | .91 | 1.5 | 1.8 | .82 |
| 26-30 | 5.8 | 6.7 | .86 | 1.7 | 1.9 | .90 |
| 31-35 | 6.8 | 8.2 | .82 | 1.7 | 1.9 | .91 |
| 36-40 | 8.7 | 11.7 | .74 | 1.6 | 1.9 | .84 |
| 41+ | 9.9 | 8.1 | 1.18 | 1.3 | 1.6 | .84 |
| YEARS HEADING A HOUSEHOLD* | | | | | | |
| AGE | African-American Odds (1) | White Odds (2) | Ratio of Cols. (1) and (2) | African-American Odds (3) | White Odds (4) | Ratio of Cols. (3) and (4) |
| | | | | | | |
| 18-60‡ | 37.45 | 37.55 | 1.00 | 25.47 | 27.28 | .93 |

SOURCE.—Weighted individual data, 1910 PUS, and African-American supplementary samples. No. of observations by age are same as in table 3.

* Versus not being a household head.

† Versus not being a household head or heading a household of another type.

‡ Years lived in these statuses if these odds remain constant as a hypothetical cohort with ages from 18-60 years.

are less likely than white men to head a *nuclear* household at all ages.⁸ Given these estimates, ever-married, native whites would head nuclear households for 1.8 more years (between the ages of 18-60) than would African-Americans.

These racial differences we describe deserve much greater attention. But our tabulations do establish modest racial differentials in 1910 that mirror contemporary ones. The family structure differences to which we now turn are more striking and we explore them in greater detail.

RACIAL DIFFERENCES IN FAMILY STRUCTURE

During the first half of the 20th century, most children lived with their mothers. However a considerable proportion of African-American chil-

⁸ These odds are well fit by a model including additive race and age effects ($\chi^2 = 2.4$ with 4 *df*). Note that this model constrains the racial differential to be equal at all ages.

dren did not (Cho, Grabill, and Bogue 1970; DuBois [1908] 1969; Grabill, Kiser, and Whelpton 1958). Most authors have attributed this racial difference to the tendency for unmarried mothers to place their children with a relative while they live and work elsewhere (e.g., Cho et al. 1970; Grabill et al. 1958). In short, most studies assume that separate residence for mothers and children is principally a response to unstable family situations (illegitimacy or widowhood).

Table 5 shows differences in the family structure variables defined earlier. The first part identifies children who did not live with their mothers.⁹ The odds of .18 for African-Americans indicate that about 15% (.18/[1 + .18]) of the African-American children ages 0–14 were not living with their mothers in 1910. The comparable odds for whites were .07 (or 6.5%). These estimates are comparable in magnitude to census data for later years (Cho et al. 1970; Grabill et al. 1958). The ratio of these odds (2.6:1) indicates that an African-American child is 2.6 times more likely than a white child to be living in a household where his or her mother is not residing.

Note that older children were more likely than younger ones to be unlinked to coresident mothers. Norms specifying coresidence for children and mothers were strongest at young ages; at older ages the exigencies of schooling and employment can necessitate leaving the parental home. Also, the proportion of mothers who die increases with the children's ages; children clearly cannot be linked to deceased mothers. While the odds of a child being unlinked increase with age, the racial differential is greatest for younger children.¹⁰ This greater racial differential (in the odds of not living with one's mother) at younger ages (0–9) suggests that the differentials cannot be accounted for by the African-American

⁹ Young children have been consistently underrepresented in population census returns (Grabill et al. 1958; Coale and Rives 1973). Coale and Rives estimate that 13% of African-American children aged 0–14 were not enumerated in the 1910 census. A substantial part of this undercount may be due to misstatements of age. Misstatements of age are a significant problem in a census which reports the age of a child in terms of an approaching birthday (see Grabill et al. 1958). Estimates for the national population by age, sex, and race are not seriously biased by the undercount. However, serious distortions may occur in smaller geographic areas, particularly cities (Coale and Rives 1973). The major concern here is whether the undercount of children would affect our estimates of the proportion not living with mothers. We suspect that African-American children not living with mothers would be most likely to be missed in the census. Thus, we speculate that underenumeration biases down our estimates of the proportion not living with mothers.

¹⁰ Formal tests show that the decline in the racial differential with age of child is statistically significant (at the .01 level). Specifically, the additive model (which allows race and age effects) has $\chi^2 = 25.2$ with 2 *df*. Allowing for a linear decline in the racial differential with age provides a near perfect fit of model to data ($\chi^2 = .04$; 1 *df*).

TABLE 5
FAMILY STRUCTURE VARIABLES BY RACE

| Variables | African-American Odds (1) | White Odds (2) | Ratio of Cols. (1) and (2) |
|--|---------------------------------|----------------------|-------------------------------|
| 1. Children who do not live with mother (by age of children): | | | |
| 0-14 | .18 (18,841) | .07 (75,886) | 2.6 |
| 0-4 | .09 (6,591) | .03 (27,572) | 3.0 |
| 5-9 | .18 (6,522) | .07 (25,311) | 2.6 |
| 10-14 | .28 (5,726) | .13 (23,003) | 2.2 |
| 2. Children who do not live with mother or father (by age of children):* | | | |
| 0-14 | .11 (18,841) | .04 (75,886) | 2.8 |
| 0-4 | .07 (6,591) | .02 (27,572) | 3.5 |
| 5-9 | .12 (6,522) | .03 (25,311) | 4.0 |
| 10-14 | .16 (5,726) | .05 (23,003) | 3.2 |
| 3. Married mothers who have children living elsewhere (by age of mother and presence or absence of spouse):† | | | |
| 15-35 | .13 (2,398) | .03 (10,185) | 4.3 |
| 15-24 | .12 (1,056) | .04 (3,454) | 3.0 |
| 25-35 | .13 (1,342) | .03 (6,731) | 4.3 |
| spouse present | .10 (2,227) | .03 (9,948) | 3.3 |
| spouse absent | .47 (171) | .52 (237) | .9 |
| 4. Female-headed families (by age of mother): | | | |
| 15-49 | .29 (6,122) | .08 (24,206) | 3.6 |
| 15-24 | .24 (1,334) | .05 (3,818) | 4.8 |
| 25-34 | .23 (2,351) | .05 (9,350) | 4.6 |
| 35-49 | .38 (2,438) | .12 (11,038) | 3.2 |

NOTE.—Figures in parentheses are *N*'s. Weighted individual data, 1910 PUS, and African-American supplementary samples.

* These are the odds that a child does not live with mother and does not live in a household headed by his/her father.

† These women have at least one surviving child and have been married only once for less than 13 years.

children's earlier age at marriage (or parenthood) or by their earlier entrance into the labor force (which might account for some difference at ages 10-14).

A substantial number of those children not living with their mothers were living in households headed by their fathers. Table 5, part 2 shows the odds that a child was not living with his or her mother and not living in a household headed by his or her father. These odds are lower than in part 1, but the racial differential is greater. In fact, African-American

children were 2.8 times more likely not to live with mother or father than were white children.¹¹

Some children are unlinked because their mothers were dead or because of errors in the data (Grabill et al. 1958). Can racial differences be attributed to these factors? To answer this question we have estimated the proportion of African-American and white children whose mothers had died by the time of the 1910 census. We begin by estimating the age distribution of mothers at childbirth by using the proportion of women who were linked to an own child under the age of one in the 1910 PUS. Allowance is made for the average one-half year displacement between the age of women reported in the census and their age at bearing these children. Next, using female age-specific probabilities of survival from the U.S. life table for the death registration area (DRA) for the period 1901–10 (Glover 1921), we estimated the proportion of mothers who would have died between the time of the child's birth and the census date. For African-Americans we lowered the mortality estimate somewhat because of strong evidence that mortality was greater in the DRA than in the nation as a whole. Specifically, child mortality was substantially lower in areas not included in the DRA than in the included areas (Preston and Haines 1991). Because no such direct comparison is possible for adults, we mapped the geographical disparity in child mortality into a disparity in adult mortality using "West" model life tables (Coale and Demeney 1983).¹² Results are shown in columns 2 and 4 of table 6.

We estimate that 8.7% of the African-American children (aged 0–14) have no surviving mother. In contrast, 5.3% of the white children have no surviving mother. The residuals from this accounting exercise suggest that 6.2% of African-American children and 1.3% of white children have surviving mothers with whom they do not live. If these rates held until children reached age 15, the African-American children would live 2.27 years separately from their mothers—1.35 years attributable to mortality

¹¹ Children are not linked to fathers (as they are to mothers) because there are far fewer "linking variables" for men. Men are not asked how many children they have fathered and how many are still alive, for instance. We can identify the father if he is head of household because the relationship of the child to head is collected for all coresidents. Thus, some children may be living with their fathers but are not identified as such because their father is not the household head. "Not living with parents" measured in this way will produce levels slightly too high. We have experimented with other definitions such as "do not live with mother, or in household headed by father but lives with relatives." These definitions do affect the "level" of child-parent coresidence, but the racial difference is very robust to definitional changes.

¹² A detailed appendix describing these procedures is available from the authors on request.

TABLE 6

PROPORTION OF CHILDREN NOT LIVING WITH MOTHERS AND ESTIMATED PROPORTION
WHOSE MOTHERS HAVE DIED

| | AFRICAN-AMERICAN CHILDREN | | | NATIVE-BORN WHITE CHILDREN | | |
|-------------------------|---------------------------|------------------|---------------------|----------------------------|------------------|---------------------|
| | Unlinked* | Mothers Died† | Difference (1-2) | Unlinked* | Mothers Died† | Difference (3-4) |
| | (1) | (2) | (1-2) | (3) | (4) | (3-4) |
| Age of child: | | | | | | |
| 0-4 | .083 | .029 | .054 | .029 | .015 | .014 |
| 5-9 | .153 | .088 | .065 | .065 | .052 | .013 |
| 10-14 | .218 | .152 | .066 | .115 | .098 | .017 |
| 0-14 | .149 | .087 | .062 | .065 | .053 | .013 |
| γ^\ddagger | 2.27 | 1.35 | .93 | 1.05 | .83 | .22 |

* Proportion (p) calculated from odds in table 5: $p = \text{odds}/(1 + \text{odds})$.

† See text for discussion of estimation.

‡ Years spent in each state if children experienced the rates shown is indicated by γ . This is computed by summing the first three rates in each column and multiplying by 5.

of mothers and .93 years to separate residence. All parallel estimates are substantially lower for whites.

Could errors in the data account for an additional part of the racial difference? Perhaps minority groups, because of their greater illiteracy or other factors, gave less accurate information to enumerators. We argue this is not the case. Other groups who were impoverished, such as immigrant Italians, Poles, and Jews, show no greater tendency to have children unlinked (see Miller et al. 1993).

In the third part of table 5 we again attempt to measure mother and child coresidence, but here we chose mothers as the unit of analysis. Specifically, we show the odds that mothers (aged 15-35) have children living elsewhere. To ensure that the children living away are "young" (and thus "should be living with mothers," given nuclear residence rules), we include only young women (aged 15-35) who have been married only once and for less than 13 years. Presumably, these mothers have sent their nonresident children to live elsewhere.

Note that the odds of having children living elsewhere is similar to, and the racial differential is greater than, estimates in table 5, part 2: African-American mothers are 4.3 times more likely than white mothers to have children living elsewhere. This racial differential does not vary significantly by the mother's age (15-24 vs. 25-35).¹³ Replicating the

¹³ A model allowing only a race effect fits these data well.

racial differential in mothers' propensity to live apart from their children provides further evidence against the claim that racial differences can be entirely attributed to differential maternal mortality.

Table 5, part 3 also shows, by the presence or absence of a husband, the proportion of mothers with children living elsewhere. We noted above that many have attributed the racial difference in mother-child coresidence to unstable or stressed family situations, such as those produced by nonmarital fertility or widowhood. Clearly, spouse-absent families would be under greater stress than families with a "male breadwinner" present. If white and African-American families were truly alike in the past, then at a minimum they should have followed the same residence rules, given the same basic family composition (i.e., if we focus only on married couples, then the rate of children not living with parents should be very low and roughly the same for both races).

Note that mothers not living with their husbands were more likely to have children elsewhere. The absence of a spouse may have made supporting and caring for children especially burdensome and, therefore, encouraged mothers to send children elsewhere to live. White and African-American women were equally likely to send their children elsewhere to live when their spouses were absent. But, a claim that such arrangements were more than a response to crisis but were, in fact, a normatively approved strategy of child rearing among African-Americans, is supported by the large racial differential in children's being raised elsewhere among families with *both* a mother and her spouse present.¹⁴ We will return to this issue in the discussion section.

Table 5, part 4 shows that the proportion of mother-child groups who had no coresident spouse/fathers (i.e., female-headed families) was much higher for African-Americans than for whites (3.6 times higher). The odds (of female family headship) are .29 (22%) for African-Americans and .08 (8%) for native-born whites. Note that while the odds of female headship increase with the mother's age (perhaps due to mortality), the race differential is greater at the younger ages.¹⁵ As in table 5, parts 1 and 2, if mortality alone were producing these differentials, then the differentials would become greater with age because mortality has longer to operate at older ages.

¹⁴ The racial difference (4.2) for spouse-present women is statistically significant ($P < .001$). The χ^2 test associated with the model of independence is 222.5 with 1 *df*. The racial difference (.9) for spouse-absent women is not statistically significant.

¹⁵ Formal analysis reveals that the decline in the racial differential (with increasing age) cannot be attributed to sampling variability ($P < .01$). A model allowing for a linear decline with age fits the data very well ($\chi^2 = .6$; 1 *df*).

THE PERVASIVENESS OF FAMILY STRUCTURE DIFFERENCES BY RURAL/URBAN RESIDENCE AND REGION

While most African-Americans in 1910 were rural and southern,¹⁶ a common claim is that family structure differences by race were concentrated in urban areas and especially in large cities. There are three plausible reasons why this might be true. First, sociologists often characterize the city as having a destabilizing influence on traditional institutions such as the family (see Billingsley 1988; Frazier, 1932*a*, 1932*b*, 1939; Wirth 1938). A major argument for differences in African-American and white families in urban areas has been the disruptive effects of the city on "simple rural folks" who migrated there (Frazier 1939). Second, mothers who had not married, those whose husbands had died, or those who were deserted may have migrated to the cities because the opportunity for female employment was much greater there (Burton 1985; DuBois [1908] 1969; Meyerowitz 1988). Third, the employment opportunities of African-American females in cities coupled with the severely restricted access to jobs for African-American men may have greatly reduced the incentive to marry and to stay married (Becker 1981; Sanderson 1979).

Table 7 shows the odds that mothers have children living elsewhere by rural or urban residence and region or state of residence. The variable, rural residence, includes small towns (towns with fewer than 2,500 residents); urban locales all have a population of at least 2,500 persons. The observed data suggest sharp racial differentials in both rural and urban areas. Formal analysis of these odds reveals that separate race and rural and urban effects describe the data well.¹⁷ Specifically, African-American mothers were 3.6 times more likely to have had children living elsewhere than were white mothers. Residence in urban areas increased the risk of having children living elsewhere by a smaller factor of 1.3 and there was no significant interaction between race and rural or urban residence.

Table 8 shows a parallel tabulation of the odds of female headship by rural/urban and state/region of residence. There is a pervasive racial difference. African-American families were much more likely to be female headed. A more formal analysis reveals interactive effects of resi-

¹⁶ The percentage distribution of the African-American population by regions of the conterminous United States in 1910 was as follows: 4.9% in the Northeast, 5.5% in the North Central, 89% in the South, and 0.5% in the West (Price 1969). In 1910 there were three major geographic centers of this population: the rural South, where roughly 79% of our sample live; the urban South, where about 10% of our sample live; and the urban North, where roughly 6% of our sample reside.

¹⁷ Logit models were fit to the data (using minimum logit chi-square regression). The data are adequately described by a model including additive effects of race and rural/urban residence. Additional detail is available from authors.

TABLE 7
ODDS THAT MARRIED MOTHERS HAVE CHILDREN LIVING ELSEWHERE, BY RACE,
RURAL OR URBAN RESIDENCE, AND REGION OR STATE OF RESIDENCE*

| REGION OR STATE | AFRICAN-AMERICAN | | WHITE | |
|------------------------|------------------|-------------|-------------|-------------|
| | Urban Odds | Rural Odds | Urban Odds | Rural Odds |
| New England | .10 (11) | (0) | .05 (280) | .03 (113) |
| Mid-Atlantic: | | | | |
| New York | .19 (19) | (2) | .05 (365) | .03 (223) |
| New Jersey | (7) | .14 (8) | .02 (118) | .00 (69) |
| Pennsylvania | .06 (19) | (8) | .04 (428) | .04 (411) |
| North Central | .23 (53) | .08 (28) | .04 (1,193) | .03 (2,044) |
| South Atlantic: | | | | |
| Delaware | (1) | (3) | (9) | .00 (18) |
| Maryland | .24 (51) | .16 (88) | .05 (78) | .09 (88) |
| Washington, D.C. | .17 (14) | (0) | .00 (35) | (0) |
| Virginia | .11 (69) | .11 (196) | .07 (60) | .02 (237) |
| West Virginia | (4) | .11 (10) | .03 (34) | .07 (194) |
| North Carolina | .19 (63) | .13 (290) | .02 (51) | .04 (304) |
| South Carolina | .29 (27) | .09 (182) | .07 (31) | .01 (141) |
| Georgia | .25 (65) | .12 (234) | .04 (70) | .04 (257) |
| Florida | .19 (25) | .11 (71) | .08 (14) | .00 (65) |
| Other South: | | | | |
| Kentucky | .11 (52) | .02 (53) | .03 (64) | .02 (332) |
| Tennessee | .21 (70) | .06 (128) | .04 (57) | .03 (323) |
| Alabama | .35 (23) | .09 (135) | .12 (47) | .03 (234) |
| Mississippi | .23 (16) | .13 (169) | .05 (20) | .02 (137) |
| Arkansas | .22 (22) | .16 (131) | .05 (23) | .02 (224) |
| Louisiana | .22 (73) | .10 (274) | .03 (37) | .01 (139) |
| Oklahoma | .13 (9) | .13 (17) | .00 (48) | .03 (250) |
| Texas | .22 (61) | .11 (229) | .02 (138) | .02 (465) |
| Mountain/Pacific | (3) | (1) | .05 (272) | .05 (444) |
| All states | .19 (757) | .11 (2,257) | .04 (3,472) | .03 (6,712) |

NOTE.—Unweighted individual data, 1910 PUS, and African-American supplementary samples. The sample is roughly self-weighting for individual states. Figures in parentheses are *N*'s; empty cells indicate that odds were not calculated because fewer than 10 women fitted category.

* Mothers are 15–35 years old.

dence and race.¹⁸ Specifically, rural/urban differences were larger for African-Americans than for whites. Compared with rural white females who were least likely to head families, urban whites were 1.3 times more

¹⁸ The data were modeled using minimum logit chi-square regression. The preferred model includes additive and interactive effects of race and rural-urban residence. The preferred model does not fit the data very well ($\chi^2 = 141.8$; 86 *df*) indicating substantial state/regional variability. We have not yet identified a meaningful group of states that captures this state variability. Additional detail is available from authors.

TABLE 8

ODDS THAT FAMILIES ARE FEMALE-HEADED, BY RACE, RURAL OR URBAN RESIDENCE,
AND REGION OR STATE OF RESIDENCE*

| REGION OR STATE | AFRICAN-AMERICAN | | WHITE | |
|------------------------|------------------|-------------|--------------|-------------|
| | Rural Odds | Urban Odds | Rural Odds | Urban Odds |
| New England | (0) | .33 (24) | .11 (335) | .12 (730) |
| Mid-Atlantic: | | | | |
| New York | .38 (10) | .25 (44) | .07 (593) | .11 (816) |
| New Jersey | .44 (13) | .36 (15) | .09 (178) | .10 (308) |
| Pennsylvania | .06 (18) | .37 (59) | .06 (1,038) | .11 (1,030) |
| North Central | .08 (70) | .41 (168) | .06 (5,035) | .11 (2,793) |
| South Atlantic: | | | | |
| Delaware | (8) | (2) | .02 (45) | .09 (24) |
| Maryland | .14 (205) | .51 (137) | .07 (215) | .11 (164) |
| Washington, D.C. | (0) | .67 (40) | (0) | .14 (80) |
| Virginia | .23 (437) | .48 (178) | .08 (535) | .16 (137) |
| West Virginia | .20 (24) | (5) | .06 (455) | .12 (76) |
| North Carolina | .24 (644) | .35 (130) | .08 (666) | .15 (113) |
| South Carolina | .24 (416) | .77 (62) | .09 (279) | .21 (52) |
| Georgia | .23 (551) | .63 (197) | .09 (585) | .14 (158) |
| Florida | .28 (250) | .40 (81) | .08 (143) | .22 (39) |
| Other South: | | | | |
| Kentucky | .13 (170) | .64 (179) | .05 (805) | .21 (166) |
| Tennessee | .21 (304) | .46 (172) | .10 (766) | .11 (141) |
| Alabama | .31 (367) | .67 (72) | .10 (506) | .17 (97) |
| Mississippi | .22 (420) | .82 (40) | .09 (319) | .06 (54) |
| Arkansas | .15 (353) | .28 (60) | .07 (479) | .19 (68) |
| Louisiana | .22 (742) | .52 (208) | .09 (303) | .17 (119) |
| Oklahoma | .24 (46) | .45 (29) | .06 (591) | .07 (104) |
| Texas | .22 (599) | .59 (216) | .07 (1,064) | .13 (299) |
| Mountain/Pacific | (6) | .18 (13) | .06 (984) | .10 (716) |
| All states | .22 (5,649) | .50 (2,131) | .07 (15,919) | .12 (8,284) |

NOTE.—Unweighted data, 1910 PUS, and African-American oversamples. The sample is roughly self-weighting for individual states. Figures in parentheses are *N*'s; empty cells indicate that odds were not calculated because fewer than 10 women fitted category.

* Mothers are 15–49 years old.

likely to head families. Compared with this rural white referent, rural and urban African-American women were 3.0 and 5.4 times more likely to head families, respectively. Thus while female family headship was somewhat greater in urban areas, the primary finding here is the existence of pervasive racial differences.

Perhaps racial differences in family structure were most stark in large cities that contained concentrations of African-Americans. Special caution is urged when examining data for large cities because of suspicions

that undercounts could have been substantial there (see n. 8). Table 9 shows the 13 U.S. cities that had the largest concentrations of African-Americans in 1910. For all cities the odds of female headship for whites and African-Americans were .12 and .54, respectively. These odds are roughly comparable to those in table 8 for all urban places with populations over 2,500. The racial difference is unmistakable (and clearly statistically significant) but there is little evidence of differences by city in levels of headship or in racial differentials.

The proportion of mothers with children living elsewhere was substantially higher in these cities compared with levels shown for all urban places (in table 7) or for the nation as a whole (in table 5). Also, the odds that a child who lived in one of these cities was not living with his mother (e.g., unlinked) is actually slightly below the national average. Frazier (1939) and DuBois (1899) both commented on a tendency for urban blacks to leave young children with relatives in rural areas. These results are consistent with such a scenario.

DISCUSSION

Living arrangements provide important clues about family structure. At the turn of the century, these arrangements were distinctly different for native-born whites and African-Americans. For instance in the cities with the largest number of African-Americans, 35% of African-American women living with children lived in a household where no spouse could be identified. The comparable figure for whites is 11% (see table 8; national estimates are 22% and 7%, respectively—see table 5). Roughly comparable estimates for the two racial groups in 1990 are 53% and 16%, respectively.¹⁹ While the 1990 figures are much higher, they maintain the sharp racial differential that was firmly established by 1910. Levels of (and racial differences in) children's living arrangements have been more stable. In 1980, 12% of African-American children did not live with their mothers; the comparable figure for whites was 3%.²⁰ These estimates are somewhat lower than those we show in table 5 for 1910.

Perhaps women cannot live with husbands and children cannot live with mothers because these persons have died. The much higher mortality of African-Americans could thus account for the racial differential.

¹⁹ The estimate is only roughly comparable because the 1990 estimate is for all SMSAs where the 1910 estimate is only for the cities shown in table 8. The 1990 data come from U.S. Bureau of the Census (1991, table 9).

²⁰ This estimate comes from Espenshade (1985, table 8). It is not strictly comparable with the 1910 figure since Espenshade's sample is children 0–18 years old who are not maintaining households or family groups. Our sample includes all those aged 0–14.

TABLE 9

ODDS OF HAVING A FEMALE-HEADED FAMILY, FOSTERAGE, AND "UNLINKED CHILDREN" FOR 13 CITIES WITH LARGEST NUMBER OF BLACKS

| CITY | AFRICAN-AMERICAN POPULATION | ODDS OF FEMALE HEADSHIP | | ODDS OF FOSTERING CHILD | | ODDS OF CHILD'S BEING UNLINKED | |
|-----------------------|-----------------------------|-------------------------|------------|-------------------------|------------|--------------------------------|-------------|
| | | African-American Odds | White Odds | African-American Odds | White Odds | African-American Odds | White Odds |
| Washington, D.C. | 94,000 | .67 (40) | .14 (80) | .17 (14) | .0 (35) | .38 (98) | .03 (195) |
| New York | 92,000 | .42 (37) | .11 (365) | .15 (15) | .05 (181) | .04 (58) | .04 (1,714) |
| New Orleans* | 89,000 | .45 (81) | .11 (73) | .33 (36) | .04 (26) | .16 (191) | .06 (256) |
| Baltimore* | 85,000 | .46 (121) | .09 (122) | .26 (48) | .04 (52) | .28 (262) | .04 (350) |
| Philadelphia | 84,000 | .61 (32) | .17 (236) | .0 (9) | .03 (90) | .12 (58) | .03 (770) |
| Atlanta* | 52,000 | .72 (103) | .21 (52) | .24 (26) | .05 (23) | .07 (218) | .03 (108) |
| Birmingham | 52,000 | .63 (26) | .26 (34) | .50 (3) | .13 (17) | .09 (48) | .04 (93) |
| Memphis* | 52,000 | .41 (48) | .07 (32) | .38 (18) | .0 (13) | .16 (94) | .06 (73) |
| Richmond* | 47,000 | .37 (63) | .14 (33) | .13 (27) | .07 (15) | .19 (124) | .0 (81) |
| Chicago | 44,000 | 1.10 (15) | .09 (176) | .25 (5) | .04 (83) | .11 (20) | .04 (844) |
| St. Louis | 44,000 | .42 (17) | .08 (111) | .67 (5) | .08 (56) | .03 (33) | .02 (459) |
| Louisville* | 41,000 | .74 (59) | .18 (52) | .15 (15) | .0 (21) | .18 (100) | .04 (152) |
| Nashville* | 37,000 | .63 (39) | .15 (31) | .23 (16) | .15 (15) | .15 (78) | .02 (61) |
| | | .12 | | | | | |
| All 13 cities | 813,000 | .54 (681) | (1,397) | .23 (237) | .05 (627) | .17 (1,382) | .03 (5,156) |

NOTE.—Figures in parentheses are *N*'s.

* Cities that were included in the black supplementary sample. These estimates are unweighted; the sample is self-weighting for individual cities.

Preston et al. (1992) show that widowhood reports by African-Americans are greatly exaggerated in census estimates and that many who report widowhood are actually separated, divorced, or never married. Mortality was high and played havoc with African-American family stability but it still cannot account for the level of spouse absence observed. Likewise, less than half of the greater tendency of African-American children to live separately from their mothers can be accounted for by the higher mortality of African-American mothers.

The differences in the living arrangements of women and children that we document were geographically pervasive—they are unmistakable in the North and South and in both rural and urban areas. These pervasive differentials are not consistent with claims that family differences originate in the African-American migration to the North or to urban areas. Some evidence of greater racial differences in urban areas (like that found in table 8) suggests migration may have played a secondary role.

Remaining racial differences could be accounted for by socioeconomic factors, such as poverty or women's employment. Becker (1981) and Sanderson (1979) would predict less marriage and more divorce for African-Americans (than for whites) based on African-Americans' higher rates of female labor-force participation and greater (male/female) wage equality. The difficulty that black males experienced earning a "family wage" could also account for their lower odds of household headship (see table 5). Similar arguments are sometimes offered for contemporary differences. Thus, stable structural or economic factors may account for the apparent continuity in racial differences.

Finally, cultural/historical factors have been understudied and misunderstood. While cultural/historical continuity has been uncovered in the religious and linguistic behavior of African-Americans (Drake 1970; Simpson 1978) and despite work on the Caribbean that takes cultural and historical influences on family form seriously (see, e.g., Smith 1956; Sanford 1975; Goody 1982), most historians of the African-American family have gone to great lengths to discount the possibility of cultural continuity between African and African-American family systems (Frazier 1939; Elkins 1963; Fogel and Engerman 1974; Moynihan 1965). For example, Frazier (1939) suggests that the African-American family disintegrated following slavery, emancipation, and the migration to northern cities. But perhaps there was no disintegration of the African nuclear family, simply put, because the African family system during slavery was not exclusively nuclear.

In sub-Saharan Africa strong ties and obligations to extended kin permeate society. Sudarkasa (1981) has suggested that these ties and obligations may rival conjugal ones. Some contemporary historians have argued that this African family pattern was reinforced by slavery in the

antebellum South (Owens 1976; Gutman 1976). Despite the absence of any legal standing for slave "marriage," slaves were able to maintain strong familial bonds, especially kin bonds (DuBois 1969; Gutman 1976; Owens 1976). These traditions could make spouse absence and separation more acceptable among African-Americans than among whites. These traditions may represent distinct cultural adaptations with potential strengths (Allen 1978; Gutman 1976; Hill 1971; McDaniel 1990; Nobles 1978; Stack 1974). Such differences are more likely to arise and be maintained in a situation of persistent residential segregation and absence of racial interaction in economic, social, and cultural spheres.

Racial differences in children's living arrangements may be partly explained by such cultural/historical arguments (DuBois 1969, [1935] 1963; Gutman 1976, 1987; Nobles 1978; Owens 1976). As noted above, separate residences for mother and children can be viewed as a violation of nuclear family residence rules in response to a crisis situation. But a counterargument recognizes separate residence as a normative feature of West African families, the institution of *fosterage*. Fosterage entails some responsibility for and control over relatives' children. These responsibilities and rights produce large proportions of children not living with parents. In fact, recent work on fosterage in modern Africa and the Caribbean (Goody 1982; Isiugo-Abanihe 1985; Page 1989; Sanford 1975) shows the practice to be very prominent. In the African context, there are many reasons for sending children to live elsewhere (Bledsoe and Isiugo-Abanihe 1989). Economic hardship is certainly one. Other reasons include the desire for a better environment for the child, one with greater access to education or health care, for instance. Some children are fostered to provide others with children. Children can provide these persons with companionship and assistance.

Perhaps the contemporary residence patterns of African-American children have distant links in traditional West African patterns. As evidence, note the contemporary racial differences we document and that the U.S. 1910 racial difference in children's living arrangements appears to be more than just a response to "crisis conditions" such as mortality and economic hardship. In fact, racial differences were most pronounced for women residing with spouses (those who should have been relatively better off, see table 5) rather than for those who were female family heads.

Of course, partitions of racial differences into demographic, economic, or cultural/historical components are overly simplistic. For instance, the attribution of a significant portion of the racial differential in female headship to mortality should not end the inquiry. Instead it begs the question: How did African-American families respond to the brutal mortality regime they faced? Perhaps female headship among African-

Americans was common because high death rates diminished the gains from investment in a stable union. In short, adequate explanation will require synthesized arguments that weave together the influence of demographic, socioeconomic, and cultural/historical factors.

CONCLUSION

We have shown clear differences between African-American and white family structures at the turn of the century. While our data allow for new and more detailed tabulations than were possible before, our results are largely consistent with previous studies. For instance, Furstenberg et al. (1975, p. 219) used 1850 and 1860 census data to show that roughly 1 out of 4 African-American households in Philadelphia were female headed. Parallel odds for native whites were 1 out of 10. Our estimates of female-headed families for 13 large cities in 1910 (see table 9) show roughly comparable levels.

Given such well-documented historical differences, why are contemporary explanations for racial differences focused on the contemporary period? We see several possible explanations. First, as noted in the introduction to this article, some researchers have misinterpreted the classic work of Gutman by inferring from it that Gutman found no racial differences. But Gutman did not claim that black and white families were alike in the past, only that black families were strong and resilient. Second, some writers have acknowledged past differences but have attributed them to racial differences in mortality. But, like Preston et al. (1992), we argue that mortality alone cannot account for historical differences. Third, racial differences have widened recently. Thus, arguments about contemporary influences are certainly required for a full explanation of contemporary differences. Finally, quantitative analyses require quantitative data. There is much more of it available for the postwar period than for earlier periods. As a result, studies and explanation focus on the more recent periods.

Our descriptive work is intended to remind researchers that racial differences are not new. Those explaining contemporary differences need to recognize that these may be rooted in long-standing differences in family and household processes, differences that are nurtured by enduring traditions of racial separation and exclusion. Each generation does not invent its own family structure anew but adapts, in the light of current conditions, the traditions and practices that it has inherited from the past. The cultural, social, and economic history of African-Americans is radically different from that of white Americans. It should not be surprising that their family structures have persistently reflected some measure of these differences throughout the 20th century.

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A Status-based Model of Market Competition¹

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This article explores the significance of status processes for generating and reproducing hierarchy among producers in a market. It develops a conception of a market as a status order in which each producer's status position circumscribes the producer's actions by providing a unique cost and revenue profile for manufacturing a good of a given level of quality. An examination of pricing behavior among investment banks in the underwriting of corporate securities provides empirical support for this status-based model of market competition. Extensions are discussed.

INTRODUCTION

That there exists a distinction between an actor and an actor's position in the social structure and that rewards are largely a function of position is one of the fundamental insights of the sociological perspective (Simmel 1950). The distinction between actor and position has been applied with much success in the field of stratification research (White 1970; Sørensen 1983). It often figures prominently in sociological critiques of economists' claims that a wide range of economic, social, and political phenomena result from the aggregation of individual preferences (Baron and Hannan 1991), and it underlies the skepticism of the psychologists' claim that behavior can be explained with reference to an actor's personality or disposition (Davis-Blake and Pfeffer 1989).

This article attempts to extend the scope of this insight by applying a particular variant of the distinction between actor and position to market producers. As sociologists have expanded their arena of inquiry to include

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economic institutions, the market has received increased attention, and some steps have been taken to specify the mechanisms through which the market is shaped by noneconomic factors (Burt 1983; Baker 1984, 1990; Granovetter 1985). More specifically, the idea that market producers occupy socially defined positions in the context of the market was introduced by White (1981*a*, 1981*b*), whose primary concern was to elaborate a typology of markets as role structures. The importance of roles in market contexts has recently been extended by Baker and Faulkner (1991).

Like White, I conceptualize the market as a structure that is socially constructed and defined in terms of the perceptions of market participants, but my focus is not so much on roles as it is on status positions. Winship and Mandel's (1983) distinction between roles as classifications across social structures and positions as locations within social structures helps distinguish my endeavor from earlier work. I do not explore how dynamics differ across markets; I examine how a producer's position in the market affects the relative opportunities open to that producer in comparison to those available to its competitors.

My first objective is to elaborate a general framework that makes explicit the connection between status and economic variables such as cost, revenue, and price. In doing so, I proceed from the micro to the macro level. I begin with a definition of status and, from the definition, build a conception of an isolated status position. From there, I move to the conception of a status order and then discuss how the economic constraints and opportunities that confront a producer are very much contingent upon the producer's position in the status order. Having laid out the framework, I then illustrate its utility by applying it to a particular case: pricing dynamics in the primary securities markets.

WHAT IS STATUS?

I define a producer's status in the market as the perceived quality of that producer's products in relation to the perceived quality of that producer's competitors' products.² There are two lenses through which status may be viewed. On the one hand, a producer's status, or more accurately, the association with that status, can be considered something that non-producing market participants (i.e., consumers, investors, and brokers

² When colloquially used with reference to markets, the word *status* is applied primarily to luxury goods. Here, I wish to avoid this implicit association. Words like *prestige* seem particularly awkward when applied to products, and the phrase *perceived quality* fails to convey the sense of an implicit hierarchy or ranking which is central to my understanding of markets.

of exchange) generally value in its own right. Whether considered an end in itself (Frank 1985) or a means toward enhanced power over other individuals (Weber 1978; Veblen 1953), greater status increases the utility derived from the association with or consumption of a good.³ While this view of status is not inconsistent with the framework I will develop, the assumption that nonproducing market participants value status is not necessary either.

More critical is a second view of market status as a *signal* of the underlying quality of a firm's products. If an actor is uncertain of the actual quality of the goods that confront her in the market, or if she is unwilling or unable to bear the search costs of investigating all the different products in the market, then the regard that other market participants have for a given producer is 'a fairly strong indicator of the quality of that producer's output.

This conception of status is compatible with the formal economic understanding of signals. According to Spence (1974), a signal is any observable indicator, of a quality or qualities, that meets two criteria: (1) the indicator must be at least partially manipulable by the actor and (2) the marginal cost or difficulty of obtaining the indicator must be nonzero and inversely correlated with the actor's level of quality. A college diploma is a signal of productivity because its attainment is at least partially within an individual's control and because it is more difficult for those who lack organizational skills (or other such attributes that help constitute productivity) to obtain a college degree. A warranty is a signal because its terms are at the discretion of the producer and because the cost of a given warranty is inversely related to quality; the lower-quality producer, almost by definition, will have to make good on the promise specified in the warranty with greater frequency.

Similarly, status meets the two criteria for signals. Even though a producer's status depends largely on the expressed opinions and actions of others, the producer nonetheless exercises at least some control over its status since its own past actions are important determinants of how it is perceived. Moreover, the difficulty of acquiring a reputation for superior quality is inversely associated with the general quality level of the producer.

Economic models of signaling activity are primarily concerned with equilibrium behavior or comparative static analyses of different equilib-

³ Note the proposition that nonproducing market participants value status does not imply that a desire for status is an overarching motive for producers as an alternative to profit maximization. Throughout, I assume that producers are interested in profit maximization, but that they realize that the means for realizing profits is contingent upon the status position occupied in the market.

rium conditions. A formal condition of equilibrium in signaling models is that the actual distribution of a producer's quality must be equal to the distribution of quality that constituents expect on the basis of the signal. In assuming an equality between what exists and what is expected, these economic models necessarily give less attention to those factors that may undercut this equality and engender only a *loose linkage* between a signal and that which it is supposed to represent.

For the relationship between status and actual quality, this loose linkage originates primarily from four sources: (1) the necessary time lag between changes in the quality of a product and changes in consumer perceptions, (2) the stochastic nature of the link itself, (3) the nature, content, and extent of a producer's relations with others in the market, and (4) the second-order nature of status. Of these four factors, the time lag contributes least to complicating the relationship between actual and perceived quality. Were the time lag the only relevant factor, then the relationship between quality and perceptions could be easily established; the quality at some time t would perfectly determine perceptions at some time $t + 1$.

A second and more significant source of the decoupling is the fact that information diffusion is necessarily a stochastic process. Not every shift in quality of a given level will be detected, not every detected shift will be communicated to the same number of potential future users, and not every communication between users will occur at the same rate.

These first two factors, of course, are not unique to the relationship between status and quality, but are endemic to the link between nearly any signal and that which the signal is supposed to represent. Indeed, they are implied by the signaling framework in which quality must be unobservable for signals to be relevant to market actors' decisions. What at least partially distinguishes the signal of status is that the loose linkage between status and quality is mediated by a producer's ties to others in the market.

A producer's network of relations mediates the link between quality and status in two ways. First, the embeddedness of action in social relations prevents contact between a producer and consumer that could potentially change the latter's opinion of the former. If a low-status producer's good is not even considered a reasonable substitute for those perceived to be of high quality, purchasers of high-quality goods will most likely remain unaware of any changes in the good because of a lack of contact. Conversely, loyal purchasers of a high-status producer's product may not discern a relative decline in the quality of their preferred product if they do not compare it with the array of choices that confront them in the market. Such dynamics have been at work in the automobile industry. In the late 1960s and early 1970s, Japanese automobiles under-

went considerable improvements in quality, but most Americans did not consider these foreign cars as an option because they did not regard the imports to be of comparable quality to domestic automobiles. Only the exogenous shock of the oil embargo broke the inertia underlying the pattern of exchange relations in the market (Halberstam 1986). Throughout the late 1980s and early 1990s buying patterns have crystallized in a different way, with many Americans not even considering a domestic car as a credible alternative to Japanese imports.⁴ Thus, one way that social relations or social networks contribute to the linkage between status and quality is by serving as access constraints, inhibiting contacts which could potentially alter perceptions by bringing them into conformance with changes in the underlying quality of products.

Social relations also mediate between status and quality because status flows through the "interlinkages" between individuals and groups (Goode 1978; Blau [1964] 1989). Ties to higher-status actors enhance the prestige with which one is viewed, while ties to lower-status actors detract from it (Faulkner 1983). Accordingly, the formation and dissolution of social relations necessarily influence how the producer is perceived. In product markets, there are several types of ties that affect perceptions of a producer's status: exchange relations with consumers, ties to third parties associated with the market, and affiliations with other producers.

To the extent that buyers observe not only the products and actions of other buyers, the formation and dissolution of exchange relations with prominent customers has a strong "spillover" effect for a producer. Network-based studies of innovation adoption provide good examples of how a producer's relations with prominent buyers affect other buyers' perceptions of a product. Coleman, Katz, and Menzel (1957) and Burt (1987) have shown that an individual's propensity to adopt an innovation is influenced by whether or not prominent others in that individual's network have done the same.

Ties to third parties are relevant especially when producers and consumers do not meet directly in the market. In such markets, the distribution channels used by a producer can have a powerful effect on the perceived quality of the producer's product (Bonoma and Kosnik 1990).

Examples of interproducer ties relevant to status are joint ventures, individuals who depart from one firm to work for another, or common membership in trade associations (Benjamin 1992). In all three of these cases, status flows through the linkages between market actors in the manner described by Goode (1978). The transfer of individuals between firms is a particularly common conduit of status. By drawing a major

⁴ See "The Japanese Borrow Detroit's Favorite Ploy: Rebates," *Business Week* (June 17, 1991).

figure away from a highly respected competitor, a firm can improve its status. These status-enhancing effects represent an additional contribution to the acquiring firm beyond the human capital that it may gain from the transfer. A familiar example is the mobility of academics between institutions; drawing a prominent member from a highly respected department is a rather typical means for augmenting a department's status.

Thus, there are three types of ties that serve as intermediate signals of quality: ties to prominent buyers, ties to third parties, and ties to other producers. Although this relational component to status is not necessarily inconsistent with economic conceptions of reputation (e.g., Kreps and Wilson 1982), it is equally true that this relational component has not received explicit attention in such work.⁵ Of course, to the extent that the recipients of the producer's ties are concerned with their own status, the producer's ability to maintain ties to a particular actor will be inversely correlated with the producer's own quality. Accordingly, these ties to high-status actors are not components of product quality, but are to some degree signals within the larger signal of status. The existence of such ties thus does not eradicate the link between actual quality and status; rather, such ties simply serve to further blur or loosen the relation.

This discussion of ties as intermediate signals can be generalized to suggest a fourth and final reason for the loose linkage between quality and status. Because status is defined in terms of perceptions and because quality is, by definition, unobservable before the transaction, the perceptions through which status is constructed can only be indirectly based upon quality and are directly based upon other signals, of which a producer's network of relations may be only one. In an examination of status among *Fortune 500* corporations, Fombrun and Shanley (1990) observe several factors that seem to affect a corporation's status, including profits, total assets, charitable donations, and market share. Though their work is concerned more with status at the level of the interorganizational field than at the market level, it does nonetheless highlight the fact that status derives from other observables, which can themselves be interpreted as signals. Status, thus, may ultimately be a more multifaceted and encompassing signal than an attribute such as education or a product warranty insofar as it denotes a producer's position relative to its competitors; yet, at the same time, the linkage between status and quality is probably looser than that between quality and other signals.

Because of the loose linkage between status and quality, it becomes possible to draw a distinction between a producer and a producer's position in the market in much the same way that the distinction can be

⁵ The closest parallel in the economic literature seems to be the work of Montgomery (1991) on job search.

drawn in organizational contexts (Simmel 1950; White 1970; Sørensen 1983). If quality shifts were recognized immediately, then the status position would be inseparable from the present actions of the producer and, therefore, would not be analytically useful. However, the greater the decoupling, the more the status position insulates and circumscribes the producer's action and the more the producer's reputation becomes external to itself. In short, due to the loose linkage of quality and status, a niche emerges as a given constraint that the producer must confront in trying to decide upon an optimal course of action.

FROM STATUS POSITION TO STATUS ORDER

I listed above a variety of factors that can contribute to the loose linkage between status and quality. But not all of these need be present in a given context for the loose linkage to exist and thus for a producer's status position to manifest itself as a constraint. Like the assertion that consumers value status as an end in itself, certain claims and assumptions may be and perhaps even ought to be included in this discussion because they provide a more accurate¹ characterization of status dynamics in particular markets; these claims, however, are not necessary elements of the status-based model. To be clear, the only assumptions regarding producer quality and the relationship between producer quality and market status that are to be regarded as essential to the model are:

ASSUMPTION 1.—Producer quality is an unobservable prior to the consummation of a transaction.

ASSUMPTION 2.—Market status is a signal of quality on which consumers can and do rely for their decisions.

ASSUMPTION 3.—A producer's relations with others in the market mediates the relationship between status and quality by creating inertial tendencies in the formation of exchange relations and by biasing evaluation in the direction of the status of those to whom the producer is tied.

It will soon be apparent that, when coupled with the implicit behavioral assumption that producers are (boundedly rational) profit maximizers, these three assumptions form the core of the status-based model. Indeed, it should soon be clear that these assumptions are sufficient to derive the loose linkage between status and quality and, accordingly, the conception of the isolated status position.

One can analytically shift from the microlevel conception of isolated status positions to the macrolevel view of the market as a tangible status order if one then makes the additional assumption that market producers can be ordinally ranked along just one dimension. Such an assumption is not as restrictive as it may first appear. The validity of the assumption

is supported by the fact that buyers are in fact able to discriminate between producers. In order to choose between the various products in the market, buyers must implicitly assign cardinal weights to the various characteristics of products. If they were unable to combine separate characteristics into an assessment of each product's overall quality, then it is not clear how they could rationally select one product over another. Granted, different buyers may express different preferences, but these preferences can be combined such that they confront producers as one aggregate buyer with a set of weights that is simply the sum of individual consumer preferences (White 1981*a*; see also Berger and Fişek [1974] on the aggregation of status characteristics).

Moreover, this recognition of a status ordering is not incompatible with the fact that producers often divide the buyer side of the market into distinct geographical or demographic segments in which each segment has a different criterion for discrimination. The acknowledgment of a status ordering simply requires a redefinition of the market to reflect divisions on the demand side. If the market is divided, each segment can be treated as its own market and, within each segment, the significance of status can be examined. For example, Coser, Kadushin, and Powell (1982) observe that the publishing industry is divided into several distinct segments, such as trade publications, college texts, and scholarly works; in a more detailed analysis of the latter of these three segments, however, Powell (1985) notes a rather well-defined status order that is largely unique to that segment. If preferences systematically differ across segments, then a producer's status position with respect to one sector need not be the same as its position with respect to another. While the boundary questions raised by the acknowledgment of market segments may make the empirical analysis of a market more difficult, the existence of market segments does not raise any conceptual obstacles to the vision of a status order as defining the market.⁶

⁶ The model would only be inapplicable to multimarket firms if the existence of firm attributes, which either transcend individual markets or which derive from behavior in a market other than the one of interest, violate the second assumption that a producer's market status is a signal of (and accordingly correlated with) market quality. In other words, if a firm could ignore quality and relational concerns within the focal market and build and maintain status solely on the basis of activities or attributes that stand outside that market, then there would no longer be a loose linkage between market status and market quality but in fact a complete break. Ultimately, it is an empirical question as to whether or not this break exists for multimarket firms, making it especially important to study status processes in markets where such firms exist. Accordingly, the empirical analysis of this article will focus on one market—the investment grade debt market—in which the actors of interest—investment banks—have a presence in multiple markets. Yet, even before such an analysis, it is noteworthy that one can point to examples such as the publishing industry, where firms may

The loose linkage between actual quality and perceptions of individual producers means that the status order exists as a structural entity. In the extreme, positions persist even in the absence of an occupant. The saying "They don't make things like they used to" underscores the fact that consumers frequently evaluate goods not just with reference to the actual goods in the market, but with reference to their perceptions of past goods. Consumers remain aware of upper-end status positions that have been vacated because of the decline in the quality level of one or more producers.

A second consequence of the loose linkage between status and quality is that access to certain rewards in the market becomes entirely mediated by the position one occupies in the status order. Just as access to the highest salary in a firm is contingent upon occupying a position at the top of the organizational hierarchy, so access to the highest quality manuscripts in the academic book market, for example, is contingent upon occupying a position high in the status order.

THE SIGNIFICANCE OF STATUS

Having elaborated the basic definition of market status, I now turn to a discussion of how the constraints and opportunities presented by a status position affect the producer's gross revenue and costs. *For a producer of a given level of quality*, additional status is most likely to translate into increased revenue, either in the form of higher prices or greater market share. This claim follows from the view of status as something valued in itself and, more important, that status is a signal of quality (see assumption 2). As Veblen (1953) makes clear in his discussion of conspicuous consumption, higher status increases what people are willing to pay because of the power that a good provides in the social sphere. At the same time, to the extent that status serves as a signal that implicitly lowers the risk that the good is below a given quality threshold, individuals are also willing to pay more for the higher-status good.

The probable impact of status on costs is obscured by the fact that higher-status producers are generally of higher quality and higher-quality goods are often more costly to produce. Therefore, the zero-order relationship between status and costs is often positive. However, *if one controls for the quality of the good*, it follows from the view of status as a signal of quality that the effect of status on costs is negative. If consumers

occupy a high-status position in one market and a low-status position in another. Such different identities across markets seems to suggest that a multimarket presence does not give rise to an identity that completely transcends individual markets and thus does not eradicate the loose linkage between status and quality within a given market.

and relevant third parties to a transaction perceive status to be a signal of unobservable quality, then they will be more reluctant to enter into a transaction with a low-status producer than they would be with a high-status producer even if both claim to manufacture the same quality good and sell it for the same price. Empirically, this greater reluctance to accept the quality claims of lower-status producers manifests itself in several cost advantages for the higher-status producers.

First, for the higher-status producer, advertising costs for attracting a given volume of business are lower. More customers simply flow to the producer without the producer actively seeking them out, and often the higher-status producer receives "free advertising" that the lower-status producer is unable to obtain. Examples of this free advertising abound. Publications from highly regarded academic presses are more likely to receive reviews in academic journals than publications from less highly regarded presses (Powell 1985). Business journalists are more prone to ask the employees of prominent firms within a market to offer insights into market trends than they are to ask employees of a less prominent firm (e.g., Kadlec 1986).

More important, if the risk-averse consumer or relevant third parties—such as retailers—require "proof" that the product confronting them is of a given level of quality, status lowers the transaction costs associated with the exchange between buyer and seller. Implicit and explicit promises of a higher-status producer regarding product quality are more likely to be accepted; therefore, the higher-status producer need not devote as much time or expense to convincing the buyer or relevant third parties of the validity of its claims.⁷

A particularly clear example of this inverse relationship between status and transaction costs was provided in an interview with the head of a middle-sized investment banking firm about procedures for underwriting. In primary securities markets, investment banks underwrite the security issues of corporations and political entities that desire to raise capital. In other words, banks assume the risk of buying new security issues from companies or governmental agencies and publicly or privately

⁷ Recently, Williamson (1991) has attempted to incorporate status-related concerns into the transaction cost framework by conceptualizing reputation as a shift parameter that lowers the transaction costs associated with conducting market exchanges. Williamson argues that in markets where actors are not anonymous, the concern with reputation will bolster actors' ex post commitments to ex ante promises. While Williamson's conception of reputation as something that is more or less present in certain markets is different from my conception of status as a property differentially distributed across producers within a market, it is noteworthy that the inverse relationship between reputation and transaction costs is similar to the inverse relationship between status and transaction costs laid out here.

reselling them to investors. Despite intense competition among investment banks for the opportunity to lead a security offering, often an investment bank does not place the entire offering itself. Rather, it forms and leads a syndicate of banks.

When asked about the advantages of status, the executive replied:

Typically, if you hear that Goldman Sachs or Salomon or whatever is doing an underwriting, they usually have pretty stringent requirements, and it is usually a plus for the company that they are doing work for that Goldman Sachs wants to be their investment banker or underwriter or whatever, [that is] a plus with reference to the market place. Half the time, if Goldman Sachs calls or Solomon calls us and says [they] are going to be an underwriter for Ford Motor or whatever and asks, "Do you want to be part of the underwriting group?" we almost don't have to do any diligence; you just say yes. On the other hand, if a smaller firm which just doesn't have the credentials calls us, we will probably do more diligence and will probably be less likely to follow suit.⁸

A third type of costs lowered by status are financial costs. Fombrun and Shanley (1990) note how status enhances a firm's ability to obtain capital from either commercial banks or from issuing securities in the financial markets. The terms for acquiring credit significantly favor the higher-status firms.

These advantages in advertising, transaction costs, and financial cost, which accrue from status, all derive solely from the view of status as a signal that reduces the reluctance of market participants to enter into an exchange relationship with a particular producer. However, if one is willing to draw on Frank (1985) and make the additional and arguably very realistic assumption that employees are willing to accept lower monetary compensation in exchange for higher status, then one can specify a fourth source of lower costs. If an employee does indeed value the status of her workplace, she should be willing to accept a lower wage or salary to work for a higher-status firm than for a lower-status one. Of course, a higher-status firm may actually offer higher salaries than lower-status competitors because it wishes to have employees (perceived to be) of higher quality. However, controlling for the (perceived) quality of the

⁸ Despite the competition among investment banks to lead an offering, the bank frequently does not underwrite the offering itself. Rather, it forms and leads a syndicate to distribute some of the risk. In forming the syndicate, the lead manager, along with the issuer, may participate in what could be quite a number of "due-diligence" meetings, where syndicate members "kick the tires" of the corporation to assess the viability of the offering. They hold these meetings as a way of maintaining financial responsibility to investors.

potential employee, the higher-status firm should be able to acquire the individual at a lower cost.⁹

In short, the consideration of these four factors suggests the following: given two producers at a particular point in time, the costs for a given quality output will be lower for the higher-status producer than the lower-status producer as long as the second core assumption—that status is indeed a signal of quality—is valid.

The inverse relationship between status and costs is in some sense the economic flip side of the argument that social networks represent access restraints that inhibit shifts in opinion. Were the demand for high-status goods completely inelastic, then the low-status producer could do nothing to overcome the access restriction. There is no amount of advertising or no size of warranty that the producer could offer to break the established buying patterns and establish the contact that would be necessary to alter perceptions. The cost difference for manufacturing and distributing a given quality good would for all practical purposes be infinite. However, except in the extreme case of complete inelasticity, the differential access of producers to purchasers of high-quality goods means simply that it will be more costly to manufacture and distribute a given volume of the same quality good.

Though status strongly influences both revenue and costs, most of my discussion has focused on its cost-related benefits. The reason for this focus is not only that the cost-related benefits are less intuitive than those pertaining to revenue, but that the cost-related benefits can actually be of greater significance to the high-status producer. These cost-related benefits afford the producer insulation from the competitive pressure of lower-status producers even in the context of intense price competition, as is perhaps best demonstrated by Stevens's (1991) account of competition among the "Big Six" accounting firms for the audits of major for-profit and nonprofit corporations. While price competition among the six highest-status accounting firms often drove competing bids for the business of the most significant clients to the range of costs and thus effectively eliminated most if not all positive rents from status, the lower-status firms were still unable to compete for the audit opportunities in the high-status niche. In one instance, Stevens details how SBO Seidman, a sec-

⁹ While highlighting Frank's (1985) observation that individuals are willing to exchange money for status, I wish to distinguish my view from Frank's analysis of intrafirm differences in compensation. Frank argues that, because individuals value status, those in a higher position in the firm are willing to accept less than their marginal productivity. Conversely, those in the lower-status position demand a salary or wage in excess of their marginal productivity. In contrast to Frank, I perceive the individual's "pond" not as a single firm, but as either all firms or a subset of firms within the market.

ond-tier accounting firm beneath the so-called Big-Six, attempted to compete for the audit of a major charitable organization. Even though SBO Seidman was fully capable of performing the audit, its request to present a bid to the corporation was denied. Only the Big Six were invited. As Stevens comments, "The charity's selection committee limited its competition to the Big Six not because the Big Six stand for superior professional standards . . . , but because the world has stamped a 'Good Housekeeping Seal of Approval' on their audits" (1991, p. 237). In effect, SBO Seidman could not pay transaction costs high enough to compensate for the status differential between it and the highest-status firms.

THE MATTHEW EFFECT AND THE CONSTRAINTS OF STATUS

This observation that status lowers the cost of producing and selling a good of a given quality has several implications. First, it rearranges the relationship between costs, signals, and quality initially posited by Spence (1974). Recall that, according to Spence, the marginal cost of a signal is by definition inversely associated with quality. However, at least for status, it is the marginal cost of quality that is inversely associated with the existence of the signal. The greater one's status, the more profitable it is to produce a good of a given quality. More simply put, whereas the economic view of signals begins with differences in quality between producers and then derives as signals those attributes for which the marginal cost of the signal is greater for the low-quality producer than the high-quality producer, the sociological view takes as its point of departure the reality of the signal and then derives the differences in quality on the basis of who possesses the signal and who does not. Both components are obviously important, but the substantive implication of the sociological view is that not only does actual quality determine perceived quality, but the latter has a reciprocal effect on the former. Because the costs and returns for investment in quality are differentially distributed across producers, the firms in a market have dissimilar incentives to make this investment.

A second implication of the status-cost relationship is the operation in markets of what Merton (1968) termed the Matthew effect. This phrase derives from the first book of the New Testament, which contains the line: "For unto everyone that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even what he hath." Merton applied the expression to the considerable discrepancy in esteem accorded high- and low-status scientists for similar accomplishments. For example, the likelihood that an article will be widely read and cited is positively correlated with the author's status. More generally, however, the Matthew effect refers to the fact that higher-status actors

TABLE 1

INTERYEAR CORRELATIONS IN THE DEBT MARKETS, 1982-87

| Correlations | Investment Grade Debt | Non-Investment-Grade ("Junk") Debt |
|--------------------|-----------------------|------------------------------------|
| Coefficients | .91* | .87* |
| <i>N</i> | 191 | 393 |

SOURCE—Securities Corporation Data Base.

* $P = .0001$.

obtain greater recognition and rewards for performing a given task and lower-status actors receive correspondingly less. The term has been applied to a diverse set of social phenomena, such as education (Walberg and Tsai 1983), intraorganizational power (Kanter 1977), and the life course (Dannefer 1987).

The cost and revenue implications of status reveal that the phenomenon is equally applicable to markets. Just as the likelihood that an article will be read and cited is positively correlated with its author's status, so the recognition and rewards that accrue to a higher-status producer for manufacturing a good of a given level of quality is greater. The nonproducing market participants expect that the high-status good is of superior quality and that the low-status good is the opposite. These differing expectations create dissimilar returns on investment for manufacturing a given product that greatly favor the higher-status producer.¹⁰

The apparent applicability of the Matthew effect raises the important question of why one or a subset of the highest-status producers do not dominate the market. For example, we have noted that, in the primary securities markets, the higher-status banks have lower transaction costs for issuing a security of a given quality. However, despite the fact they have lower transaction costs, the higher-status banks do not dominate the market. Table 1, which is based on data from the Securities Data Corporation (SDC) data base, lists the correlation of market share between year t and $t - 1$ for the years 1982-87 in two of the primary securities markets: the market for investment grade debt and the market for non-investment-grade debt. In the former market, the correlation is .91; the correlation in the later is .88. Figure 1 shows the macrolevel consequences by depicting the Herfindahl indices for these two markets

¹⁰ One consequence of this fact is that a high-status producer's position is invariably its own to lose. A change in the status order depends at least as much on poor performance from those at the top as the exceptional performance of those on the bottom; this has been most convincingly demonstrated by the shift in positions occupied by automobile manufacturers in the 1970s (Halberstam 1986).

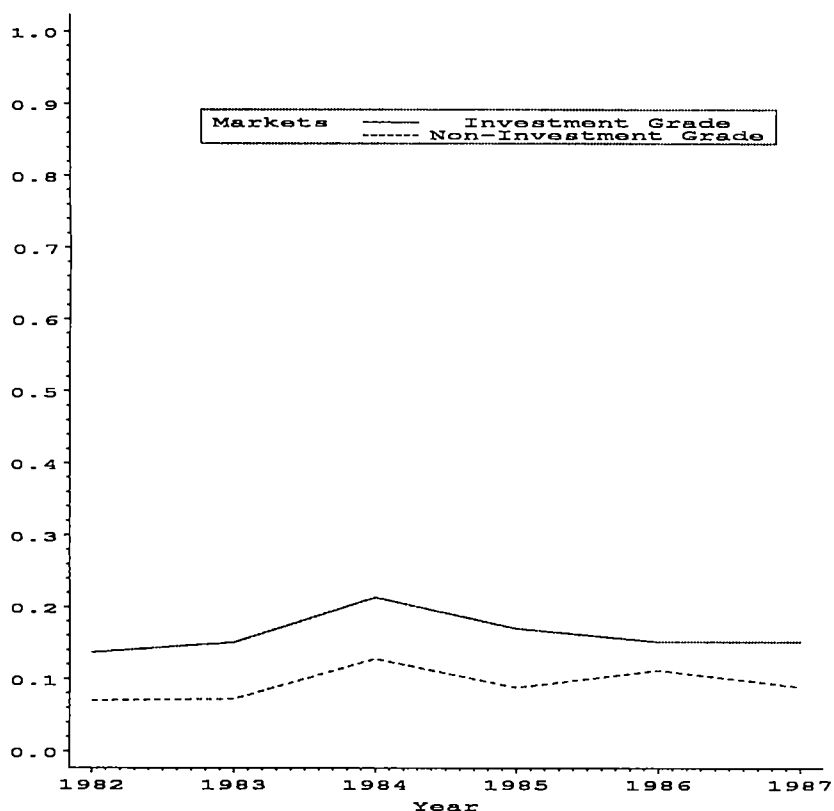


FIG. 1.—Herfindahl indices for debt markets

over this period. The measure approaches zero as the market nears a perfectly competitive situation with an infinite number of producers possessing an infinitely small market share. The measure approaches one as the market becomes a perfect monopoly. Except for a rise in concentration in 1984, the index reveals no consistent trend toward increasing concentration in either market.

Price theory offers only one possible theoretical reason why the higher-status firms would not corner the market if they could command a higher revenue and lower costs across all levels of quality. If the higher-status firms exhibit long-run diseconomies of scale as they expand into the market such that price per unit rises above the market value of the good, then there may be constraints on the expansion of the high-status producer. Possible sources of long-run diseconomies of scale are inherent

limitations on a factor of production or loss of managerial control. However, one of the earliest results of the industrial organizations literature was the lack of evidence for long-run diseconomies of scale with respect to production costs (Bain 1956; Johnston 1960). Therefore, at least within the production ranges of these studies, there is no empirical grounding for the assertion that limitations on the high-status producers emerge from diseconomies of scale.¹¹

As a result, it is difficult to explain in light of price theory why the second half of the quote from Matthew cannot be taken more literally, why "from him that hath not shall be taken away even what he hath." If a higher-status producer can manufacture a given quality good at a lower cost and even command a potentially higher price, what prevents the higher-status producer from completely dominating the market either through underbidding the lower-status producers at all quality ranges or taking over the lower-status producer's operation and attaching its name to the operation? To resolve the apparent anomaly, it is necessary to reconsider the third core assumption of the status-based model, which specifies the relational bases of status.

Since the relationship between actual and perceived quality is mediated by the producer's ties to others in the market, the producer invariably changes how it is perceived if it broadens relations with others in the market. Even relations that may be only indirectly connected to the actual quality of the product have a similar effect. As noted earlier, ties to buyers, third parties, and even other producers all affect how one is perceived, and status necessarily implies a certain exclusivity in the formation of exchange relations (Goode 1978). Even if there is no differentiation in the perceived quality of the actors to which producers have ties, the same dynamics apply. The only difference is that status will depend more on the number than on the identity of these other actors. As White (1981a) observes, a producer's volume affects how common its product is perceived to be in comparison to others.

If the reputation of the highest-status actor declines to the point where its status is just equal or below that of its nearest competitor, its niche in the status order becomes vacant, opening the opportunity for this previously lower-ranked actor to display more selective standards and thus fill the vacated niche. The lower-ranked producer no longer faces a

¹¹ It is important to distinguish diseconomies of scale from growth within a particular market from diseconomies of scope, which might accrue from growth through mergers across markets. The dismantling of corporations in the 1980s has been taken by some as evidence that there are managerial inefficiencies that result from combining firms producing in separate markets. However, this trend does not call into question the basic conclusion from Bain (1956) and Johnston (1960) that within a particular market, there is no evidence for diseconomies of scale.

relative disadvantage in competing for business in the higher-status niche. Rather than being driven from the market, it has the opportunity to occupy the now-vacant position of what was the higher-status producer. Or, if the lower-ranked producer does not attempt to move into the niche, the opportunity is available for an entrepreneur in the wings to do the same. To the extent that a higher-status producer attempts to expand into the position of a lower-status competitor, it changes its reputation and thus alters the cost-and-revenue profile that provided it with the initial advantage. As a result, just as status processes help reproduce inequality by constraining those at the bottom of the status hierarchy, so status processes also place limits on the higher-status producer's expansion into the lower end of the market.

Recognition of this fact leads producers to construct different identities to the extent that they wish to compete in different ends of the market. For example, Hart, Schaffner, and Marx, the nation's leading manufacturer of branded men's tailored clothing in 1980, sold suits under three different labels, each confined to a specific price range (Tedlow 1982). In constructing separate identities, a producer forgoes any short-run advantage in costs or revenue that would accrue to the lower-status product from its association with the higher-status product. The producer realizes that ultimately such an association tarnishes the image of the high-status product as much as it might improve the image of the low-status product. This lower status in turn leads to less net revenue and hinders the degree to which the firm can profitably invest in high quality. Expansion, therefore, requires that the firm enter the market as two distinct actors. It should be noted, however, that this type of expansion does not represent a "solution" to the constraint imposed on the high-status actor since this actor does not derive any competitive advantages from status in the low-quality end of the market. It faces basically the same cost-and-revenue profile as the other low-status actors when manufacturing the low-quality product, since its low-end product is perceived to be identical to the rest; it perhaps derives cost advantages in the low end of the market from improved economies of scale, but not from status.

It is, of course, possible that the producer may be able to quietly alter its quality without strongly influencing market perceptions. Implicit in the idea of loose linkage is the fact that the causal connections between actual quality, market relations, and perceived quality are not completely determinate. However, the recognition that reputation may not diminish is in fact indicative of the general problem faced by the producer seeking to alter its niche. To change positions is to immerse one's production decisions in the vagaries surrounding the relationship between actual and perceived quality. Instead of optimizing within a given cost-and-revenue profile of which one is fairly cognizant, one instead opens oneself up to

high uncertainty about these profiles because one cannot predict how perceptions on the demand side and actions on the producer side will be affected by a shift in quality.

By cultivating a distinct position or identity in the status order, the producer reduces the unpredictability that confronts the nonproducing market participants' selections of goods. While quality will always remain an unobservable before the consummation of a transaction, a distinct reputation nonetheless constitutes a tangible signal by which consumers can compare producers. At the same time, the occupancy of a distinct status position reduces the unpredictability confronting the producer. An awareness of its own position in the market allows the producer to minimize mistaken production decisions. Even lower-status producers have an incentive to reproduce the status order to the extent that it allows for a minimization of such mistakes.

As Leifer and White (1987) observe, rational producers are aware that their success in a market is a function of their distinct identity, and the reproduction of this identity is a fundamental principle guiding market behavior. Status, thus, becomes an important explanatory variable in understanding the stable inequality of markets in light of the Matthew effect. Status, or, more accurately, the loose linkage between status and actual quality, constrains the profitability of invading either a lower or higher niche.

In emphasizing the features of the market that are conducive to reproducibility, the model developed here is structural, as the term has been classically applied in both sociology and anthropology. Yet, such structural analyses are necessarily incomplete. I ignore such questions as the provocative one raised by White (1981b): "Where do markets come from?" It seems clear that the same stress on reproducibility cannot apply prior to the existence of a tangible structure. I also ignore the dynamics of mobility that occur even after the market structure is established. Even though most markets are stable in the sense that a producer's position in the market one year is invariably a good predictor of its position in the next, mobility clearly occurs. Though all producers benefit from the existence of the status order, all do not benefit equally. Hence, there is reason to believe that lower-status producers may either attempt to enhance their mobility within the status order or change the status order entirely, even if doing so exposes them to vagaries and uncertainties that they would not otherwise have to face. The model is far from a complete account as it does not directly address such dynamic issues. However, even as it stands, the model can be defended on the grounds that it provides a relatively well defined sociological lens with which to view market phenomena.

At a general level, the theoretical framework provides insights into many features of real-world markets that are either unexamined by or in tension with neoclassical theory. The sustained inequality of positions in markets in light of the fact that higher-status producers can make a given quality good at a lower cost is probably the most important, but there are others. For example, Ijiri and Simon (1977) observe that most producers halt production volume at a point before marginal costs begin to rise beyond price. Such an observation is clearly in tension with the central predictions of neoclassical theory, but it is quite compatible with the notion that status positions provide severe constraints on production decisions. Recognizing that profitability is bound to identity, producers halt production before it reaches a level that threatens that identity.

Another important observation in apparent tension with conventional economic theory is one made by Buzzell and Gale (1987). In a comparative study of firms across markets, they find that returns on investment are positively associated with perceived quality. Such an observation violates neoclassical economic theory's prediction that profits should be driven to zero over the long run. If profits are higher in the higher-quality ranges, then more producers should enter those quality ranges until profits become identical across the full quality spectrum.

In contrast, such a relationship is not only consistent with the status-based model, it is a characteristic of markets that the status-based model predicts *must be present* in a stable market if one assumes (as I do) that producers are indeed profit maximizers. Otherwise, the higher-status producers would constantly be tempted to "cash in" their status and seek to cultivate a lower-status niche.¹²

Nevertheless, in order to develop more substantial support for this particular alternative to the neoclassical view, I move from the general to the particular: an examination of pricing dynamics in the primary securities markets. While it is of course difficult to generalize on the basis of any one case, it is worth noting that there are several features of the investment banking context that would seem to minimize the significance of status processes. Hence, if the relevance of status can be demonstrated in this context, we may presume that it should be relevant in other contexts as well.

¹² This observation leads to the hypothesis that markets in which status is positively correlated with profits will be more stable than markets in which status is inversely correlated with profits. For the purposes of testing such a hypothesis, stability can be defined in terms of factors such as shifting market share, changes in relative product quality, or differences in buying patterns.

INVESTMENT BANKING

As underwriters of securities in primary securities markets, banks enter into relations with three sets of exchange partners—issuers, investors, and competing banks. Here, I will focus primarily on the exchange relationship between issuer and bank, though it is not possible to consider this relation in complete isolation from the others. The service that the bank sells to the issuer is the ability to effectively price and place the security at terms as favorable as possible to the issuer and to “make a market” for a given issue.¹³ Placement ability is contingent upon the extent of its connections to investors and often to other banks, which are willing and able to join a syndicate to distribute the security. With strong and varied connections to these two groups, the bank is better able to gauge supply and demand and thus price and place the offering more effectively. In short, what the bank “produces” as underwriter is a mobilized syndicate of banks and an array of investors willing to purchase the security.

The “price” that an investment bank charges a corporation for underwriting a security is called the gross spread. The gross spread is the difference between the dollar value that a corporation pays an investment bank for the offering and the dollar value at which the bank resells the offering to the market.

There are two broad classes of securities that banks underwrite in the primary securities markets: equity and debt. Equity, which is alternatively referred to as stock, represents an ownership stake in the corporation. Debt, of which the most common type is a bond, constitutes a legally binding obligation of the issuer to pay the holder of the debt a sum of money at clearly demarcated points in time.

Relying on divisions that are frequently made in the trade publications (e.g., *Investment Dealers' Digest* [IDD]), it is possible to further divide these broad categories of securities into different markets. For example, one special type of equity market is the market for initial public offerings. An initial public offering, or IPO, is a company's first distribution of stock to the public. There are two major corporate debt markets: the market for investment grade debt and the market for non-investment-grade debt. What distinguishes investment grade from non-investment-grade debt is the financial history and soundness of the issuing firm.

The analysis here will focus on the market for investment grade debt. Unlike equity issues, debt issues are evaluated by major ratings agencies, and the ratings provide a strong guideline for the price at which the bond

¹³ To “make a market” is to announce a bid price at which it will buy the security and an ask price at which it will sell the security.

is offered to investors. Moreover, corporate bonds are almost exclusively purchased by institutional investors and are accordingly less frequently traded in secondary markets than equity issues. Because of the pricing guidelines and the comparatively small need for making a market on a given issue, there is comparatively little room for banks to distinguish themselves in issuing debt.

In comparison to non-investment-grade debt, the underwriting of investment grade debt would seem to be especially insulated from status concerns because of the low probability of default that defines investment grade issuers. This type of security is colloquially referred to as "vanilla debt," a label that reflects the lack of complications involved in underwriting the issue. One ex-Shearson broker commented to me that he personally could successfully execute a typical investment grade issue even after he had left the firm. All such an issue would require would be a few phone calls to major institutional investors.

One should not take this somewhat facetious comment to mean that there is *no difficulty* involved in the placement of investment grade debt. Particularly as issues get larger, the challenge of placement becomes greater and requires a more extensive knowledge of and connections to the demand side of the market. Two bankers associated with a much smaller firm than Shearson noted the size of an issue as a reason why their firm would be neither willing nor able to underwrite a given issue. Nevertheless, apart from the factor of size, which is relevant in any of the primary securities markets, the comment is illustrative of the low level of difficulty that bankers attribute to underwriting in this particular market.

Finally, the importance of price in an issuer's selection of an investment bank, especially in the market for investment grade debt, can probably not be overstated. Eccles and Crane (1988) note that the phrase "Loyalty is a basis point" was particularly common among investment bankers in the mid-1980s. A basis point is .01% of the value of the offering. Though the phrase was probably an exaggeration, it reflected the bankers' strong belief that price (i.e., spread) was an extremely important factor in the exchange relationship between issuer and underwriter. An issuer would switch investment banks if it could find a slightly lower price in the market. Such a strong preoccupation with price would seem to imply that status exerts a minimal effect upon the market decisions of issuers and thereby provides some justification for the primary securities markets as a difficult test case for the status-based model.

To frame this assessment in terms of the three assumptions critical to the status-based model of market competition, this context is a challenging one because it is at best only weakly consistent with the assumption that quality is an unobservable; it is implicit, therefore, that the context

is only weakly consistent with the assumption that consumers (in this case, the issuers) should use status as a signal of quality. To balance the assessment, it is also a market in which interpersonal and interorganizational networks are critical to doing business (Eccles and Crane 1988; Baker 1990). This network nature of investment banking has two consequences. First, it means that the third assumption—that social relations mediate between status and quality—is more easily met in investment banking than in markets where social relations are less critical. Second, the significant role of interpersonal and interorganizational networks in conducting transactions opens the possibility for transaction costs to play a larger role in outcomes than they otherwise would, and to the extent that the advantages of status are contingent upon reductions in transaction costs, status can play a larger role in this market than one in which there are effectively no transaction costs. Thus, there are clearly aspects of the case that are conducive to the importance of status processes, though, on balance, there seem to be a sufficient number of countervailing factors to make this a challenging case for the basic model.

DATA

Data for an examination of the investment grade debt market are drawn from the SDC data base between 1982 and 1987.¹⁴ These data contain extensive information on all of the corporate security offerings underwritten by investment banks over that period. In particular, for each issue, data are available on the type of offering, type of registration, spread, volume, bond rating, and the lead manager and comanagers. The primary purchasers of the SDC data are the investment banks themselves, who use the data mostly to assess their share of the market and their penetration into particular industrial sectors.

THE DYNAMICS OF THE PRICING MECHANISM

The status-based model draws our attention first and foremost to the role of costs and price in sustaining the hierarchical pattern of exchange relations in the market. Because the higher-status producer can manufacture a good of a given quality at a lower cost, it can effectively underbid the lower-status producer seeking to enter the higher-quality niche.

As has been repeatedly emphasized, price is very important in the issuer's selection of an investment bank in the investment grade market. Intense price competition necessarily implies no revenue advantages (i.e.,

¹⁴ These data were graciously made available to me by Robert Eccles and Dwight Crane.

positive rents) from high status on a given transaction. If issuers are choosing primarily on the basis of price, then the banks should not be able to command a premium for status. Even if issuers would prefer an extremely high-status firm to a comparatively low-status firm, they may be indifferent between those five or six banks at the top of the hierarchy, leading these top banks to compete among themselves on the basis of price in much the same way that the highest-status accounting firms competed with one another in an earlier example.

However, the lack of benefits on the revenue side does not preclude benefits on the cost side. As previously noted, higher status leads to lower transaction costs in forming syndicate and investor relations. The reputation for having stringent requirements means that it is less difficult and less costly for a bank to lead a given offering, and there is some limited qualitative evidence from the industry, such as that regarding Merrill Lynch (e.g., Kadlec 1986), that a higher-status firm can retain an employee of a given level of quality at a more favorable compensation arrangement for the firm. The Matthew effect, therefore, manifests itself in the investment grade market primarily in the form of low transaction costs and perhaps in the form of lower salaries as well, while having little or no effect on revenue.

Given the minimal impact of status on revenue but the advantages on cost, I hypothesize that the price that an investment bank receives for underwriting a given issue should be inversely related to status. In the bidding context in which banks confront issuers, the higher-status banks should take advantage of their lower cost to underbid their competitors for the bonds that they wish to underwrite.

To clarify this hypothesis, it is helpful to refer to a hypothetical scenario. Assume that there are only four investment banks in the industry that compete with one another for every issue. Assume further that the banks are aware of each others' costs (though we will drop this assumption momentarily). Figure 2 depicts such a situation. The vertical axis denotes increasing status; the horizontal axis indicates dollar values expressed in some arbitrary unit. The horizontal line for each bank represents its costs. If a bank successfully bids a dollar amount that falls to the right of the point where the horizontal line ends, then it earns a profit. Thus, if bank 4 makes a bid at D, it earns a profit; an expected bid at C, however, would result in losses. In a situation where all four banks desire the deal, bank 1 can practically guarantee that it will win the deal by issuing a bid at point A, just below bank 2's costs. Another firm will be able to win the deal only if it takes a loss.

Assume now the four banks are considering a second deal. Bank 1 decides that it is not worth the investment, but the other three banks desire the deal. In this case, bank 2 bids at point B and wins the deal to

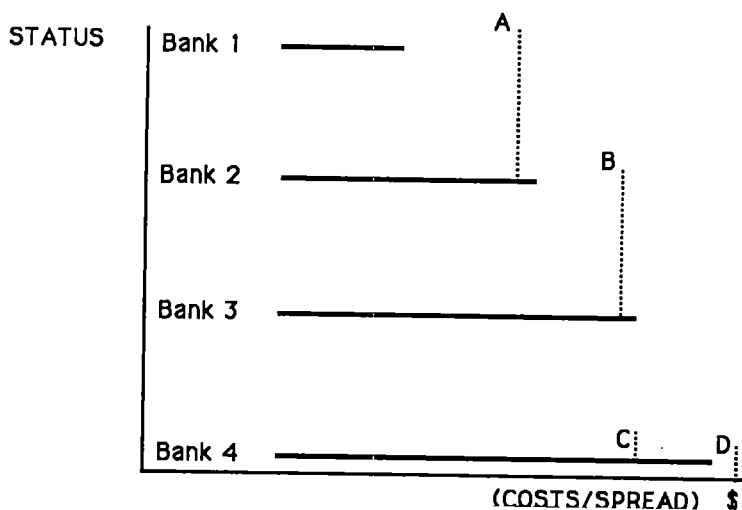


FIG. 2.—Hypothetical scenario of four banks

the extent that other firms are not willing to take a loss. If we make the reasonable assumption that firms cannot consistently take losses on their offerings, then this competitive situation in the context of a different cost structure results in the observed negative relationship between status and spread.

Dropping the assumption that producers are aware of each other's costs does not affect the expected relationship between quality and price. The only consequence of uncertainty about competitors' costs is that producers will make bids closer to their own costs and anticipated risk. Since they cannot be certain about the degree to which they are able to undercut their competitors' bids, they are forced, if they truly want the offering, to lower their bids to compensate for this uncertainty.

If bank 1 tries to corner the market by taking advantage of its lower costs, it will cease to have the reputation for "stringent requirements." Its costs will necessarily increase since it will have greater difficulty obtaining investors and syndicate members for a given quality of security. If these costs rise to the point that bank 1's costs are higher than bank 2's, bank 1 essentially loses the highest-status position in the market and its ability to outbid 2 for future business.

Despite the fact that the actual industry is more complicated than this model, the same dynamics should apply. One major conceptual difference between the imaginary four-bank market and the actual market is that there are probably several groups of banks that are of roughly equivalent status and therefore have essentially identical cost profiles. The five

or six highest-status firms presumably all face similar costs for performing a given issue. However, the only consequence of this similarity is to drive the banks of equal status to bid as closely as possible to costs when competing against one another. The negative relationship between status and price remains. Another difference is that not every bank is asked to bid on a given issue. But, again, this should not change the fact that of those bidding, the highest-status bank should be able to win the deal by just underbidding the competitor closest in status.

It should be underscored that the status-based model does not predict that price will be negatively associated with status in all markets. Rather, it predicts only that the costs for producing a given quality product are negatively associated with status. The reason that we expect the negative relationship between price and status in the investment grade market is that the advantages of status are primarily on the cost side. However, this situation is simply a special case of a more general relationship that can be easily represented with minimal formalism. If p_h and p_l are the prices charged by a high-status producer and a low-status producer, s_h and s_l are the status of the two producers, and Θ is the premium that a buyer is willing to pay for each unit increment in status, then the pricing mechanism reproduces the hierarchical ordering in the market to the extent that

$$p_h < p_l + \Theta(s_h - s_l). \quad (1)$$

If advantages are strictly on the cost side, then Θ equals zero, and the inequality reduces to $p_h < p_l$.

ANALYSIS

For the purposes of this analysis, I use percentage spread as the dependent variable to denote the price charged by an investment bank. The percentage spread is the gross spread divided by the dollar amount of the offering. Use of percentage spread rather than gross spread allows for greater comparability across issues. Of the 3,541 investment grade offerings listed in the SDC data base between 1982 and 1987, information is available on percentage spread for 2,782 of the issues. Careful inspection of the data suggests that the pattern of missing information is not random; the likelihood that data are missing is frequently correlated with the size of the issue and the revenue of the issuer. Because of this significant missing data on the dependent variable, selectivity bias is a potential danger in the analysis. Following Berk (1983), I correct for selectivity bias via a two-stage procedure. Using a dichotomous logistic model, I construct a "selection equation," which estimates the probability that an

observation has information on spread. Then I incorporate the predicted probability (PREDPROB) as a regressor in the main equation.

The independent variable of greatest theoretical significance is, of course, status. My measure of status derives from what are called "tombstone advertisements." Tombstone ads are the announcements in major financial papers and trade journals that list the issuer, content, and syndicate members of a given security offering. Figure 3 depicts an example of a tombstone ad drawn from the February 19, 1985, issue of *IDD*. At the top of the advertisement is the name of the issuer and the information about the issue. The lead manager of the issue is always the first bank to be listed, followed by one or several comanagers. In figure 3, the lead manager is Salomon Brothers, Inc. The comanager is Merrill Lynch Capital Markets. The rest of the banks are syndicate members.

Tombstone ads have been in existence since before the turn of the century. Before the emergence of an electronically integrated market, these ads presumably did serve a conventional advertising purpose of informing investors of the existence of the security. However, they no longer serve any such function. They most often appear in publications one or two days after a security has been issued on the market, while the vast majority of institutional investors have been aware of the offering since minutes after it was released for issue.

While the lead management and comanagement positions are highly coveted because they are the highest-status positions on a tombstone, occupancy of these positions does not necessarily mean that a bank is of higher status than all those that appear below. Higher-status banks may agree to join a syndicate that is managed or comanaged by an equal or lower-status bank.

However, in agreeing to be part of the syndicate, banks are extremely conscious of the status ordering within that syndicate. Syndicate banks are arranged hierarchically into what are called brackets; the higher brackets are more prestigious. Like the number of banks, the number of brackets will vary from offering to offering; the quantity can be as small as 1 and not infrequently as large as 9 or 10. Within each bracket, banks are listed alphabetically; there are, therefore, no status distinctions within brackets, only across brackets. In figure 3, the first bracket begins with the First Boston Corporation and concludes with Morgan Stanley; the second bracket begins with ABD Securities and concludes with Dean Witter Reynolds.

Notably, if the lead manager places the bank in a lower bracket than the bank believes is appropriate, the bank will withdraw from the syndicate. Conversely, if the bank is placed higher than is considered proper, members of the syndicate who have been improperly placed below the bank will withdraw from the offering. A particularly prominent example

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Merrill Lynch Capital Markets

The First Boston Corporation

Goldman, Sachs & Co.

Lehman Brothers
Shearson Lehman/American Express Inc.

Morgan Stanley & Co.
Incorporated

ABD Securities Corporation

Bear, Stearns & Co.

Alex. Brown & Sons
Incorporated

Deutsche Bank Capital
Corporation

Dillon, Read & Co. Inc.

Donaldson, Lufkin & Jenrette
Securities Corporation

Drexel Burnham Lambert
Incorporated

EuroPartners Securities Corporation

E. F. Hutton & Company Inc.

Kidder, Peabody & Co.
Incorporated

Lazard Frères & Co.

PaineWebber
Incorporated

Prudential-Bache
Securities

L. F. Rothschild, Unterberg, Towbin

Smith Barney, Harris Upham & Co.
Incorporated

Swiss Bank Corporation International
Securities Inc.

UBS Securities Inc.

Wertheim & Co., Inc.

Dean Witter Reynolds Inc.

American Securities Corporation

Daiwa Securities America Inc.

A.G. Edwards & Sons, Inc.

Interstate Securities Corporation

McDonald & Company
Securities, Inc.

Moseley, Hallgarten, Estabrook & Weeden Inc.

The Nikko Securities Co.
International, Inc.

Nomura Securities International, Inc.

Thomson McKinnon Securities Inc.

Tucker, Anthony & R. L. Day, Inc.

Yamachi International (America), Inc.

FIG. 3.—Example of a tombstone advertisement

of the latter occurred in 1987 on a \$2.4 billion bond offering by the Farmers Home Administration; 10 banks withdrew from the offering when 13 regional and small minority-owned firms were listed before them on the tombstone (Eccles and Crane 1988).

Major shifts in bracket position are rare events. One is not likely to observe more than one or two shifts in the higher-bracket positions over any given five-year period. Informal observation of the tombstones over the 1980s seems consistent with this claim.

Because syndicate position is such a close reflection of a bank's status in the industry, it has been used as a measure of status in other scholarly work (e.g., Carter and Manaster 1990) and is an appropriate measure for the purposes of this paper. The status scores for selected banks in the investment grade market are listed in table 2. Details of how the status scores for the banks were derived from the tombstone ads and further justification of their use are presented in the Appendix.¹⁵

To assess the effects of status on percentage spread, it is necessary to include several control variables in addition to PREDPROB, the predicted probability of inclusion in the sample derived from the selection equation. One of these is the size of the offering, AMT, which is measured in terms of logged dollars. Though it is more difficult to underwrite a large offering than a small offering, the marginal difficulty of underwriting each additional dollar increment decreases with the size of the offering. As a result, it seems reasonable to expect that the size of the offering should have a negative effect on the percentage spread. In the investment grade debt market, the rating of the bond is also an important determinant of spreads. A higher rating implies lower risk, which results in a lower spread. *Standard and Poors'* ratings for investment grade debt range from AAA to BBB. Rating information is available for all offerings between 1982 and 1986. For these years, dummy variables are constructed for all ratings above BBB+, with BBB+ and below forming a residual category; an additional dummy variable, SPRATMIS, was coded "1" for all offerings in 1987 and "0" for offerings in the other years.

I also control for the recent joint transaction history of the bank and company in all product markets. It seems reasonable to expect that prior or concurrent transactions with a bank will lower the spread due to "client-specific economies." Superior information about an issuer due to prior transactions should allow the bank to underwrite an offering at a lower cost. Two dummy variables, LSIMUL and LHIST1, account for

¹⁵ A full listing of the status scores for all the banks is available from the author upon request.

TABLE 2
SELECTED STATUS SCORES IN THE INVESTMENT GRADE DEBT MARKET

| Bank | Status | Rank |
|------------------------------------|---------|------|
| Morgan Stanley | 3.30879 | 1 |
| First Boston Corporation | 3.03206 | 2 |
| Goldman Sachs | 2.87465 | 3 |
| Merrill Lynch | 2.84215 | 4 |
| Salomon Brothers | 2.82667 | 5 |
| Lehman Brothers Kuhn Loeb | 2.19846 | 6 |
| Paine Webber | 2.10382 | 7 |
| Prudential Bache Securities | 2.09874 | 8 |
| Dean Witter Reynolds | 2.04583 | 9 |
| Warburg Paribus Becker | 2.02556 | 10 |
| Smith Barney Harris | 2.01689 | 11 |
| Dillon Read | 2.01074 | 12 |
| Bear, Sterns | 2.00232 | 13 |
| Kidder Peabody | 1.99902 | 14 |
| Shearson | 1.99621 | 15 |
| E. F. Hutton | 1.99388 | 16 |
| Donaldson Lufkin & Jenrette | 1.98863 | 17 |
| Lazard Freres | 1.98856 | 18 |
| Wertheim Securities | 1.98685 | 19 |
| L. F. Rothschild, Unterberg | 1.98629 | 20 |
| Drexel Burnham Lambert | 1.98431 | 21 |
| UBS Securities | 1.86799 | 22 |
| M. A. Schapiro and Co. | 1.68572 | 23 |
| Bell Gouinlock | 1.57457 | 24 |
| Atlantic Capital | 1.23710 | 25 |
| Burns-Fry and Timmins | .85649 | 50 |
| Robert W. Baird and Co. | .66863 | 75 |
| Sanford C. Bernstein and Co. | .44116 | 100 |
| Folger Nolan Fleming Douglas | .21968 | 125 |
| Anderson & Strudwick | .07925 | 150 |

this transaction history. In particular, LSIMUL captures whether or not the bank and issuer are involved in another transaction at the time of the deal. I selected a 120-day window around the date of the offering; if the bank leads an offering for the corporation in any of the primary markets or gives merger/acquisition (M&A) advice to the issuer during this period, then LSIMUL is coded "1," if not, LSIMUL is coded "0." Discussions with bankers revealed that the process of deciding on a particular issue and bringing the offer to market can often take about this length of time. Accordingly, if a bank managed two offerings for the same firm over this time period, I regarded the issues as "simultaneous." If the bank managed an offering or assisted in M&A more than 120 days

but less than 1 year prior to the offering, LHIST1 is coded "1," otherwise LHIST1 is coded "0."

Also relevant to the spread is whether the offering is negotiated or competitive. In the latter, competing banks submit sealed bids to the issuer, and the lowest bid wins the offering. Though this type of offering is not particularly common for corporate securities, many public utilities are required by law to solicit bids in this form. In negotiated offerings, the firm selects a bank on the basis of discussions with one or several investment banks. Price is still an important factor underlying the exchange between corporation and client, though it need not be the only factor which affects the corporation's decision to choose a particular bank. Due to the fact that competitive offerings are awarded strictly on the basis of price, it follows that sealed bids typically have lower spreads. A dummy variable, COMPET, is coded "1" if the offering is competitive and "0" if it is negotiated. Note that even in a sealed bid competitive offering, status can be relevant to market processes to the extent that it leads to a reduction in costs.

Finally, it is important to control for whether or not the security is what is called convertible. Some corporations issue bonds that they are willing to convert into stock at a predetermined conversion rate. This feature reduces the risk of holding the bond and presumably, therefore, also decreases the spread. If the bond offering is convertible, CONVERT = 1; CONVERT = 0 otherwise.

Column A of table 3 presents the results. In discussing these results, I will focus almost exclusively on the effect of status on spread since nearly all the control variables are of little sociological interest. The one possible exception to this rule are the effects of LHIST1 and LSIMUL, which measure client-specific economies. Both variables have a negative effect on spread, though the effect of LSIMUL is not statistically significant. The negative effect is consistent with the view that a firm can derive economic benefits from seeking to embed exchange exchanges in ongoing relations.

Focusing on the variable of main interest, we observe that STATUS has a statistically significant and negative impact on price at the .01 level. A unit change in STATUS leads to a reduction in spread of .080. While the substantive significance of this coefficient may not seem considerable, it is important to evaluate it in light of the phrase that "Loyalty is one basis point" and in light of the difference in status scores across the banks. If we refer back to table 3, we see that the difference in status between, for example, second-ranked First Boston and fourteenth-ranked Kidder Peabody is 1.03. The difference between these two banks translates into an ability or willingness of the former to underbid the latter by eight basis points.

TABLE 3
EFFECT OF INTERACTION BETWEEN SIZE AND STATUS ON PERCENTAGE SPREAD IN THE INVESTMENT GRADE MARKET

| Variable | A | B | C | D |
|-------------------------|---------------|----------------------------|---------------|----------------|
| INTERCEPT | 1.244 (.153) | 1.221 (.152) | 8.608 (.750) | 14.298 (2.239) |
| STATUS | -.080 (.013) | -.062 (.013) | -2.813 (.274) | -2.271 (.287) |
| STATUS * log(AMT) | ... | ... | .151 (.015) | .121 (.016) |
| Log(VOLHIST) | ... | -.006 (.0008) | -.006 (.0008) | -.126 (.162) |
| Log(VOLHIST) * log(AMT) | ... | ... | ... | .007 (.001) |
| Log(AMT) | -.0116(.008)* | -.007 (.008)* | -.413 (.412) | -.451 (.041) |
| CONVERT | .679 (.0275) | .679 (.027) | .665 (.026) | .658 (.027) |
| AAA | .004 (.025)* | -.012 (.025)* | -.018 (.025)* | -.021 (.025)* |
| AA+ | -.101 (.027) | -.105 (.027) | -.124 (.027) | -.134 (.027) |
| AA | -.086 (.024) | -.093 (.023) | -.090 (.023) | -.096 (.023) |
| AA- | -.084 (.025) | -.079 (.024) | -.080 (.023) | -.087 (.024) |
| A+ | -.053 (.025) | -.055 (.025) | -.054 (.024) | -.062 (.024) |
| A | -.086 (.023) | -.090 (.023) | -.089 (.022) | -.093 (.022) |
| A- | -.045 (.026) | -.044 (.025)* | -.040 (.024)* | -.052 (.025) |
| SPRATMIS | -.111 (.021) | -.091 (.021) | -.088 (.020) | -.096 (.020) |
| LHIST1 | -.008 (.003) | -.007 (.003) | -.008 (.002) | -.008 (.002) |
| LSIMUL | -.001 (.003)* | $-9 \cdot 10^{-4}$ (.003)* | -.002 (.003)* | -.002 (.003)* |
| COMPET | -.186 (.018) | -.178 (.0173) | -.172 (.0170) | -.173 (.0169) |
| PREDPROB | -.492 (.105) | -.444 (.105) | -.459 (.103) | -.492 (.102) |
| R ² | .27 | .29 | .31 | .32 |

NOTE.— $N = 2,787$. Numbers in parentheses are SEs.
*Not significant at the .05 level.

COMPETING SIGNAL: VOLUME

Higher-status banks are able to underbid lower-status banks because status is a signal of quality. The more that the issuer, potential syndicate members, and investors can be assured of the quality that the underwriter will exhibit in managing the offering, the less difficult and less costly it is for the underwriter to put the deal together. However, as noted earlier, status is a second-order signal, and, while more encompassing and multifaceted than other signals, the linkage between status and quality is perhaps looser than the linkage between quality and first-order signals. If the lack of status is to represent a real constraint, then lower-status banks must not be able to compensate for their inferior position in the status order by the cultivation of first-order signals.

The primary first-order signal is the recent volume history of the bank. The link between short-term volume history and underwriting quality is quite tight since a bank's quality with respect to both issuers and investors is contingent upon its insight into the market, which in turn depends on the extent to which it is in the deal stream between a relatively large number of issuers and investors. Insofar as underwriting volume is a quantitative indication of the degree to which a bank is in this deal stream, such volume is clearly a determinant of quality. Volume is also a signal of quality properly defined; the marginal difficulty and cost of underwriting a given issue at a given point in time is inversely associated with recent underwriting volume, since lower volume implies less knowledge. A bank with less knowledge of the market exposes itself to greater risk in seeking to make competitive bids for a given issue.

There are several institutional manifestations of recent volume as a signal. Perhaps the most important are the league tables, which provide annual or quarterly rankings of the banks according to their volume of underwriting activity in the various primary markets. They are published at quarterly intervals in the major trade journals. The ex-Shearson banker quoted earlier commented that the banks are so obsessed with their position in the tables that they call the SDC and *IDD* information service to insure that the services have not forgotten to include a deal that they have managed. While the banker interviewed believed that the ascribed importance to ranks was unjustified, he also pointed out that many bankers perceived that such an omission could lead to a decline in their rank in the league tables and thereby undercut their standing in the eyes of market participants.

Significantly, Hayes (1971) has observed that volume—at least as an indication of the bank's relational position—is also a critical underpinning of status; however, a major difference between volume as a first-order signal and status as a second-order signal is that the latter is rela-

tively insulated from short-term fluctuations in the former. There are at least two reasons for this fact. First, volume, in and of itself, does not affect status; rather, it is the volume that is underwritten well that influences status. Increased volume will cease to have a positive impact on status if it is achieved either through the underwriting of issues that offer less than expected returns on their investment or through the poor execution of offerings. Repeated exceptionally weak returns on investment will cause the bank's status with investors to decline (e.g., Miller 1986). To the extent that a bank's status with investors declines, it is likely that its status with issuers will decline as well, since the lower status on the investor side will mean that the bank will find it more difficult and therefore more costly to effectively place a security.

A second, more important reason for the difference between volume and status is that volume is not the only factor that underlies status. Another, for example, is a reputation for honesty. When one of Morgan Stanley's associates became involved in an insider trading scandal in 1978, the status of the firm suffered even though few at Morgan Stanley were involved (Chernow 1990, pp. 634–35). When E. F. Hutton was convicted for involvement in a large-scale trading scandal in the late 1980s, its status declined precipitously until it was absorbed by Shearson Lehman American Express in a merger to form Shearson Lehman Hutton. Though there is no highly visible, institutionalized ranking of firms in terms of honesty to parallel that which exists for volume, violations of codes of SEC regulations are noticed by market participants and can have an impact on perceptions of a firm.

Given that the status order and volume rankings are two informational orders in which decisions are potentially embedded, the constraining effects of status ultimately depend on the degree to which the status order exerts an independent effect on market outcomes beyond that exerted by short-term fluctuations in volume. In effect, the issue is one of whether the second-order signal of status has an effect on price when the first-order effect of volume is controlled.

There are some compelling reasons to believe that this status effect should be minimal and that the short-term informational order should be most relevant in primary securities markets. We noted earlier that in the debt markets, especially investment grade debt, there is a rather high degree of information to distinguish the abilities of banks and the qualities of securities. However, to the extent that information is differentially distributed across banks, we would expect recent information to be of almost exclusive importance. Rapidly changing, unpredictable conditions endemic to financial markets would at least seem to suggest that it is only recent knowledge that is of any considerable utility to market participants.

If informational decisions are based more on the yearly fluctuations in volume and less on the status ordering in the tombstones, then the time lag between shifts in quality and shifts in perception will necessarily be shorter, and the significance of the Matthew effect for the primary securities markets will be minimal. Recall that it is the lag between shifts in quality and shifts in perceptions that insulates a given producer from the competition of those of lower status. If a low-quality bank can come to be regarded as identical to a high-quality bank simply by increasing its volume over a short time frame, then it faces essentially the same cost and revenue profile as that of the high-quality bank. It may have to absorb some short-term costs to expand at a rapid rate, but ultimately there is no persistent constraint on its ability to invade the high-quality niche. In this case, status would simply be a by-product of underlying economic processes. It is only when the lower-quality bank must confront a relatively disadvantageous cost-and-revenue profile over an extended time frame that the status ordering and Matthew effect can be considered relevant.

More specifically, to the extent that the status ordering is indeed irrelevant to market decisions and behavior, then inclusion of a variable for the more recent volume history should eliminate the direct effect of status upon either the terms of trade of particular exchanges or the observed pattern of relations. I, therefore, repeat the prior regressions including a variable for short-term volume history, VOLHIST. This variable is the volume of offerings for which a bank was lead manager in the 12 months prior to the month in which the deal was issued. Volume history thus is a moving one-year window, which I have updated monthly. Obviously, any particular choice of volume history is at some level arbitrary; I selected one year because the league tables rarely summarize more than a year of information. Due to a positive skew in the distribution of the variable, the variable is converted into logged dollars.

Though volume history is included as the primary signal of the short-term informational order, it is important to note that interpretation of a coefficient for volume history cannot be unambiguous. To the extent that economies of scale are relevant, the volume history measure summarizes such effects; moreover, insofar as the volume measure necessarily implies greater knowledge of the supply and demand conditions at the moment of the offering, these knowledge effects are also bound up in this variable. However, we are not so much interested in clearly interpreting the effect of volume as in determining whether the inclusion of the variable eliminates the effect of status upon the terms of trade.

Column B of table 3 presents the results. The most noteworthy finding is that the effect of status remains significant even after the inclusion of the variable for volume, though the apparent status effect does drop from

-.080 to -.062. The volume coefficient is negative and statistically significant at the .05 level. The relative magnitudes of the volume and status coefficients provide insight into how much a lower-status bank would have to exceed a higher-status bank in annual volume in order to bid the same as the higher-status actor. The status coefficient is 10 times that of the coefficient for volume history, implying that a given bank would theoretically have to underwrite $\exp(10) = 22026.5$ times the volume of a bank one unit higher in status in order to compensate for the preexisting status differential. When considered in these terms, the .6 unit difference in status between fifth-ranked Salomon Brothers and sixth-ranked Lehman Kuhn Loeb clearly seems impenetrable to a strategy of "eating" deals to signal quality. Even a .005 unit difference in status, such as that between seventh-ranked Paine Webber and eighth-ranked Prudential Bache Securities, seems difficult to overcome simply by increasing volume.

To be clear, volume is a signal of quality that apparently reduces costs and thereby facilitates the bank's bidding ability; however, the effect of this signal is insufficient to overwhelm even minor differences in the status order. The effect of status, even after the inclusion of the volume variable, means that a lower-status bank cannot simply buy its way into a high-status niche by increasing its volume over the short term.

In addition to allowing us to distinguish the relative importance of the two informational orders (status and volume) confronting market participants, these results, especially the negative relationship between price and status, provide a unique opportunity to compare conventional economic models and the status-based model. Economic models invariably predict that if there is a superior good in the market at a lower price than an inferior good, then the inferior good will be driven from the market. In effect, the results in table 3 suggest that the superior good is less expensive than the inferior good; the higher-status producers underwrite a given offering at a lower price than the lower-status producers, but, as we observed earlier in table 1 and figure 1, there is considerable stability in the investment grade market. There is no trend toward increasing concentration, as we would expect if the higher-status banks were monopolizing the market. Such stability is completely consistent with the status-based model. The higher-status banks realize that their reputation with investors, position in the market, and hence cost structure is contingent on their ability to preserve their ties with high-quality clients. Thus, whereas conventional price theory predicts that the negative relationship between price and status should lead to a domination on the part of the highest-status actor(s), the status-based model anticipates the stability of the market that is actually observed.

Moreover, the status-based model also helps make sense of a similar,

prior finding that emerged in an analysis of innovation in the primary securities markets. In an examination of 58 product innovations in the investment banking industry between 1974 and 1986, Tufano (1989) found that pioneers of an innovation charge underwriting spreads that are 18–25 basis points lower than imitators charge; he concluded that such a finding is anomalous from the point of view of economic theories of differential pricing. He suggested that perhaps innovators may be trying to “buy market share” by charging a lower price (p. 94). Even if true, such a conclusion does not explain why there are limits on the process. Why is the innovator not able to “buy” the whole market? Since boundaries around financial products are always somewhat arbitrary, it is clearly possible to conceive of these product innovations as defining their own markets. As such, the conclusions of the foregoing analysis can be applied to product innovations.

THE RELATIONSHIP BETWEEN OFFERING SIZE AND UNDERWRITING SPREAD

So far, the results show that, on average, higher-status banks underbid lower-status banks for a given deal in the investment grade market. Such a result is consistent with the claim that issuers are primarily concerned with price in their selection of an underwriter, whereas potential syndicate members and investors are more concerned with status. These groups have reason to be more concerned with status than the issuer. They are creditors, while the issuer is the debtor. As creditors, they have comparatively more to lose if the issue falls in value because of the poor performance of the bank. The issuer, on the other hand, will make the same interest payments regardless of how the security is placed.

Nevertheless, the issuer still faces some risk. If investors or syndicate members lose money on a particular offering, they will almost undoubtedly be less likely to purchase a security from the same issuer in the future. As a result, a poor performance by the underwriter will make it more costly for the issuer to raise money in the financial markets; the issuer will have to compensate the purchasers of its security for what they perceive to be additional risk.

Consequently, there might be some reason to expect positive return on status, especially for the larger offerings. As previously noted, not all issues are considered to be of the same difficulty; in particular, the larger an issue, the more challenge is involved in its placement and the more questions that there would be about a given bank's ability to successfully accomplish the task. For the RJR Nabisco junk bond offering, Kohlberg Kravis Roberts and Co. (KKR) doubted very much if even Salomon Brothers, a special bracket firm and the bank with the second largest

underwriting volume in the junk bond market, could effectively lead the offering (Burrough and Helyar 1990). While the RJR offering was indeed an exceptionally large offering, it does raise the issue of whether or not there are positive returns to status for increasingly large issues. The fact that large issuers underbid the lower-status issuers on average does not preclude the fact that higher-status banks may be able to derive a premium from underwriting the larger, more difficult issues. In terms of the formalization in (1), it is reasonable to expect that as the issue becomes larger, Θ should move away from zero, and p_h should become greater than p_l .

This reasoning suggests a hypothesis: there should be a positive interaction between the size of the issue and the status of the underwriter in terms of the regressions on percentage spread. The larger the issue, the greater the returns to status ought to be. More specifically, given the earlier regressions, we would expect a positive interaction between $\log(\text{AMT})$ and STATUS .

Column C of table 3 reproduces the regression in column B with the inclusion of an interaction term for STATUS and $\log(\text{AMT})$. Column D is the same as C except that it adds an additional interaction term between $\log(\text{VOLHIST})$ and $\log(\text{AMT})$. In both regressions, the interaction between STATUS and $\log(\text{AMT})$ is positive and highly significant. In column D, the effect of status in the investment grade market is

$$-2.27 + .121 * \log(\text{AMT}).$$

Through algebraic manipulation of terms, it can easily be shown that the effect of status becomes positive when the offering is greater than \$140 million. This value is clearly within the range of observed values; the median offering size across all banks is \$100 million. At \$200 million, or the seventy-fifth percentile in terms of offering size, a one-unit difference in status, such as that which existed between First Boston and Kidder Peabody, translates into a four-basis-point benefit for the former over the latter.

Considered in conjunction with the results from columns A and B in table 3, we observe that higher-status banks on average underbid the lower-status banks; however, for the larger issues, the latter must underbid the former, and, as the qualitative evidence suggests, they must do so from a relatively disadvantageous cost structure. The result is significant because it illustrates the fact that for the larger, more difficult issues, status is relevant not only to the investor and potential syndicate members but to the issuer's decision as well.

This result points to at least one factor that may be relevant in establishing scope conditions for the applicability of status to a producer's relations with a particular set of exchange partners, be they consumers

or some other set of actors associated with the market, and in so doing it helps specify when producers will derive positive rents from status and when they will only derive cost advantages. As a constituency's risk from entering into an exchange relationship with a particular producer increases, we would expect that status should become more relevant to their decision. If the risk is almost entirely borne by third parties to the transaction, such as syndicate members or retailers, then the advantages will strictly be on the cost side. However, to the extent that the risk is borne by consumers, producers will also be able to obtain positive rents as well.¹⁶

Of course, we would need to be careful about drawing any strong inferences about scope conditions on the basis of one case. As articulated at the outset, the applicability of the status-based model is contingent on the validity of three key assumptions: (1) that quality is unobservable, (2) that status is regarded as a signal of quality, and (3) that perceptions of a producer's status are contingent upon the identity of those to whom the producer is tied. The investment grade market is a compelling case because the first key assumption is minimally met, and there are many features of the market which would lead us to believe that the second assumption would be minimally met as well. Nevertheless, the salience of interpersonal and interorganizational networks in investment banking means that assumption 3 has more *prima facie* validity than it might have, for example, in mass consumer markets, where individual buyers are largely anonymous with respect to one another. Of course, this third assumption could apply in such markets. The status that consumers ascribe to a particular car manufacturer may indeed be contingent upon the identity of those who drive that manufacturer's cars, but we must nonetheless be careful about generalizing on the basis of this one analysis.

CONCLUSION

I have sketched out a conception of the market in which the constructed and maintained reputations of producers provide a tangible basis for decisions. I have attempted to provide a theoretical and empirical justification for removing price theory from the abstract Walrasian auction or even game-theoretic scenarios and situating it in the tangible status order that underlies and circumscribes the actions of producers. In doing so, I have tried to provide a basis for the convergence of economic and sociological work on markets. As economists look increasingly to processes of

¹⁶ It is difficult to imagine a circumstance in which the producer could derive no cost benefits but positive rents since risk on the consumer side would generate both rents and lower transaction costs for the higher-status producer.

retrospection in decision making (e.g., Kreps 1990), they can potentially gain from how social structure enters into the decision-making process. As the examination of the investment banking industry has been intended to indicate, a concern with status is not incompatible with a recognition of the importance of price theory. A "bidding war" exists among the banks, but the bidding war is embedded in a socially defined context. Only by taking this context into account can we understand the stability of markets when a higher-status producer can offer a good of a given quality at a lower cost than can a lower-status competitor. Status is, therefore, not simply an epiphenomenal reflection of quality. Rather, status exerts a strong influence on market outcomes by providing producers with different incentives to invest in quality and placing constraints on their ability to expand outside their niche in the market. Thus, just as status has been shown to affect behavior in interorganizational fields (Galaskiewicz 1985), so we have seen how status can inform our understanding of the market.

Yet, beyond providing insight into the paradox raised by the applicability of the Matthew effect to markets, this conception of market competition represents a point of departure for further sociological work on markets. One question of interest concerns mobility. The fact that preservation of identity is a constraining force does not mean that firms are unable to shift their position in the market. Mobility and a stable structure are no less compatible in a product market than in a labor-market context. Moreover, it is only once the underlying structure is made clear that the actual determinants of mobility can be accurately examined. Therefore, far from being in tension with a concern for mobility, this framework helps make such an examination possible.

A second issue of interest is to more explicitly incorporate the firm into the analysis of the market. As noted earlier, many firms are actively involved not just in one market, but across multiple markets. I hope the examination of the investment banks has indicated that a multimarket identity is not inconsistent with the dynamics posited by the status-based model. Nevertheless, an interesting question for additional analysis is how the status acquired in one market can be transferred to another market, as when IBM entered the personal computer market after being the dominant actor in the mainframe computer market. It seems reasonable to expect, particularly in young markets, where firms have had little opportunity to establish a reputation in that arena, that "imported" status could be particularly relevant to understanding organizational outcomes. More generally, such an examination could help pave the way for a more systematic understanding of change in markets.

A third issue of interest, which is also related to the issue of change, is the complex role of innovation in status-based competition. If investment

banking is representative of other markets, innovation is not only consistent with but partially responsible for the reproduction of the status order. The highest-status banks are the ones that are most able to successfully introduce innovations into the market, and the innovator role contributes to the perception of superior quality. Nevertheless, to the degree that a lower-status bank is able to successfully introduce an innovation into the market, the innovator role is a powerful means of mobility, as revealed most dramatically by the rise of Drexel Burnham Lambert in the 1980s. In extreme cases, innovation may lead to a change in the status order itself.

The incorporation of status processes into the understanding of market competition thus seems to provide considerable ground for the development and extension of a sociological approach to markets. Despite some important insights into market processes, current sociological work on markets has so far lacked any unifying concepts or themes beyond the pronouncements that social relations matter and that actors are not as atomized as specified in the neoclassical model. At a minimum, the theoretical conclusions and empirical findings of this article allude to the importance of status and perceptions as one such theme.

APPENDIX

Measurement of Status

For the purpose of this analysis, I collected the tombstone ads which appeared in the *Wall Street Journal* in 1981, the year before the six-year span covered by the SDC data base. During this period, there were 180 tombstones for investment grade debt issues. Having established the status of banks in this initial period, we can explore how the flow of issuers becomes distributed across the status positions in the following years.

One might argue that to assess the effects of status in the later years of our sample, we should collect not just the 1981 data, but also data from the following years. However, there are two reasons not to do so, one practical and the other theoretical. Practically, collection of such additional data imposes considerable cost and time demands, and there are good reasons to suspect that the returns from such additional data collection would be small. The fact that major shifts in status position are relatively rare events means that changes in any given bank's status over this period are likely to be minor. Moreover, we would expect that any bias resulting from the failure to collect the additional years of data would be in the direction of minimizing the observed effect of status. Hence, to the extent that the 1981 data have a significant impact on

economic outcomes in the later years, it would seem reasonable to infer that the effects of status would be no less significant if the later years were taken into account.

Even if the data were easily obtainable, a reliance upon the 1981 status scores is preferable to contemporaneous status scores. Status is relevant to market outcomes because it is decoupled from quality shifts. If status shifts perfectly mirrored quality shifts, then status would, for all practical purposes, be little more than an artifact of economic processes. In this particular case, the status order is relevant only to the degree that it helps to insulate perceptions of banks from shifts in determinants of underlying true quality. So, in the 1980s, the status order is a significant market structure only to the extent to which a bank's initial status insulates it from the beneficial or adverse effects of market changes. To assess how economic changes affect changes in status, we would of course need longitudinal data on the latter; however, if we wish to examine status not as a dependent but as an independent variable, the need for such longitudinal data is questionable.

One might argue that a drawback of this data is that the derived status scores are based more on the banks' perceptions of each other than on the perceptions of the issuers. However, two factors reduce the potential significance of this problem. First, since status manifests itself in the investment banking context in large part by lowering the transaction costs associated with the formation of banking syndicates, the perceptions of the banks themselves are highly relevant. Second, the assumption that perceptions of the competitors are highly correlated with perception of clients seems fairly plausible; but if the assumption does not hold, we should once again expect that poor measurement would weaken rather than strengthen the results.

Banks that made less than three appearances in the tombstones over the year observed were excluded from the analysis; 170 banks appeared in more than three syndicates. Invariably the excluded banks were relatively minor foreign banks that appeared in syndicate offerings because the issuer was based in their country.

If a strong principle of transitivity applied to the status ordering, we could simply rank the banks by those appearing above and below them. However, the facts that (1) the number of brackets varies from tombstone to tombstone and (2) there are minor fluctuations when a bank obtains an unusually small or large share on a given offering means that this simple procedure will not work for data obtained in 1981 even if it may have worked in an earlier period, such as that studied by Carter and Manaster (1990).

In making use of the tombstone data, I apply one of the standard

status measures for relational data on status, Bonacich's (1987) $c(\alpha, \beta)$ measure. Formally, the measure is defined as follows:

$$c(\alpha, \beta) = \alpha \sum_{k=0}^{\infty} \beta^k R^{k+1} \mathbf{1}, \quad (1)$$

where α is a scaling factor, β is a weighting factor, R is a relational matrix, which is 0 along the main diagonal and in which cell r_{ij} summarizes the relative superiority (or inferiority) of i with respect to j , and $\mathbf{1}$ is a column vector of ones.¹⁷ For the purposes of this analysis, a given cell r_{ij} is the proportion of times that a bank i appears above bank j in the tombstone ads in which they jointly appear.¹⁸ Thus β was set at three-quarters of the reciprocal of the largest eigenvalue, though alternative positive values were examined and yielded no considerable substantive difference in the status scores. The parameter α is standardized such that

$$\sum_{i=1}^n c_i(\alpha, \beta)^2 = n,$$

where n is the number of actors in the social system. Both the construction of the matrices and the calculation of status scores were programmed in FORTRAN, with certain IMSL routines being used.

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¹⁷ Bonacich's $c(\alpha, \beta)$ measure can alternatively be one of centrality or status. Bonacich (1987) notes that the interpretation of the measure is contingent upon the nature of the ties. If the ties are symmetrical, the measure is more appropriately one of centrality; if the ties are asymmetrical, the measure is more appropriately interpreted as one of status.

¹⁸ If two banks did not appear together, an imputation procedure was used that is discussed more fully in Podolny (1991).

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Crime as Strategy: Testing an Evolutionary Ecological Theory of Expropriative Crime¹

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This article presents a refined specification of Cohen and Machalek's general evolutionary ecological theory of expropriative crime and results of tests employing two complementary approaches: (1) the development of a game-theoretic model that mathematically tests the logical adequacy of the theory's fundamental assumptions and (2) the conduct of computer simulation experiments to analyze the model's behavior and test its consistency with novel hypotheses suggested by the theory. Mathematical analysis indicates that the assumptions are logically consistent. Experiments generally confirm the theory's hypotheses, but indicate that several modifications are necessary. Additional theoretical insights obtained from the simulation experiments are also discussed.

In a recent article, Cohen and Machalek (1988) propose a general theory of expropriative crime that offers a comprehensive synthetic explanation for a wide range of empirical findings generated by the competing disciplines that have studied crime. Drawing from the theoretical framework of evolutionary ecology, they emphasize that the key to understanding the nature and distribution of illegal behaviors by which offenders expropriate resources (objects of symbolic or material value) from other lies in understanding the relationship between productive and expropriative strategies and their interdependence within populations of individuals. Unlike many theories that characterize criminal behavior as pathological

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or abnormal, this theory argues that expropriative crimes are best viewed as behavioral strategies typically used by normal individuals in unexceptional social systems to meet their needs.²

This approach is unique among criminological theories because it focuses on a neglected sociological element of crime—the evolution of behavioral *strategies* that arise and spread through populations by means of culturally mediated processes. These cultural processes are akin to natural selection in the sense that the more successful strategies are more likely to be imitated and then proliferate within a population. Most extant theories of crime concentrate on identifying the different structural characteristics of societies or the differences in individual traits or experiences that increase the probability that people will engage in crime. By contrast, while evolutionary ecological theory accounts for the possible criminogenic influence of individual and social factors, it argues that expropriative criminal strategies should not be viewed *solely* as the properties of individuals. Instead, these strategies should be viewed as behavioral options, the expression of which often varies independently of the characteristics of the persons who adopt them (Cohen and Machalek 1988, p. 467). Thus, the authors identify behavioral strategies, rather than individuals and their personal and social traits, as the focus of analysis for the study of crime. Furthermore, they treat both criminal and non-criminal behavioral strategies as competing entities in their own right. This approach allows them to explain how strategies can influence their own proliferation. In addition, this perspective permits the generation of novel hypotheses that cannot be derived from extant criminological theories. While Cohen and Machalek deduce nine such hypotheses, they offer no new evidence with which one could evaluate their theory's adequacy. In fact, the authors concede that such tests may be difficult to produce, given the complexity of their theory.

Here we present the results of a test of the theory that employs two complementary approaches: (1) development of an evolutionary game-theoretic model that tests the logical adequacy of the theory's fundamental assumptions mathematically and (2) computer simulation experiments that employ the model to assess whether or not the novel hypotheses can be supported throughout a wide range of conditions. As we will show, these approaches were quite productive. A number of new theoretical insights were obtained from the experiments by specifying the model so that it accounted for important issues suggested by the literature on cultural evolution and systematics (e.g., cultural biases for or against certain

² This explanation of crime as a normal characteristic of naturally functioning societies is consistent with Durkheim's (1895) dicta but invokes substantially different causal processes.

types of strategies, lags in their transmission, and transmission reliability). These approaches also helped refine the theory. As described by Cohen and Machalek, the theory made extensive use of general concepts and examples from evolutionary ecology that are foreign to many social scientists. One of the most important and useful steps in the modeling process we employed here was to tease out the implications of these concepts and specify them explicitly. As one might expect with such a broad and synthetic theory, several issues neglected in the initial exposition of the theory were identified during this process. The following refined specification addresses these issues and describes explicitly how the mechanisms and processes invoked by the theory might function.

A REFINED EVOLUTIONARY ECOLOGICAL THEORY OF CRIME

As its name implies, the theory uses both ecological and evolutionary concepts to explain crimes by which people illegally expropriate resources from others. These two fields of study have quite different, but complementary, foci. Whereas ecology focuses upon the reciprocal interrelationships between living organisms and their environment, the focus of evolution is the processes by which these relationships lead to cumulative change in the characteristics of organisms or populations over time. Earlier in this century, the synthesis of ecology with evolution vitalized the biological sciences by uniting explanations of *how* individuals and groups of organisms function with explanations about *why* they change over time. Similarly, Cohen and Machalek complement their ecological explanation for the differences between expropriative crime rates in populations at a particular point in time with a description of the evolutionary dynamics that produce significant change in these rates. It is this synthesis that makes the theory "general."

Ecological Aspects of Criminal Behavior

The ecological components of this theory include such factors as routine patterns of activity, the availability and distribution of resources, modes of production, and competition. Cohen and Machalek argue that the social organization of production within human populations creates an opportunity structure that invites invasion by various strategies of expropriation. Individuals acquire valued resources from a particular resource niche by employing behavioral strategies. If a strategy involves production of goods or services through one's own efforts, it is characterized as "productive." If it involves taking material or symbolic resources from others through force, fraud, or other illegal actions, it is characterized as

"expropriative."³ There is often a great deal of variation in both the ability of individuals to acquire resources within different niche dimensions and in their valuation of, or motivation to acquire, a particular resource. Much of this variation arises from sociocultural, psychological, physiological, and developmental factors that are related, either directly or indirectly, to the ability of individuals to compete legitimately (and illegitimately) for valued resources within different social environments. This means that asymmetries between persons that are associated with competitive advantage for acquiring resources (e.g., social characteristics and individual traits as well as differences in motivation) significantly impact strategy selection and evolution within populations.

One of the most important and unique features of the theory, however, is the contention that the selection of productive and expropriative strategies need not always derive from individual differences. According to the original specification of the theory, the likelihood that an individual will adopt a particular strategy is related to its potential for success—what we will refer to as "payoffs." Strategy payoff is, in turn, strongly dependent on the kinds of strategies adopted by others in the population and the frequency with which they are employed. This "frequency dependence" (Fisher 1930) means that rarer nonconformist strategies (such as illegal expropriation) will often enjoy an advantage over more common legal strategies simply because the payoffs can be higher because of less intense competition. It follows that populations in which everyone pursues the same strategy may be highly susceptible to invasion by nonconformist strategists. Hence, the theory predicts that expropriative activity will often emerge in response to opportunities created by patterns of behavioral uniformity and that *ceteris paribus* expropriative behavioral strategies will proliferate when their frequency-dependent payoffs increase. This somewhat simplistic assumption requires respecification, however, because it ignores the other possible effects of frequency dependence.

³ For example, *productive strategies* associated with the automotive industry include such behaviors as legal exploitation of "natural" resources for manufacturing, transportation of raw materials and finished goods, manufacturing and assembly, sales, maintenance and repair, motor-vehicle licensing, and traffic enforcement. *Expropriative strategies* in this same arena would include fraudulent billing by mine owners for iron ore shipments, armed hijacking of tractor trailers full of auto parts, embezzlement of autoworkers' pension funds, thefts of tools by assembly line workers, salespeople's rolling back of odometers on used cars, billing for unnecessary repairs, motor vehicle theft, and fraudulent registration of a stolen vehicle. Note that productive strategies provide the opportunities and/or targets for expropriative strategies. To the extent that individuals find opportunities for expropriation tempting, productive strategies may also act to increase their motivation to expropriate.

A more accurate specification of this key concept must acknowledge that the frequency with which a behavioral strategy is employed in a population may have several possible alternative effects on its potential payoffs: positive, null, and negative. Positive frequency dependence is common in cooperative situations (see Arthur 1988) and where economies of scale prevail. In this case, as more people employ a strategy, its returns increase. The absence of frequency dependence is also possible. For example, this could occur when the number of individuals exploiting a particular niche is trivial compared with the quantity of resources available. It is not reasonable, therefore, to assume that the returns to productive strategies are always negatively frequency dependent. In the special case of expropriation within a social context, however, the theory's assumption is indeed reasonable because one or more of the following will frequently occur: (1) there may be direct competition for expropriated resources that diminishes payoffs, (2) fear of victimization can lead individuals to employ protective counterstrategies, or (3) righteous indignation can produce a general public response against "cheaters" (see Sugden 1986). While this insight has important implications for operationalizing the theory, the prediction that a different mix of expropriative and productive strategies can exist in otherwise identical populations due to frequency-dependent effects alone still appears to be quite reasonable.

The theory's ecological components allow us to conceive of alternative strategies as competing against one another within a population. This conception is important because it enables the theory to explain how different strategy proportions can evolve over time because they, like other culturally transmitted information, are differentially retained and transmitted by individuals. Thus evolutionary processes are viewed as acting directly on the pool of alternative strategies, while individuals are treated simply as the temporary executors of these strategies.

Evolutionary Aspects of Criminal Behavior

The theory uses a Darwinian approach to describe how criminal strategies evolve culturally. Cohen and Machalek (1988, p. 491), like researchers from a variety of disciplines (e.g., Holland 1975; Anderson, Arrow, and Pines 1988; and Goldberg 1989; Calvin 1990), argue that evolutionary reasoning is appropriate because natural selection may operate upon both genetic and nongenetic informational media. Thus, a Darwinian approach may be used to describe the growth or decline over time of learned behavioral strategies based on media such as language or other forms of symbolic expression—so long as media-specific differences in the constituent processes and mechanisms of evolution by natural selection (replication, variation, selection, transmission) are taken into account (see

Boyd and Richerson 1985). In sum, if cultural traits such as criminal strategies are "inherited" via teaching and imitation, and if different traits (such as successful criminal strategies) have different likelihoods of being transmitted, then a Darwinian model of criminal strategy evolution is applicable, even though this "inheritance" is mediated by culture rather than DNA.

The evolutionary implications of the theory become clearer when we focus on the processes by which successful expropriative strategies provoke deterrent responses or counterstrategies from individuals and groups within societies. Over time, ecological interactions in which illegal expropriations occur tend to produce defensive counterresponses such as protective measures. These measures tend to increase the cost of expropriation and thus could lead to its decline. While this process may dampen the attractiveness of *existing* expropriative strategies, it may also lead to the development of new or modified illegal strategies that, in turn, stimulate additional counterresponses.⁴ Thus, a self-reinforcing system emerges that is reminiscent of predator-prey coevolution or "arms races." These dynamic coevolutionary processes will tend to yield one of two possible outcomes: either the ratio of criminal to noncriminal strategies becomes relatively stable at some nonzero level or it oscillates indefinitely. In either case, the theory asserts that the resulting strategy ratio within a population reflects the advantages of certain *strategy proportions*, not necessarily a mix of different types of individuals as many other theories of crime predict.

In summary, evolutionary ecological theory claims (Cohen and Machalek 1988, p. 496) to include two sets of causal factors neglected by competing explanations of crime. These are (1) intrinsic properties of expropriative criminal strategies that also affect the rate of occurrence and successful completion of expropriative crime, and (2) the independent influence a strategy's relative frequency may have on the rate of expropri-

⁴ For example, prior to the 1950s many autos did not have locking ignition systems. Early car thieves who possessed driving skills were therefore able to expropriate any automobile to which they could gain access. In response to rising rates of auto theft, locking ignitions were introduced during the 1950s. Thieves then needed two strategies, driving and "hot wiring," to steal a car. In the 1970s, locking steering columns were introduced. This protective counterstrategy could only be overcome by thieves who were armed with devices such as dent pullers that could disable steering locks and also had driving and hot wiring skills. Again, as thieves became adept at overcoming steering locks, more car owners began employing alarm systems and insurance companies began offering discounts for alarmed vehicles. As this "arms race" between car owners and expropriators continues, we again see new types of expropriative strategies for defeating alarm systems emerging. We also see the emergence of more sophisticated counterstrategies such as radio beacons that enable police to locate stolen vehicles quickly.

ative crime. Cohen and Machalek (1988, p. 497) acknowledge that, as with other theories of evolution and crime causation, definitive empirical tests of these hypotheses are difficult to accomplish. As a first step toward hypothesis evaluation and testing, however, they suggest using computer simulations based upon game-theoretic models that would allow estimation of the independent effects of strategies on expropriative crime rates, controlling for differences in individual characteristics between populations.

METHODS FOR TESTING SYSTEM DYNAMICS

Here we evaluate Cohen and Machalek's assumptions and the hypotheses based upon them through evolutionary game-theoretic simulation models. As Vagners (1974, p. 1172) notes, game theory is a well-established approach to theory testing "where the criteria [for solution] are difficult to establish other than by subjective assumptions." Game theory uses a simple model to capture the generic properties of a complex dynamic process. It attempts to get at the *shape* of the fundamental interactions that drive a process. Evolutionary game theory avoids dubious assumptions about the degree of "rationality" present in criminal behavior by using the Darwinian principle that successful phenotypic variants—whether of physical or behavioral traits—are more likely to appear in the future than are unsuccessful variants.

Those unfamiliar with the use of computer simulation modeling may not appreciate its utility for theory building and testing. Essentially this approach aids us in two ways: first, by requiring that abstract verbal concepts and relationships be specified in unambiguous mathematical equations and, second, by allowing us to explore the effects of changes in various parameters and combinations of parameters on the model's behavior. Mathematical models force us to specify a theory more exactly than is possible with verbal descriptions alone. Using such models in computer simulation experiments allows us to test the reasonableness of the specification and its implications for the behavior of a complex system of relationships that may be difficult or impossible to obtain using other analytical techniques. This approach has proved very useful for studying the behavior of complex systems in ecology, engineering, physics, and other fields, both for theory development and for focusing empirical research on those aspects of a research problem that are most important (e.g., see Boyd and Richerson 1985; Sugden 1986; Kelley and Thibaut 1978; Axelrod 1984, 1986, 1987; Maynard Smith 1982; Holland 1988).

Here we use simulation modeling both to evaluate Cohen and Machalek's hypotheses and to gain insight into the relative importance of each parameter or combination of parameters. We accomplish this by examin-

ing the model's behavior during thousands of iterations on the computer in which the value of specific parameters (and parameter combinations) is varied while other parameters are held constant. This allows us to challenge the theory at its most fundamental level. For example, if it can be established that, given the basic assumptions that underlie this theory, expropriative crime will not persist in a population owing to frequency-dependent effects—or if it will persist only under relatively restrictive circumstances—its explanation of why crime is “normal” will be falsified. The results of these experiments will then be used to develop the theory further.

The theory asserts that expropriative crime will tend to persist at the population level because the payoffs for expropriation are often negatively frequency dependent. At the individual level, there will always be some persons within societies motivated to employ expropriative strategies due to asymmetries of ability and/or need. To illustrate this point, Cohen and Machalek (1988, pp. 496–97) ask their readers to imagine two populations, A and B, that are identical in terms of the individual and group characteristics often used to explain crime. If expropriative crime were found to be significantly higher in population A relative to B, strategy analysis, unlike competing theories of crime, could provide many alternative hypotheses to account for the difference. For example, among the alternative hypotheses offered by the theory (1988, p. 497),⁵ a higher rate of expropriative crime could be expected in A relative to B: (1) because of frequency-dependent effects alone, (2) if there were more opportunities for expropriation in A due to greater productive strategy innovation, (3) if expropriative strategies were transmitted more easily in A, (4) if the yield of expropriative strategies was higher in A, (5) if the material, social, psychological, and biological “execution costs” of expropriation were lower in A, and (6) if competition among expropriators was less intense in A.

These hypotheses are evaluated here using a model described by a simple set of difference equations that specify the fundamental assumptions and functional relationships proposed by the theory. These assumptions are as follows: (1) individuals require resources to survive and prosper, (2) they most often obtain resources by employing behavioral strategies within social settings, (3) behavioral strategies are acquired most often through cultural transmission processes in which people learn

⁵ These six hypotheses include Cohen and Machalek's central contention that frequency-dependent effects alone may be responsible for the evolution of different strategy proportions and the six also include most aspects of the nine novel hypotheses they propose (Cohen and Machalek 1988, p. 497). Using the more concise specification of the theory presented here, it would have been redundant to discuss each of these hypotheses in its original form.

from or imitate others, (4) strategies with a high probability of success are more likely to be transmitted than those with a low probability of success, (5) competition for resources is pervasive because populations tend to fill all of the "resource space" available in a particular environment, (6) variation in individual traits and characteristics differentially affects the ability of people to employ one or another behavioral strategy successfully in certain social and environmental contexts, (7) some individuals within any population are therefore less able than others to compete successfully for resources using a particular strategy, and (8) individuals who are less able to compete tend to be motivated to seek out alternative strategies that they expect to offer greater access to valued resources.⁶ The behavior of this system of equations is analyzed mathematically and graphically to identify system boundaries and equilibria. Using this analysis as a point of departure, the model will next be tested through a series of computer simulation experiments. The numerical techniques we employ to analyze model behavior and test hypotheses are described in greater detail below.

An Expropriative Crime Game⁷

Assume that all individuals in a very large population play repeated rounds of a game with one another in groups of size N (where N is a fixed integer and $N \geq 2$). Further assume that only two possible behavioral strategies, S_1 and S_2 , may be used to play the game. Individuals are randomly assigned to these groups at the beginning of each round. During each round, individuals acquire resources by applying a behavioral strategy. The net value of the resources each acquires depends upon the behavioral strategy she employs, as well as the strategies employed by other members of the group. After each round of play, the population is reassembled and each individual meets M other potential role models at random (where M is a fixed integer and $M \geq 1$). During each meeting, the individual learns what behavioral strategy the other person used during the preceding round. She also obtains information about the net

⁶ Cohen and Machalek obviously make the assumption of pervasive competition given in item 5 even though they do not state it explicitly. It is consistent with ecological theory in general and is specifically applied to humans by Hawley (1986, p. 43).

⁷ We acknowledge the assistance of Joel R. Peck (1988) who suggested that this problem might be studied through the use of a similar set of equations. While the game used here is similar to that suggested by Peck, the equations have been modified extensively to simplify returns to production and include new factors such as costs associated with expropriation, lag effects, and biased transmission of strategic information.

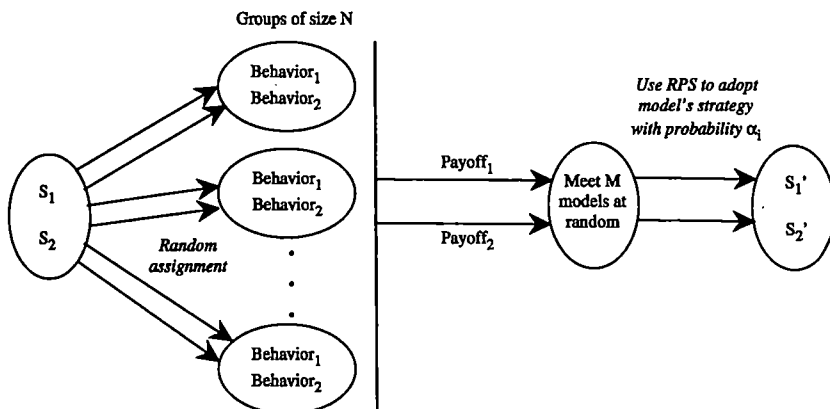


FIG. 1.—Life-cycle diagram. S_1 's and S_2 's are randomly assigned to groups of size N . They interact and receive payoffs according to their strategy and the strategies used by other group members. They meet M others from the population at large and apply a relative payoff scheme (RPS) with probability α_i to choose their strategy for the next round.

value of resources obtained using that strategy. Individuals then adopt a strategy for the following round.

Which strategy they adopt depends upon two factors, a relative payoff scheme and a transmission bias constant.⁸ The relative payoff scheme (henceforth RPS) links the likelihood that a strategy will be imitated and employed with its relative success during the previous round. Relative success is calculated after each round as the ratio of the sum of all payoffs for a strategy to the total payoffs during that round. This captures the idea that more successful strategies are more likely to be imitated. The transmission bias constant(s), α_i (such that $0 < \alpha_i \leq 1$), defines the probability that individuals playing the game will use the RPS as the basis for imitating a strategy in the next round. This accounts for the possibility that the general propensity of people to use the RPS learning rule as the basis for imitating others' strategies may vary from one population to another. In some versions of the game, the transmission bias constant may vary by strategy. This accounts for "bias" for or against different types of strategies, a concept that is important from a cultural evolutionary perspective (e.g., Boyd and Richerson, 1985, pp. 81–131; Lumsden and Wilson 1981, pp. 53–98). Figure 1 describes the steps through which players proceed for each round of the game. This is commonly referred to in ecology as a "life-cycle" diagram.

⁸ A "relative payoff scheme" is analogous to Maynard Smith's (1982, pp. 60–77) "relative payoff sum" learning rule.

| Strategy | S_1 | S_2 |
|----------|-----------|--------------|
| S_1 | 1 | $1 - \gamma$ |
| S_2 | $c\gamma$ | 0 |

FIG. 2.—Payoff matrix for producers (S_1) and expropriators (S_2) with group size $N = 2$. Note that the payoff for an S_1 paired with another S_1 is 1, the value given in the upper left-hand unshaded quadrant. Similarly, the payoff for an S_2 paired with an S_1 ($c\gamma$) is given in the lower left-hand quadrant, and so on.

Players and Rules

Assume that S_1 's acquire resources by production without diminishing the resources produced by others in their group (see our earlier discussion of frequency dependence) and that S_2 's acquire resources by expropriating them from producers in their group. Also S_1 's retain the total net value of their resources so long as there are no S_2 's in their group. If there are one or more expropriators in a group, each producer will lose a fraction, γ , of their resources. Therefore, if the net resources acquired by each producer in the absence of expropriators is 1, producers acquire $1 - \gamma$ resources when expropriators are present. Assuming that producers cannot lose all they produce in a round, the following inequality applies:

$$0 < \gamma < 1. \quad (1)$$

In the simple case where $N = 2$, the resources an expropriator obtains during a round depend upon the other player's strategy, the value of resources expropriated (γ), and the costs associated with expropriation (e.g., material, social, psychological, and physical costs, risk of punishment, depreciation of expropriated resources by a "fence," etc.). These costs are represented by a depreciation constant, c , where $0 < c < 1$. Figure 2 describes the payoff matrix for groups of size $N = 2$.

Extending this payoff matrix to games in which $N \geq 2$, we may calculate the average payoffs for each strategy during a given round of play. The average payoff for resources acquired by S_1 's is w_1 . The average

payoff for S_2 's is w_2 . Let p equal the proportion of producers in the population during a round of play. And let q equal the proportion of expropriators in the population. Note that $q = 1 - p$. The average payoff obtained by producers during a round may therefore be expressed as: $w_1 = p + (1 - \gamma)(1 - p) = 1 - \gamma(1 - p)$.

Where $N \geq 2$, the average payoff that expropriators obtain during a round depends upon three things: the average number of expropriators per producer,⁹ the value of resources expropriated, and the costs associated with expropriation. In order to keep the game as simple as possible, assume that the expropriators in each group share their gains equally. For any group, there are k producers and $N - k$ expropriators. Expropriated resources, γ , are depreciated by c . The payoff for each expropriator in any group may therefore be expressed as:

$$\frac{kc\gamma}{(N - k)}.$$

To determine w_2 , the average payoff to expropriators in all groups, we must multiply the probability of an expropriator interacting with k producers, given group size N , by the average payoff. This may be expressed as follows using the binomial probability distribution:

$$w_2 = \sum_{k=0}^{N-1} \binom{N-1}{k} \left(\frac{kc\gamma}{N-k} \right) p^k q^{(N-1-k)}.$$

As is demonstrated in the Appendix, this may be simplified as:

$$w_2 = pc\gamma \left(\frac{1 - p^{N-1}}{1 - p} \right). \quad (2)$$

Given these assumptions, it is now possible to show that p' , the proportion of the population employing productive behavioral strategies during the next round of play, can be calculated from the following equation:

$$p' = p \left(1 - \frac{q\alpha_2 w_2 [1 - (1 - \bar{w})^M]}{\bar{w}} \right) + q \left(\frac{p\alpha_1 w_1 [1 - (1 - \bar{w})^M]}{\bar{w}} \right), \quad (3)$$

where α_1 and α_2 are biases affecting the probability of imitating S_1 and S_2 respectively. This means that p' , the proportion of S_1 's in the next round of play, is equal to (the proportion of S_1 's in the last round) minus

⁹ This is a conservative specification of the game since producer's losses are not sensitive to the number of expropriators in each group. It simplifies the game mathematically and makes it easier to falsify Cohen and Machalek's hypotheses by making it more difficult for expropriative strategies to increase.

(S_1 's who defect to S_2) plus (S_2 's who defect to S_1). In other words, changes due to the addition of new recruits and losses due to defection.

We define the average population payoff as: $\bar{w} = (pw_1 + qw_2)$. Player sampling error is accounted for in equation (3) by the term $[1 - (1 - \bar{w})^M]$ which uses sampling variation to attenuate the reliability of transmission. When the number of models (M) met by players after each group interaction is smaller, the chance that players will err when applying the relative payoff scheme is greater. When players employ the RPS rule, they compare the payoffs obtained by models with the population average for all players during that round. By inspection, we observe that $\bar{w} \leq 1$. Therefore, as M increases, the sampling error term approaches the reciprocal of the true sample mean against which w_1 and w_2 are compared under the RPS rule:

$$\frac{[1 - (1 - \bar{w})^M]}{\bar{w}} \rightarrow \frac{1}{\bar{w}}.$$

Thus, as M becomes larger, players' estimates about which strategy provides better relative payoffs according to the RPS rule become more accurate.

Mathematical Analysis

Subsequent analysis will be easier if we first make equation (3) more tractable. Recall that

$$p' = p \left(1 - \frac{q\alpha_2 w_2 [1 - (1 - \bar{w})^M]}{\bar{w}} \right) + q \left(\frac{p\alpha_1 w_1 [1 - (1 - \bar{w})^M]}{\bar{w}} \right).$$

Now let

$$z = \frac{[1 - (1 - \bar{w})^M]}{\bar{w}}.$$

Therefore, $p' = p - pqz(\alpha_2 w_2 - \alpha_1 w_1)$. Since $q = 1 - p$, we may restate this equation in the following, more manageable form:

$$p' = p - pz(1 - p)(\alpha_2 w_2 - \alpha_1 w_1). \quad (4a)$$

For most of the analysis that follows $\alpha_2 = \alpha_1$. In these cases the equation will be rendered as

$$p' = p - pz\alpha(1 - p)(w_2 - w_1). \quad (4b)$$

Given equations (4a) and (4b), it is obvious that $p = 0$ and $p = 1$ are equilibria. That is, when no members of the population are producers, or when all are producers, the proportion of producers (p) will remain

constant from one generation to the next. As $p \rightarrow 0$, inequality (1) tells us that expropriators get nothing. The $p = 0$ equilibrium is unstable since, according to inequality (1), $(1 - \gamma) > 0$. Therefore, the proportion of producers will always increase when producers are sufficiently rare. The existence of the other extreme equilibrium, $p = 1$, is somewhat more informative. As we see from equation (4a), when $p = 1$, $p' = 1 - z\alpha(0)(w_2 - w_1) = 1$.

The question of equilibrium stability is central to our analysis. Cohen and Machalek assert that the dynamic relationships described by the model cause crime to be normal (i.e., to persist in societies over time at some intermediate value). This condition will exist only if there is a stable internal equilibrium. We can see that if $0 < p < 1$, then $p' < p$ if and only if $w_2 > w_1$. Where $w_1 < w_2$, $p' \leq p$ for $0 < p < 1$. Under some combinations of parameters, there may be values of $p > 0$ which are still too low to support any expropriators. In those instances, $w_1 < w_2$ and $p' = p$. In other words, p will increase if, and only if, the payoffs to production (S_1) are greater than the payoffs to expropriation (S_2). As p approaches unity, $w_1 = 1$. To obtain the limit for w_2 , we apply the general form, noting that, for any number r such that $0 < r < 1$,

$$\sum_{i=0}^{\infty} r^i = \frac{1 - r^{x+1}}{1 - r}.$$

This allows us to rewrite equation (2) as

$$w_2 = pc\gamma \left(\frac{1 - p^{N-1}}{1 - p} \right) = c\gamma \sum_{i=1}^{N-1} p^i. \quad (5)$$

It is therefore evident that the limit of w_2 as p approaches unity is $c\gamma(N - 1)$. Thus, the $p = 1$ equilibrium is stable when production allows players to gain more resources than expropriation, that is, when $1 > c\gamma(N - 1)$. If this inequality is reversed, the $p = 1$ equilibrium is unstable and any slight perturbation will cause q to increase. Note that, for $N = 2$, this means that the $p = 1$ equilibrium will always be stable. We may interpret this to mean that expropriative crime will not pay for even a few criminals if groups are sufficiently small and the yield per victim ($c\gamma$) is sufficiently low. If expropriation is relatively lucrative, and if victims are plentiful, there will always be room for a few S_2 's.

Test Criteria

From the standpoint of the theory, the two extreme equilibria are themselves relatively uninteresting. Since humans generally interact in groups larger than two, the finding that $p = 1$ is always stable for $N = 2$ also

tells us very little. Based upon the analysis thus far, however, it is apparent that both boundaries are unstable under a wide range of conditions. We might therefore expect a frequency-dependent equilibrium, a limit cycle, or some other type of attractor in the interior. This expectation is important since, given the assumptions listed previously, Cohen and Machalek's explanation for the persistence of crime can *only* be valid if there is some stable intermediate point or points around which expropriative strategies will increase when rare and decrease when common. If this is not the case, if there are no stable internal equilibria, the analysis thus far tells us that additional factors must be invoked to explain the observed persistence of expropriative crime in human societies. The null hypothesis to be tested using this model may therefore be stated as:

HYPOTHESIS 0.—There are no stable intermediate equilibria (i.e., equilibria for which $0 < p < 1$) for values of $N > 2$ in this system of equations.

From equation (4a), it can be demonstrated that intermediate equilibria can occur at points where $w_1 = w_2$ since $p' = p - pz\alpha(1 - p)(0) = p$. Therefore, when $w_1 = w_2$, $p = \hat{p}$ (the equilibrium value of p).

Given that intermediate equilibria are possible, we next need to determine how stable they may be. Equilibria are stable if and only if Δp (defined as $p' - p$) declines when $p > \hat{p}$ and increases when $p < \hat{p}$. The term Δp may be derived from equation (4b) by moving the first p term to the left side of the equation:

$$\Delta p = p' - p = -pz\alpha(1 - p)(w_2 - w_1).$$

This now permits us to make the following observations about the behavior of this system of equations: (1) if $w_2 > w_1$, Δp is negative and p decreases toward \hat{p} and (2) if $w_2 < w_1$, Δp is positive and p increases toward \hat{p} .

Figures 3a, 3b, and 3c illustrate the relationship between w_2 and w_1 for various parameter values that are arguably quite reasonable.¹⁰ In figure 3a there is a single stable internal equilibrium, figure 3b has both stable and unstable internal equilibria, and figure 3c has no internal equilibria. (Note, however, that increasing group size N by 2 will cause the system to develop an equilibrium point. This will be discussed in more detail later.) These figures demonstrate the types of behaviors that the model can produce and that both stable and unstable internal equilibria *are* possible. As will be shown later, changes in α , γ , c , and M influence the rate of change, but not the sign, that is, they influence the

¹⁰ Note that the various parameter values used in figs. 3–10 were selected in an attempt to clearly illustrate model behavior discussed in the text. None of the figures presents atypical behavior.

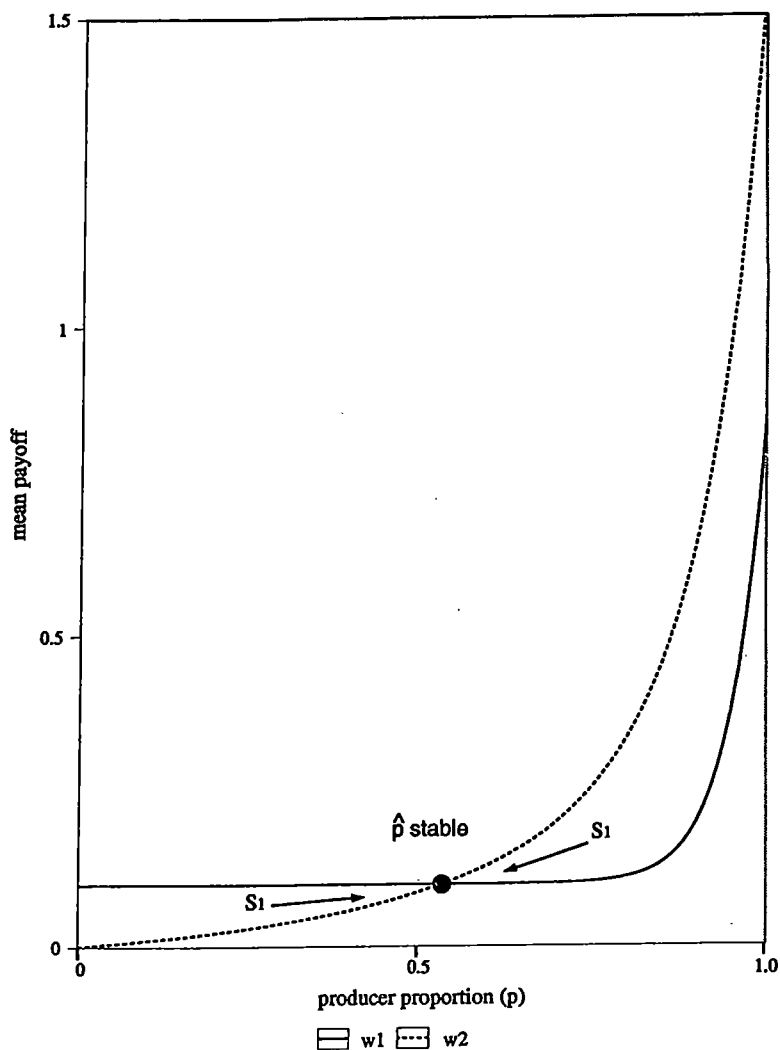


FIG. 3a.—Mean payoffs for producers (S_1) and expropriators (S_2) intersect at a single stable internal equilibrium point ($M = 5$, $N = 20$, $\alpha = 0.1$, $c = 0.1$, $\gamma = 0.9$).

magnitude, but not the direction, of Δp . The arrows near equilibrium points show whether the proportion of expropriators is increasing or decreasing. Equilibria are stable if the arrows are converging, unstable if they are diverging. (By convention, stable and unstable equilibria are marked with solid and empty circles respectively.)

Phase plots provide us with another way to discover if internal equilib-

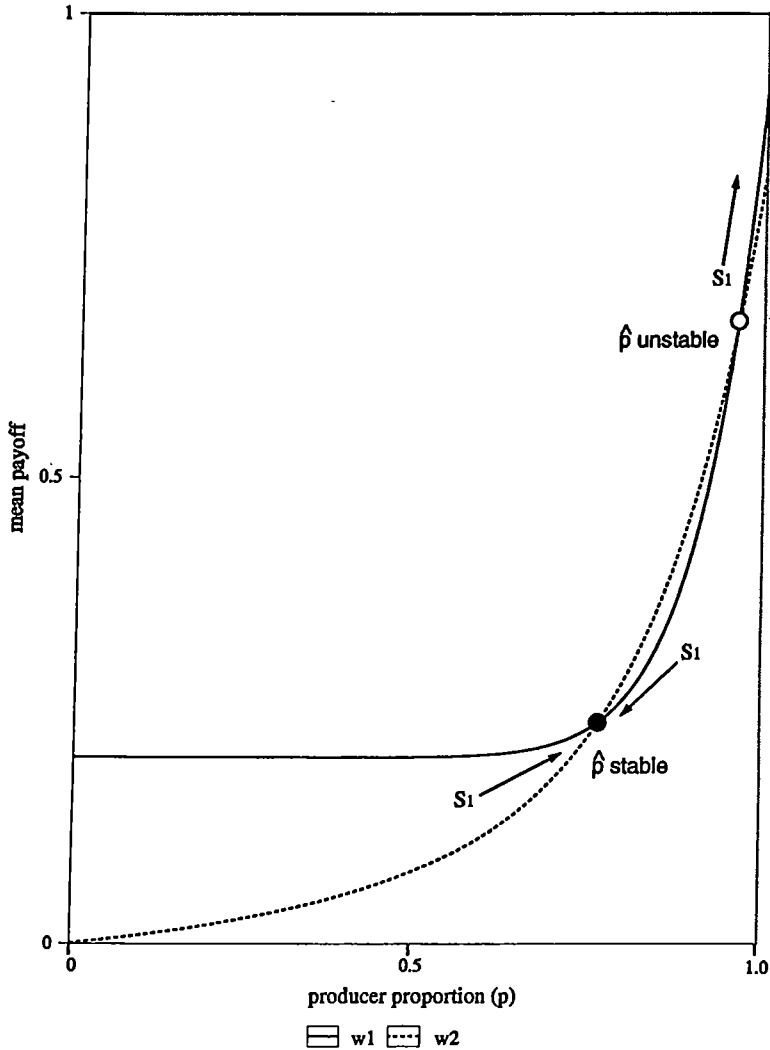


FIG. 3b.—Mean payoffs for producers (S_1) and expropriators (S_2) intersect at two equilibria points. Note that only one is stable ($M = 5$, $N = 12$, $\alpha = 0.8$, $c = 0.1$, $\gamma = 0.8$).

ria exist in a system by showing whether p will increase, decrease, or remain unchanged at various starting values. Because it allows more direct comprehension of the way parameters and constants appearing in the equations for a model affect system behavior, this graphical approach can be more informative than mathematical expressions alone. Each point on the phase plots that follow compares the initial producer propor-

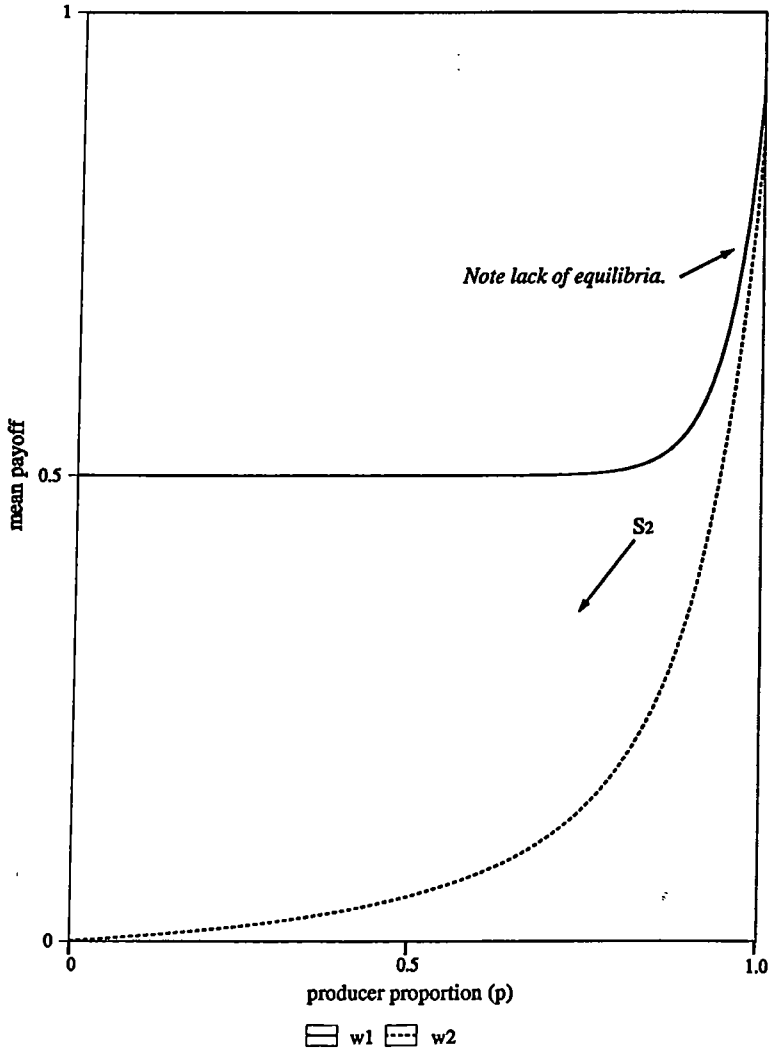


FIG. 3c.—Mean payoffs for producers (S_1) and expropriators (S_2) do not intersect. Under these conditions, expropriation will always tend to decline ($M = 5$, $N = 20$, $\alpha = 0.1$, $c = 0.1$, $\gamma = 0.5$).

tion (p) with the proportion after one time step (p'). The relative distance between points in a phase plot indicates the rate at which the proportion is changing, with greater distance indicating faster change. Comparing a series of the plots with the equilibrium vector (\hat{p}), allows us to determine at which point(s) the proportion will increase or decrease. Since the proportion of producers and expropriators is stable when $p = p'$, we

may determine how many internal equilibria are possible by observing where the \hat{p} vector intercepts the series. Figure 4 shows a phase plot above a line graph of w_1 and w_2 generated with the same parameter values in order to illustrate the relationship between the two types of graphical representations. In a system with no equilibria, the phase plot would not cross the equilibrium (\hat{p}) vector. For multiple equilibria, it would cross the \hat{p} vector more than once (see Edelstein-Keshet [1988, pp. 164–209] for an intuitive treatment of this topic).

As the foregoing figures indicate, the null hypothesis must be rejected. Based upon mathematical and graphical analysis, it appears that this system will always have a stable intermediate equilibrium for $0 < p \leq 1$ so long as N is sufficiently large. This confirms that the frequency-dependent effects Cohen and Machalek propose are both possible and plausible given the set of assumptions stated previously. It is therefore now possible to use this model to examine the hypotheses listed previously.

SIMULATION EXPERIMENTS

Simulation experiments using the model developed in the previous section allow us to examine a series of hypotheses derived from the theory's novel predictions, even though they would be difficult to test mathematically. In addition to allowing hypothesis testing, experiments of this sort provide a way to gain information about the relative importance of various parameters used in the model to account for the theory's assumptions. They also enable us to examine relationships between these parameters under various conditions. This information can provide theoretical insights and may be useful for guiding future empirical research.

Special attention was paid to the model's sensitivity to individual and collective parameter changes, resistance to perturbation from equilibrium points, and consistency of results. Since it was not possible to test the entire parameter space at all scales, both extreme and intermediate parameter values were sampled by iterating the model through 500 generations for each value tested in an attempt to identify unexpected behavior. Once the model's behavior was better understood, its sensitivity to initial conditions was tested. Parameter values were then selected that were theoretically plausible, yet intermediate, so that no single parameter could dominate the model's behavior throughout the entire range of initial conditions examined. Several approaches were used in order to obtain a thorough sample. First the change from p to p' was measured for various parameter values at intervals of 0.01 for the range $p = 0.1$ to 0.99. The model was next iterated through 13,500 consecutive "generations" or rounds of play from an extreme value until Δp became ex-

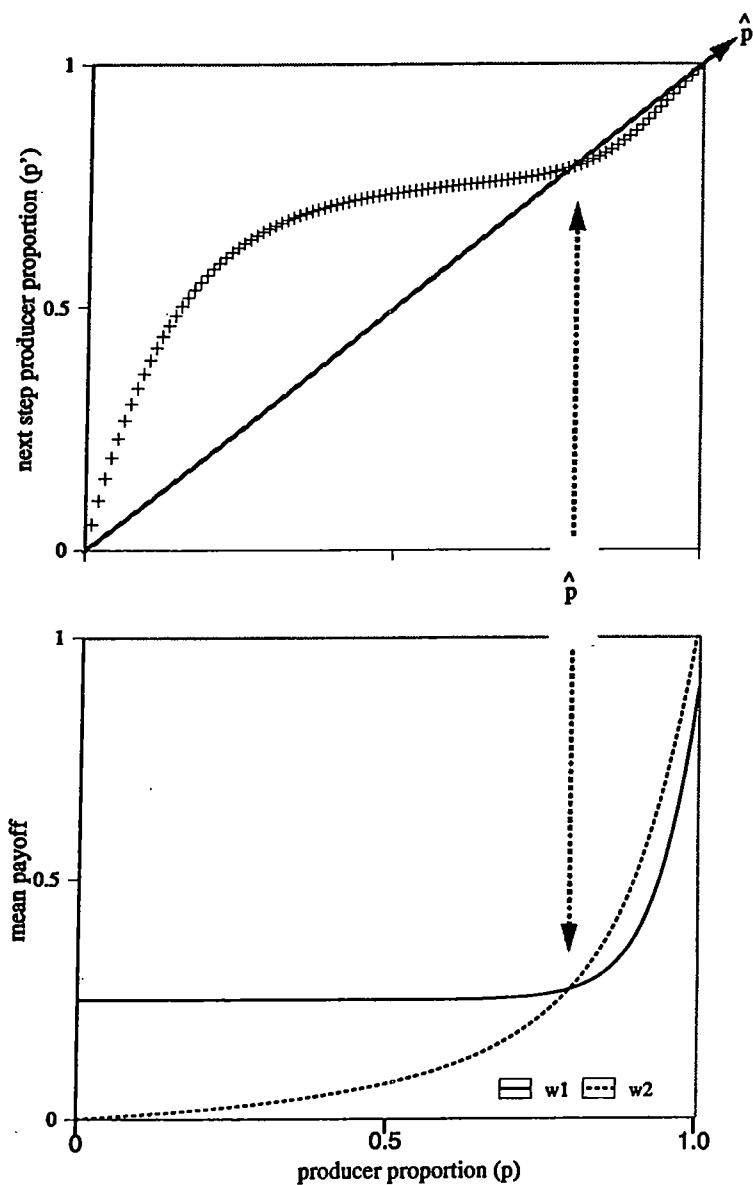


FIG. 4.—Comparing phase plot of change in producer proportion with mean producer and expropriator payoffs (w_1 and w_2). Note that the equilibrium points on both graphs are located at identical producer proportions ($M = 20$, $N = 16$, $\alpha = 0.9$, $c = 0.1$, $\gamma = 0.75$).

tremely small. The advantage of this approach is that it does not sample the system at artificial intervals that might obscure unusual behavior. When, as was usually the case, one or more internal equilibria were present, they were approximated iteratively using an interpolative optimization scheme. Rate of change per unit of parameter value was also monitored to identify parameter regions in which unexpected behavior might be found. Roughly 200,000 generations were sampled in total. Precision for calculations was kept at 15 decimal places.

Hypothesis 1

Recall that Cohen and Machalek specify two populations A and B that are replicas of one another in terms of the level of asymmetry between the abilities and/or needs of individuals, as well as any other biological, psychological, and social factors that have commonly been proposed to explain crime. Holding these factors constant, they predict that the population with a higher innovation rate for productive strategies will tend to have a higher crime rate because more opportunities for expropriation will be created (see also Cohen and Felson 1979; Cantor and Land 1985). While it was not possible to test for changes in strategy diversity using the model in its present form, the broader question of opportunity can be addressed.

For example, to test the hypothesis that, *ceteris paribus*, more opportunities for expropriation are associated with increases in the rate of expropriation, we examine the role of group size (N). This seems appropriate since the number of individuals in each group determines the number of opportunities S_2 individuals have to expropriate in a given round. From equation (5), we see that w_2 is a positive function of N such that

$$w_2 = c\gamma \sum_{i=1}^{N-1} p^i.$$

One method for examining the sensitivity of the proportion of expropriators (q) to changes in N is to iterate the model repeatedly. The model was iterated for 500 generations at various values of N while other parameters were set so that some level of expropriation would be assured during early generations. Had the proportion of expropriators (q) decreased for any increase in N , the prediction would not have been supported. As earlier analysis predicted, when $N = 2$, $q \rightarrow 0$. Even when N was too low to support increases in q , however, larger values of N were associated with slower rates of decline in q . In all cases where N was greater than the limit for w_2 , q increased for any increase in N . It is interesting that, in most cases, the rate of change in q caused by increases

in N appears to decline as N increases. This indicates that the influence of N reaches a point of diminishing returns. Analysis showed further that the model is capable of rather complex behavior—even when arguably reasonable parameter values are used. The phase plots in figure 5 were generated by sampling the change from p to p' for $0.01 \leq p \leq 0.99$ at intervals of 0.01. They show that the system may have no equilibria, a single equilibrium, or multiple equilibria depending upon the value of N .

As figure 5 demonstrates, larger group size (denoted by higher values of N) appears to be associated with a decrease in p (and conversely with an increase in q) so long as the proportion of producers is greater than the limit for w_2 . This suggests that group size is one of the parameters that determines whether the proportion of expropriators in a population will increase. This finding is consistent with the prediction that more opportunities for expropriation will tend to be associated with a higher crime rate (Cohen and Felson 1979).

Hypothesis 2

The theory predicts that the replica population in which expropriative strategies are transmitted more easily, quickly, and economically will tend to have a higher expropriative crime rate. Two factors are associated with strategy transmission in the model. They are the transmission bias constant(s) (α_i) and the number of other strategists (potential role models) individuals meet at the end of each round (M). In order to test this question directly, the terms α_1 and α_2 were set for different values in equation (4a). Clearly, one would expect higher values of α_i to increase the rate at which strategy S_i is transmitted. As was noted earlier, as M increases, sampling error associated with the RPS decreases since:

$$\frac{[1 - (1 - \bar{w})^M]}{\bar{w}} \rightarrow \frac{1}{\bar{w}}.$$

As a consequence, as M increases, the strategy that is more favored by the relative payoff scheme should be imitated more frequently. Conversely, the less favored strategy should be imitated less often. Moreover, as M increases, the influence of α_i declines. Figure 6 superimposes the phase plane plots of p and p' . Plot *a* was generated with a higher value of M than plot *b*. It is evident here that, when M increases, the population of strategists that is favored under the RPS learning rule (in this case S_1) increases more rapidly. Note, however, that *a* and *b* converge with the equilibrium vector at the same point. This occurs because M has no effect on the proportion $p:q$ at which $w_1 = w_2$.

The results achieved by iterating the model with various values of

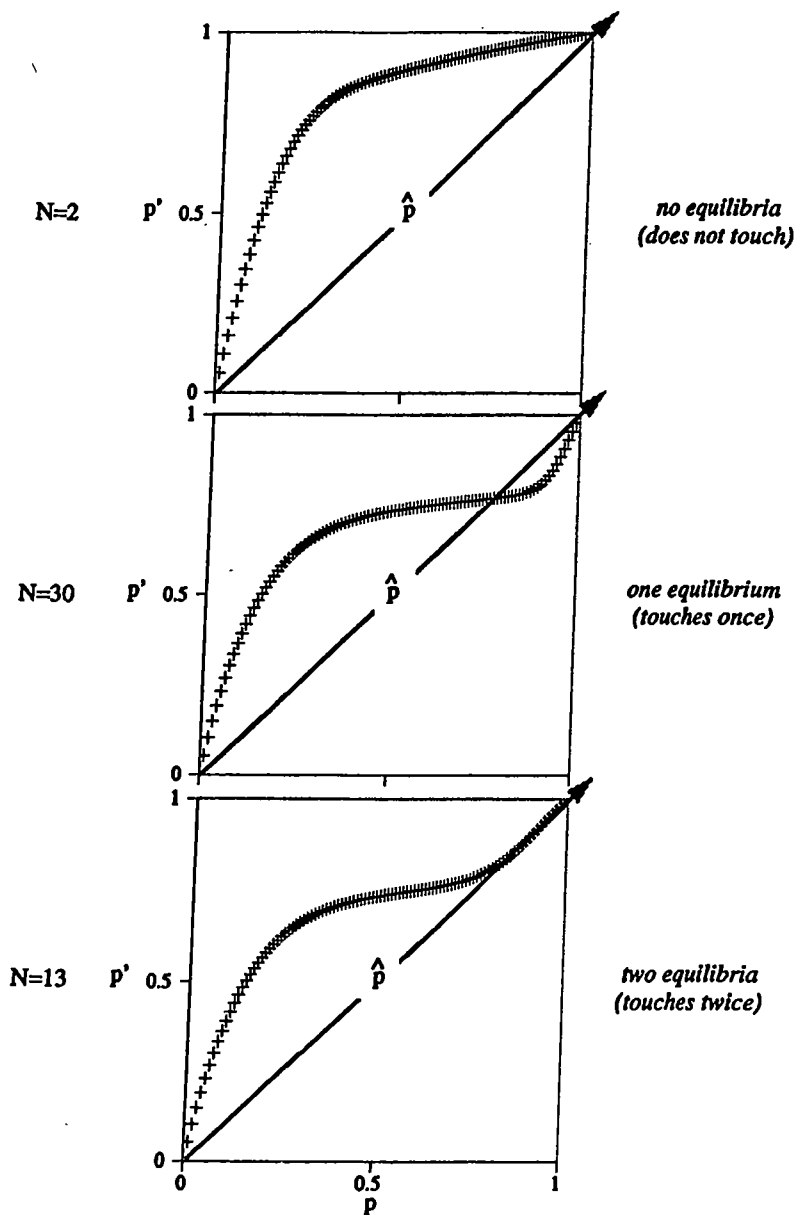


FIG. 5.—Phase plots of p , p' for various values of N . Note that changing N can cause the system to have no, one, or multiple equilibria ($M = 20$, $\alpha = 0.9$, $c = 0.1$, $\gamma = 0.75$).

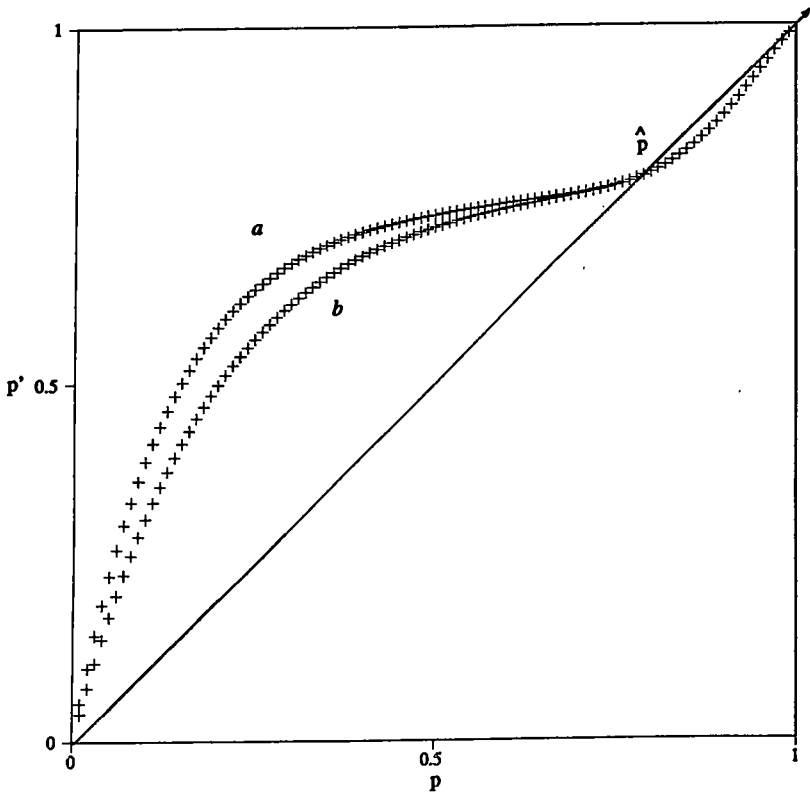


FIG. 6.—Phase plots of p , p' showing effect of increasing the number of potential role models (M). In series a , $M = 20$, in series b , $M = 13$ ($N = 16$, $\alpha = 0.9$, $c = 0.1$, $\gamma = 0.75$).

M from a starting value in which there was little competition among expropriators were also consistent with this interpretation. Increasing the value of M while other parameters were held constant increased the speed with which equilibrium was reached but did not affect the proportion $p:q$ at equilibrium.¹¹

Simulations using various values of the transmission bias constants (α_i) also yielded results consistent with theoretical expectations. At higher values of M , the effect of α_i declined rapidly; that is, when player sampling became more accurate due to larger sample size, the influence of transmission bias declined. As figure 7 shows, when $\alpha_1 = \alpha_2$, increasing α had an effect similar to that observed for M . That is, all else equal,

¹¹ Because of space limitations, figures for these experiments are not shown here. Copies may be obtained from the first author.

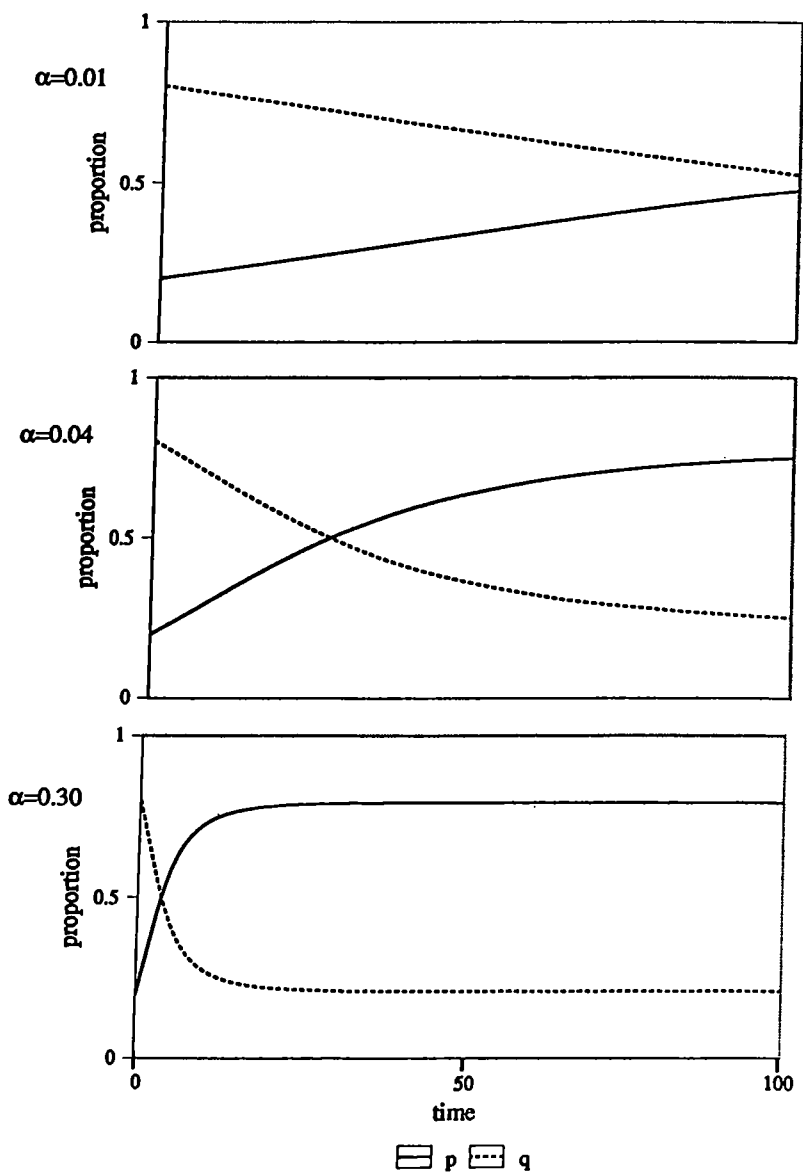


FIG. 7.—Effect of increasing the value of α on the relative proportions of p and q over 100 generations ($M = 10$, $N = 13$, $c = 0.11$, $\gamma = 0.75$).

higher values of α are associated with more rapid movement toward \hat{p} , the point at which the proportion $p:q$ became constant. When α_1 and α_2 were assigned different values, the effect was quite striking. Changing the difference between α_1 and α_2 influenced both the location of the equilibrium and the rate at which the population reached this point. Figure 8 shows the results of three successive treatments over time.

In a general sense, the results of this set of experiments are consistent with Cohen and Machalek's hypothesis that the replica population in which expropriative strategies are more easily transmitted (i.e., where M is greater) will tend to have a higher crime rate. However, since there can be stable internal equilibria, this hypothesis should be amended to reflect the fact that increases in the rate of strategy growth in a stable system that are due solely to increases in the quality of information influence the speed with which equilibrium is reached, but *not* the location of the equilibrium itself. This suggests that the population in which expropriative strategies are more easily transmitted because individuals tend to have more interaction and sample more models will experience higher crime rates while an expropriative strategy is new. Once an equilibrium has been reached, however, the rates should be equivalent in both populations. As will be discussed later, this issue is probably of little importance in the real world where the number of possible expropriative strategies is very large. When α_2 was increased relative to α_1 , a higher proportion of $q:p$ existed at equilibrium. The strength of this effect diminished as M increased. This behavior in the model is analogous to differences in belief systems between two populations. As opposed to changes in M that influence only the rate at which equilibrium is reached, changes in the difference between α_1 and α_2 can indeed increase a population's crime rate, as was hypothesized. The implications of these findings are discussed below.

Hypothesis 3

Several of the hypotheses proposed by Cohen and Machalek must be considered together here because the differences between them are too fine to be accounted for separately by the model. Some of these hypotheses involve factors that facilitate or inhibit the effectiveness of defensive counterstrategies. Others address expropriation costs directly. Nonetheless, it is possible to examine these predictions by observing that all would prove false if the following hypothesis is falsified: All else equal, the crime rate will increase when the net material, social, psychological, and biological execution costs of employing expropriative strategies diminish.

Like the previous hypothesis, this one provides a good example of how

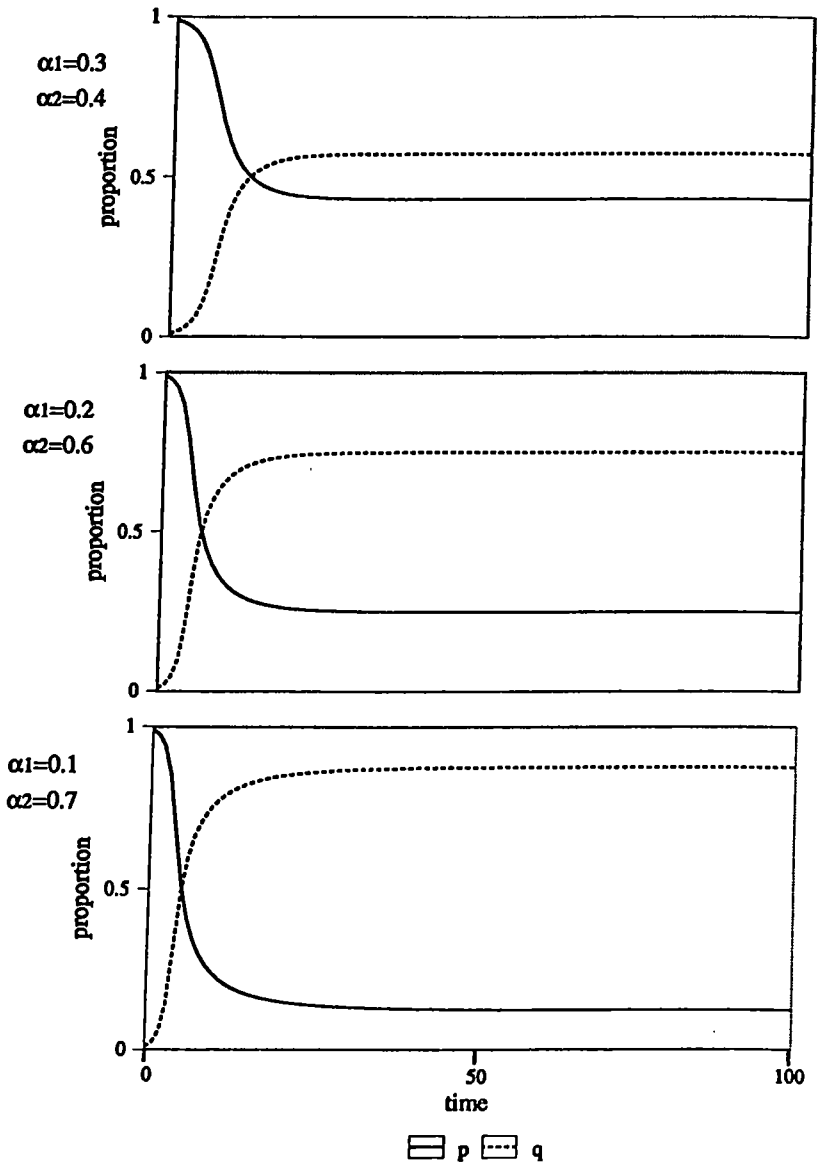


FIG. 8.—Effect of increasing the difference between α_1 and α_2 on the relative proportions of p and q over 100 generations ($M = 5$, $N = 12$, $c = 0.25$, $\gamma = 0.8$).

simulation models may be used to gain theoretical insights. We may explore the effects of changes in expropriative costs by measuring changes in the proportion of producers associated with changes in the cost constant (c). It is obvious from equation (5) that larger values of c indicate less depreciation of expropriated resources. This, in turn, produces an increase in expropriative payoffs (w_2). As w_2 increases, so does the proportion of expropriators. Figure 9 illustrates that as c increases (i.e., as costs decline), the equilibrium point drops to lower proportions of S_1 individuals. A second, perhaps less obvious, effect is that the *rate* of change in w_2 slows as $p \rightarrow \hat{p}$. Note that some of the more interesting system behavior, such as multiple internal equilibria, are obtained when expropriated resources are depreciated at a realistic level, that is, between 0.1 and 0.2 (e.g., figs. 3b above and 10a–c below). In the “real world,” stolen goods are often “fenced” at around 10 cents on the dollar.¹²

Hypotheses 4a and 4b

Two additional hypotheses examined here involve the theory's predictions that, *ceteris paribus*, expropriative crime rates will be higher in a replica population: (a) in which there is less competition between expropriators and (b) where expropriative strategies yield higher returns. The negative effect of increased competition among expropriators on the mean payoff they receive (w_2) is specified in the development of equation (2). As equation (2) shows, higher returns to expropriation can result from increasing p , c , or γ , all of which cause w_2 to increase. The effects of changes in w_2 , p , and c were examined earlier. Experiments conducted to test the sensitivity of the model to changes in the fraction of each producer's resources taken by expropriators (γ) did not produce any unusual behavior. Increases in γ produce increases in w_2 and decreases in w_1 so that the equilibrium point at which they intersect shifts toward lower values of p . This shows that the proportion of S_2 strategists will be greater at equilibrium for higher values of γ . Both of these findings are consistent with hypotheses 4a and 4b.

¹² Interestingly, a parallel has been observed in biological systems where the efficiency with which energy is transferred from one trophic level to the next is usually about 10%–20%. For example, 100 units of potential energy in grass consumed by beef cattle produce 10–20 units of potential energy. When humans consume the cattle, 1 or 2 units of potential energy are derived from those same 100 units initially put into the system by grasses. This is thought to be one of the reasons predators at the top of the food chain are so rare (Pianka 1983, p. 295).

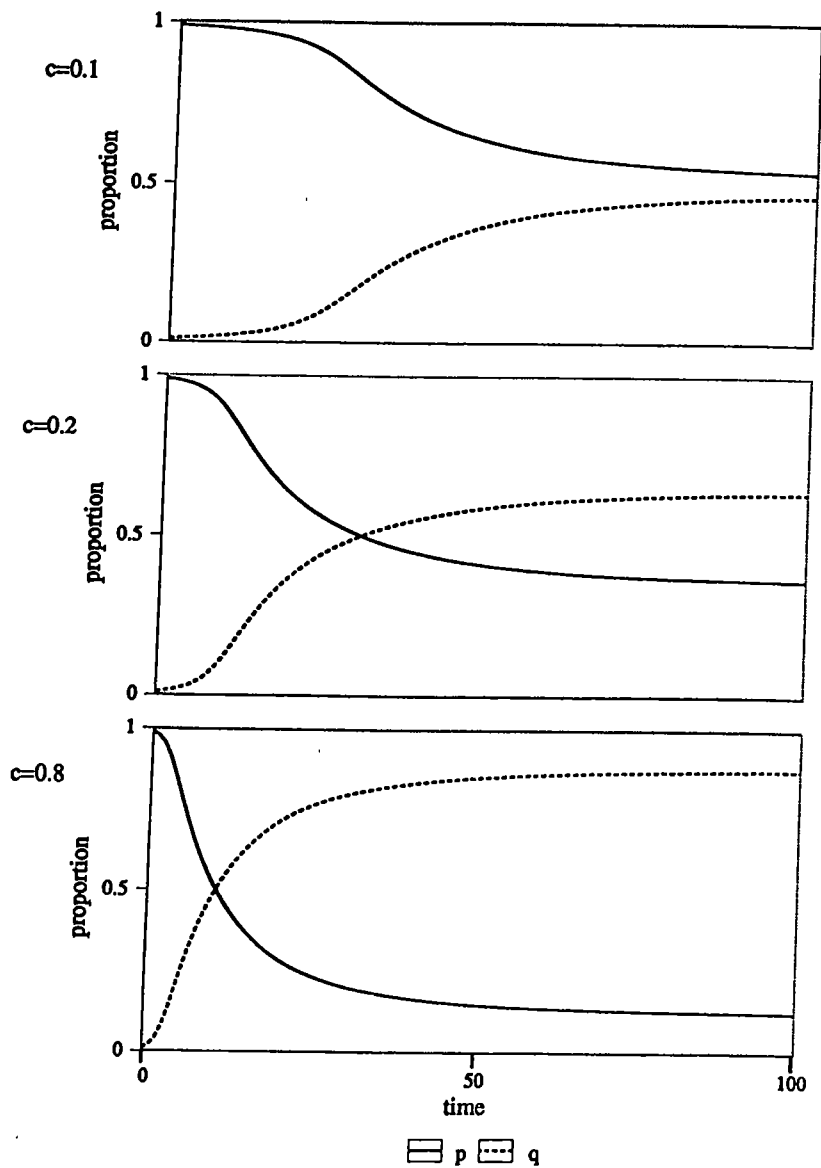


FIG. 9.—Effect of increasing the value of c on the relative proportions of p and q over 100 generations ($M = 5$, $N = 20$, $\alpha = 0.1$, $\gamma = 0.9$).

DISCUSSION

Game-theoretic models such as we have used here provide an opportunity to test subjective assumptions experimentally. Like automotive engineers with a scale model of a new race car, we can tweak the controls, examine the effects of various modifications, and determine under what conditions it is most likely to spin out of control. All of these activities add to one's understanding of the system. After simulating reality with a model, however, it is always appropriate to return to the realm from which the motivation to model arose and ask "What may we realistically conclude on the basis of these simulations?"

In a real world, people do not march in lockstep through a difference equation. Nor are they limited to just two resource acquisition strategies. In the real world the watchword is *diversity* not uniformity. Instead of the double-stranded dynamics described by this model of expropriative crime, we are faced with a spaghetti-like tangle of relationships. Having developed some understanding of the possible dynamics involving two strands, let us briefly examine the implications of these dynamics for the whole system. Two different perspectives suggest themselves. Appropriately, one is ecological, the other evolutionary.

Ecological Implications

From an ecological perspective, we note the importance of resource and behavioral strategy diversity. Our analysis indicates that as more resource niches are exploited by members of a population using different productive strategies, more opportunities for the application of expropriative strategies tend to be created. Opportunistic factors, as well as asymmetries of ability and/or need, may therefore motivate individuals to acquire and employ various expropriative strategies. This allows them to shift between expropriative niches as payoffs change. On the basis of the theory's hypotheses and the behavior of the model studied here, we might expect that this would tend to diminish direct competitive effects in any single expropriative niche. Since the shape of the competition/payoff curve describes a negative function, more niche dimensions would tend to allow a given number of expropriators to obtain higher payoffs (or tolerate higher costs). All else equal, we would expect to find proportions of expropriators that were quite small and payoffs to expropriators that were relatively high along any one niche dimension. Under such conditions, a stable equilibrium might eventually be reached between proportions of producers and expropriators. In the real world, however, equilibria tend to be much more difficult to reach because (among other things) the accuracy of information about the payoffs to different strate-

gies is limited, and because different transmission *lags* may be associated with information about different strategies.¹³

Evolutionary Implications

From an evolutionary perspective, lag effects tend to produce fluctuations in a system over time. As the level of expropriation increases in a relatively stable system, payoffs decline due to competition. Defensive counterstrategies therefore tend to remain constant as a system approaches equilibrium. Many of the figures presented thus far illustrate this pattern. When fluctuations are induced due to informational lags, however, expropriation may increase beyond equilibrium levels. If the magnitude of these fluctuations is not too great, the system will eventually settle down at or around an equilibrium point. Larger fluctuations, however, may "blow the system apart." Figures 10a, 10b, and 10c nicely illustrate the effect of introducing lag terms into the model by using nonexceptional parameter combinations after changing equation (4b) to read:

$$p^{t+1} = p^t - p^t z^t q^t (\alpha_2 w_2^{t-8} - \alpha_1 w_1^{t-3}).$$

The assumption underlying this equation is that information about cryptic behaviors such as criminal expropriation may be expected to be more difficult to acquire than information about productive strategies. In this specification, information regarding the payoffs to expropriation is lagged eight time steps, while information regarding the payoffs to production lags by only three time steps. Note that informational lags can produce effects that are strong enough to force a system that would otherwise be stable away from equilibria. In this situation, any perturbation away from an equilibrium will cause the system to begin oscillating again. These types of perturbations might occur, for example, if the payoffs to an expropriative strategy (such as picking pockets) increased occasionally due to irregular public events such as unusual conventions, sporting events, or other entertainment events.

In the real world, lag effects may also be expected to affect defensive counterresponses (e.g., information about increasing expropriative threats may cause the level of guardianship to increase). If this information is lagged, the effort devoted to defensive counterstrategies may be greater than required by strict economic rationality. As with lagged information about resource payoffs, this may cause the proportions of both expropriative and productive strategies to oscillate wildly. When one

¹³ Botkin (1990) reviews a substantial body of empirical evidence indicating that, in complex systems involving living organisms, equilibria are seldom, if ever, actually achieved and that the perception of stability tends to be an artifact of scale.

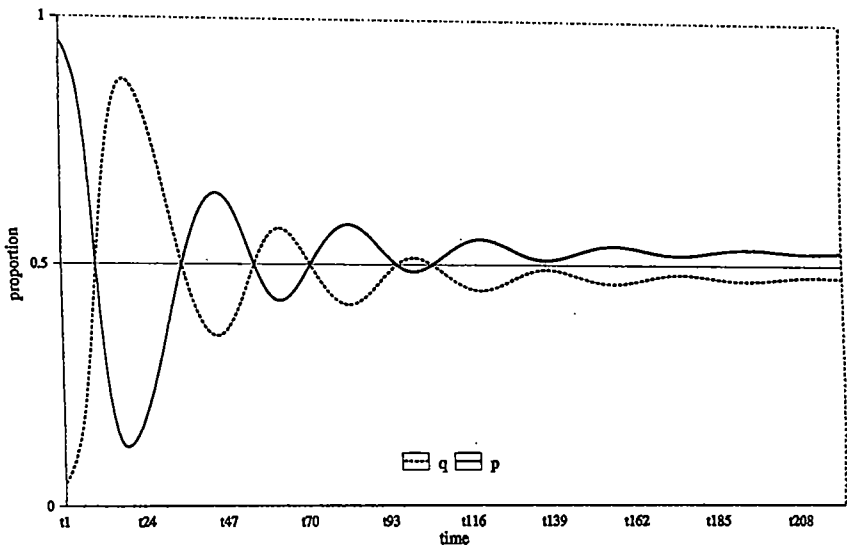


FIG. 10a.—Lags in information about relative payoffs to production (lag for $w_1 = t - 3$) and expropriation (lag for $w_2 = t - 8$) induce irregular oscillations in both the magnitude and rate of change in the relative proportion of strategies S_1 and S_2 . In this system, the oscillations dampen out at or near $t300$ ($M = 5$, $N = 12$, $\alpha_1 = 0.2$, $\alpha_2 = 0.3$, $c = 0.15$, $\gamma = 0.8$).

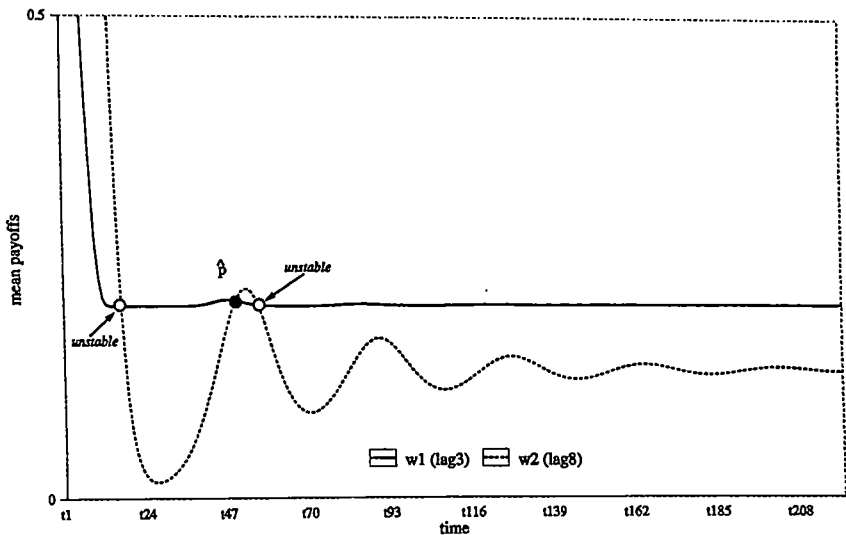


FIG. 10b.—Information lags (here $w_1 = t - 3$ and $w_2 = t - 8$) can produce effects which are strong enough to force a system away from equilibria that would otherwise be stable. Because of this, the system never reaches equilibrium and, after $t50$, any perturbation will renew oscillation. Without further perturbation, oscillations dampen out at or near $t300$ ($M = 5$, $N = 12$, $\alpha_1 = 0.2$, $\alpha_2 = 0.3$, $c = 0.15$, $\gamma = 0.8$).

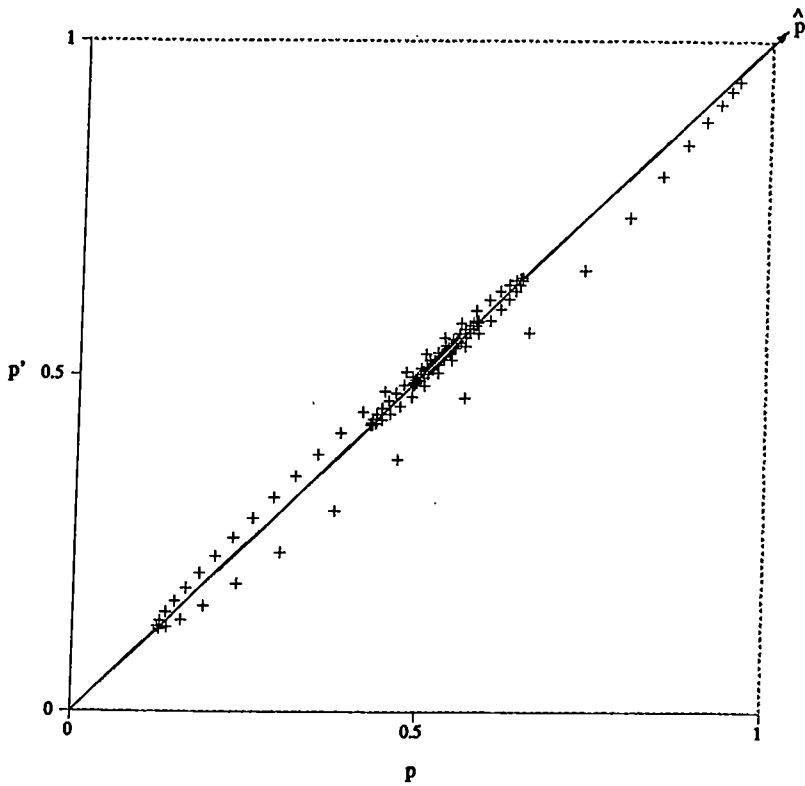


FIG. 10c.—Phase diagram of the effect of information lags (here $w_1 = t - 3$ and $w_2 = t - 8$) illustrates how the proportion of producers (p) spirals down from an initial value near 1.0 to a low near 0.2 then spirals back up and around the equilibrium vector at or near 0.5 ($M = 5$, $N = 12$, $\alpha_1 = 0.2$, $\alpha_2 = 0.3$, $c = 0.15$, $\gamma = 0.8$).

considers these types of oscillatory behavior occurring along many productive/expropriative niche dimensions, the ubiquitousness of expropriative crime seems “normal” indeed. Note that, even though we continue to rely on a simple model to guide our thinking, the picture that now emerges appears much more tangled and complex. It also appears to be more realistic.

SUMMARY AND CONCLUSIONS

The essence of our approach has been to use mathematical models and the brute strength of the computer to simulate real-world behavior. This

allowed us to test key hypotheses from Cohen and Machalek's general theory of criminal expropriation and clarify the complex relationships upon which it is based. Computer simulation is a suitable research strategy when one is studying a complex problem in which the direction of effects on the dependent variable is not well understood. By observing the model's behavior under repeated simulations, it may be possible to gain a greater understanding of one's subject matter. Simulations thus provide an economical method of making certain kinds of phenomena accessible that might otherwise be difficult or impossible to study. Game-theoretic simulation models such as we have employed here provide a method for refining theoretical formulations and testing hypotheses experimentally. They also perform the useful function of identifying inconsistencies and ambiguities in theoretical structures and are most appropriate for examining contest situations such as crimes that involve competition for scarce resources.

We have addressed several questions associated with the system dynamics proposed by the theory. The most fundamental question was whether the proposition that expropriative crime would persist due to frequency-dependent payoffs was *possible*. An evolutionary game-theoretic model was developed around a set of revised assumptions about resource competition, behavioral strategy acquisition, and differences in individual motivation arising from asymmetries of ability and/or need. While expropriation was treated as being negatively frequency dependent in our revised version of the theory, no frequency-dependent effect was specified for production. Based upon mathematical analysis of the system of equations that define the model, the theory's explanation for why crime is normal appears to be both possible and plausible. Additional insights were gained when the model was used to conduct simulation experiments, although it generally behaved as predicted throughout the wide range of possible parameter values tested.

Cohen and Machalek claim that their theoretical perspective allows the formulation of novel insights and hypotheses about expropriative crime (1988, pp. 496–97). In order to support this claim, they propose a number of hypotheses about criminogenic effects arising from strategy evolution and dynamics. Recall that these hypotheses derive from comparisons of two populations that are exact replicas of one another insofar as most of the factors that have commonly been used to explain crime are concerned. They predict that expropriative crime rates will tend to be higher in the otherwise identical population where (1) more opportunities for expropriation exist because of productive innovation, (2) expropriative strategies are transmitted with greater facility, (3) the execution costs of employing expropriative strategies are lower, and (4) competition

among expropriators is less intense or the yield of expropriative strategies is greater.

These hypotheses were evaluated using computer simulations to model changes in the relevant parameters over many generations and at many different initial proportions of producers and expropriators. Experimental results were consistent with hypotheses 1, 3, 4a, and 4b. On the basis of our analysis, however, the second hypothesis must be modified: Increasing the rate of strategy growth because of improved information quality apparently influences the speed with which equilibrium is reached, but not the location of the equilibrium point itself. This suggests that the population in which expropriative strategies are more easily transmitted will experience higher crime rates *only while an expropriative strategy is new*. As equilibrium is approached, however, the rates should tend to become equivalent in both populations. As predicted in hypothesis 2, expropriative strategy growth increases when transmission biases favor expropriation. Analysis of the model under a wide range of parameter values demonstrated that its behavior is consistent with Cohen and Machalek's theory; this indicates that it was correctly specified.

The main purpose of our analysis was to challenge the theory at a fundamental level. While tests based upon numerical experimentation and mathematical analysis are incapable of confirming the theory, they are capable of falsifying it. Our analysis leads us to conclude that the system dynamics proposed by Cohen and Machalek to explain expropriative crime as a normal outgrowth of routine economic, social, and productive interactions in naturally functioning societies are both reasonable and possible. Future empirical research oriented toward establishing whether or not the effects they propose actually occur outside the laboratory appears to be warranted.

Two lines of research seem appropriate for assessing the theory's theoretical and practical utility. First, from a theoretical standpoint, an empirical test of the theory's core assumption that increased productive strategy diversity allows crime to increase by providing more opportunities for expropriation seems to be a logical next step. Although it is as yet unclear how best to operationalize productive strategy diversity, two approaches seem promising. The first might employ some measure of economic output per capita. Baumol, Blackman, and Wolff (1989, p. 227), for example, suggest gross domestic product (GDP) as a useful indicator of prospective consumption. The rationale behind such a measure is that increases in productive strategy diversity tend to be related (at least in part) to increases in the complexity of productive technologies that, in turn, are positively related to per capita income. Increases in per capita income tend to lead to more spending for durable goods which

can be expropriated (see Cohen and Felson 1979). An alternative approach would be to operationalize productive strategy diversity as occupational diversity and compare expropriative crime rates for MSAs with different levels of occupational diversity. In this instance, ecological theory indicates that it would be important to attend to both the number of different types of occupations and the relative distribution of the population among those types. (See Thiel 1967, pp. 92–134; Pielou 1975; Allison 1978; Begon, Harper, and Townsend 1986, pp. 594–97; May 1986; Magurran 1988; Walker 1988, pp. 115–45.) The first approach, while rather indirect, has the advantage of being amenable to longitudinal analysis and might therefore provide more information about the evolution of strategies over time. Longitudinal data for the second approach are not regularly collected and thus cannot provide information about strategy evolution. The specification for the second approach, however, is much more direct than that for the first.

Second, to the extent that the model employed here captures the shape of relationships between key factors associated with expropriative crime, additional simulations and analysis may assist in the formulation of crime control policy. For example, one of the critical questions in criminology is to what degree a person's enduring individual traits affect her propensity for crime. Although the model developed here is neutral on this question, it does provide some insights that have important policy implications.¹⁴ Under this model, a higher crime rate could be due to either a greater number of individuals employing expropriative strategies (increased prevalence) or a relatively constant number of individuals expropriating with greater frequency (increased incidence). Our analysis indicates that changes in transmission bias that favor expropriative strategies (increasing α_2) are positively related to the proportion of expropriators in a population. This finding suggests that the prevalence of expropriation could change in response to changes in beliefs about expropriative strategies (see Hirschi 1969). One of the insights gained from this research, however, is that the frequency with which expropriative strategies are employed may be largely *independent* of expropriator prevalence.

¹⁴ As with most game-theoretic models, behavioral strategies can be conceived of as *either* alternatives that all people adopt at p and q proportions of time (pure strategies); or as strategies that $p\%$ and $q\%$ of the population always adopt (mixed strategies). The mathematical equivalence of these conceptions when there are two strategic options means that the model is neutral on the question of how much a person's enduring traits affect her propensity to expropriate. As Maynard Smith (1982, pp. 17, 183–88) points out, this equivalence is not necessarily found if there are more than two strategic options. In that instance, the results for a population employing mixed strategies can be stable while the results for a corresponding pure strategy population are unstable, and vice versa. See also Cohen and Machalek (1988, pp. 473–75).

If this observation is valid, it could help policymakers assess the relative desirability of different approaches to controlling crime (e.g., public education programs designed to change people's behavior may reduce the prevalence of crime, while those that affect expropriative costs/rewards may have more influence on frequency). An evolutionary ecological approach provides a structure for addressing these issues holistically so as to include both micro- and macrolevel processes. We are not aware of any other contemporary theoretical approach that appears to integrate these processes as parsimoniously as does the theory we examined.

APPENDIX

Following a proof used by Peck (1988) for a similar set of equations, it is possible to show that

$$\sum_{k=0}^{N-1} \binom{N-1}{k} \left(\frac{kc\gamma}{N-k} \right) p^k q^{(N-1-k)} = c\gamma p \left(\frac{1-p^{N-1}}{1-p} \right).$$

The right-hand term may be expressed more explicitly as

$$\sum_{k=0}^{N-1} \left(\frac{(N-1)!}{(N-1-k)!k!} \right) \left(\frac{kc\gamma}{N-k} \right) p^k q^{(N-1-k)}.$$

In turn, this may be rewritten to give

$$\sum_{k=0}^{N-1} \left(\frac{(N-1)(N-2) \dots (N-k+1)(N-k)(N-k-1)!}{(N-1-k)!k!} \right) \times \left(\frac{kc\gamma}{N-k} \right) p^k q^{(N-1-k)},$$

canceling and rearranging yields

$$c\gamma \sum_{k=0}^{N-1} \left(\frac{(N-1)(N-2) \dots (N-k+1)(N-k)!}{k!} \right) k p^k q^{(N-1-k)}.$$

This is the same as

$$c\gamma \sum_{k=0}^{N-1} \left(\frac{(N-1)!}{k!(N-k)!} \right) k p^k q^{(N-1-k)}.$$

Since the first term in the numerator of the series equals zero, we may rewrite this as

$$c\gamma \sum_{k=1}^{N-1} \left(\frac{(N-1)!}{k!(N-k)!} \right) k p^k q^{(N-1-k)}.$$

This reduces to

$$c\gamma \sum_{k=1}^{N-1} \left(\frac{(N-1)!}{(k-1)!(N-k)!} \right) p^k q^{(N-1-k)}.$$

Reducing further gives

$$c\gamma \sum_{k=0}^{N-2} \left(\frac{(N-1)!}{k!(N-1-k)!} \right) p^{k+1} q^{(N-2-k)}.$$

Finally, this may be rewritten to yield

$$\frac{c\gamma p}{q} \sum_{k=0}^{N-2} \left(\frac{(N-1)!}{k!(N-1-k)!} \right) p^k q^{(N-1-k)}.$$

In view of knowledge of the binomial probability distribution (e.g., Pearson 1974, p. 89; Clapham 1990, pp. 15–16), this may be interpreted as the product of

$$\frac{p c \gamma}{q},$$

and the probability of less than $N - 1$ successes in $N - 1$ Bernoulli trials, where the probability of a success during a particular Bernoulli trial is p . The expression may therefore be rewritten to give the desired expression:

$$c\gamma p \left(\frac{1 - p^{N-1}}{1 - p} \right).$$

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Book Reviews

Feminist Methods in Social Research. By Shulamit Reinharz with Lynn Davidman. New York: Oxford University Press, 1992. Pp. viii + 413. \$39.95 (cloth); \$19.95 (paper).

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Feminist Methods in Social Research claims a prime location on my "good books" shelf. I think anyone remotely interested in feminism or research methods will want a copy. It will be an excellent addition to a graduate methods or feminist methods course. There is no other book like it; it chronicles, nourishes, and leads the way. It is at once our history and our future.

Shulamit Reinharz asks, What is feminist research? She answers through an ambitious, painstaking, inductive analysis of what self-identified feminists do to produce knowledge. Rather than trying to codify a set of procedures, she catalogs the kinds of methods that feminists use. Rather than telling feminists what to do, she shows what we have been doing. She wants to find ways to produce knowledge without reproducing patriarchal ways of knowing.

The text consists of 13 chapters, 80 pages of engrossing notes, and 50 pages of feminist bibliographic references. Sociologists, psychologists, economists, and other women's studies scholars are cited. In 11 substantive chapters, Reinharz reviews methods used by self-identified feminists in interviewing, ethnography, survey research and statistical research formats, experimental research, cross-cultural research, oral history, content analysis, case studies, action research, multiple methods research, and original feminist research. Because Reinharz works inductively, taking her analytical lead from the expressed concerns of the feminist researchers, each of the substantive chapters has a different topical heading. In general, however, the chapters provide discussions about historical roots of the particular method; specific feminist issues, critiques, and revisions; ambivalences and dilemmas; consequences of the feminist method for mainstream social science, feminist research, social change, and the feminist researcher.

The concluding chapter is a meta-induction in which Reinharz identifies 10 aspects of feminist research: (1) it is a perspective in which there are (2) a multiplicity of methods and (3) an ongoing criticism of nonfeminist scholarship (4) guided by feminist theory and (5) transdisciplinary perspectives and oriented toward (6) social change, (7) human diversity, (8)

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self-reflexivity, (9) connectedness to the people studied, and (10) to the reader of the research.

For me, however, the value of *Feminist Methods in Social Research* is not captured by an account of the scope and content of the text. I was moved by the histories of feminist women in the development of both qualitative and quantitative social scientific methodology. I was excited by the methodological inventiveness of others. Sometimes I felt I was reading three texts at once—the main text, the relevant notes, and the bibliography. My copy of the book is now quite marked up, color coded, dog-eared, and tabbed. Reading was, thus, an adventure; the outcome is a personalized reference book.

With a text as ambitious as *Feminist Methods in Social Research* there will undoubtedly be quibbles and complaints; perhaps even hurt feelings on the part of feminist researchers not chronicled. Given the vastness of Reinharz's task, however, I believe we can only thank her for her contribution to our ongoing dialogue about feminist research and forgive her any omissions. Her accomplishment prompts, however, two new tasks.

First, we need a feminist analysis of how research methods, themselves, are named and categorized. What are the consequences of deploying standard (patriarchal) methods categories? Are the distinctions Reinharz makes between interviewing, case studies, and oral history, for example, or the association of survey research with statistical methods, for another, necessary or beneficial? We need to explore how standard research categories and the rhetoric of research mute women's actual research practices, create divisiveness between feminists, and replicate patriarchal ways of knowing.

Second, Reinharz's text does not cover some of the newer feminist methods. Much of feminist-postmodernist research is not discussed in her book, and many of the issues of feminist-postmodernist researchers (such as how writing genres limit and control women's lived experiences) are not presented. The task now is to inductively analyze research by self-identified "feminist-postmodernists." I believe that we will find considerable diversity among these researchers and that an analysis of their work will prove liberating.

Reinharz's goal is to demystify feminist research and to contribute to the ongoing conversation among feminists and between feminists and antifeminists. In my reading, she has reached her goal. She has explored diversity among feminist researchers, pointed to our commonalities, and held fast to a belief in the possibility of a self-correcting feminist renaissance, built in gratitude upon the work and lives of other feminists.

What's Wrong with Ethnography? Methodological Explorations. By Martyn Hammersley. New York: Routledge, 1992. Pp. 230. \$49.95.

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Martyn Hammersley is worried. According to him, ethnography now is widely accepted, criticisms that it is not scientific have subsided, and there "may even be a danger that qualitative work will largely supplant quantitative research" (p. 123). Yet there remain unresolved issues from earlier disputes between qualitative and quantitative researchers, and there are perplexing questions about ethnography's ability to represent reality and about relationships between ethnography and practice. With special attention to changes proposed by recent advocates of critical, relativist, and practitioner ethnography, Hammersley sets out to answer the question posed by the title of this volume of essays.

So, what is wrong with ethnography? The first section, "Ethnography, Theory, and Reality," takes ethnographers to task for focusing on description rather than on theory and for being antiphilosophical and therefore not aware of ever-present tensions between realism and relativism. Hammersley believes that recent antirealist trends do not offer an answer to this dilemma because relativism collapses into circularity and this leads to the loss of the ethnographer's authoritative voice. Although he is pessimistic about any resolution to the realism/relativism tension in ethnographic works (p. 42), he does offer his own model of "subtle realism" to replace dichotomous thinking.

Hammersley believes that typical ways of judging ethnographic quality via either quantitative or qualitative criteria are insufficient. He defines "good research" as providing "information that is both true and relevant to some legitimate public concern" (p. 68). Although he criticizes ethnographers for examining sites that rarely have intrinsic relevance and while he raises general questions about the capacity of ethnography to produce relevant results (p. 77), part 2, "Ethnography, Relevance, and Practice," turns to the issue of evaluating ethnographic quality as measured in terms of "truth" and "relevance." In these chapters, Hammersley argues that there is no evidence that ethnography is generalizable, and he accuses ethnographers of exaggerating the likely validity of their work "both in general and in comparison with other kinds of social research" (p. 124). Again, proposed modifications do not offer a route to validity: critical theory is described as not theoretically cogent, "practitioner ethnography" is criticized for merely assuming that practitioners have correct judgments while ignoring the importance of the research community serving as the ultimate judge of research quality and relevance.

Part 3, "Qualitative versus Quantitative Method," challenges taken-for-granted differences between qualitative and quantitative data and between experimental, survey, and ethnographic research. Here, the

stated goal is to alert researchers that there is a complex array of methodologies and that "retreat into paradigms effectively stultifies debates and hampers progress" (p. 182). Hammersley argues that qualitative and quantitative methods are not so very different, so research strategy should depend on the specific purposes and circumstances of the research. In the final main chapter, "So, What Are Case Studies?" he proposes that ethnography is synonymous with "case study" and that "case study" might profitably be understood as a "case selection strategy," which may or may not imply qualitative data or a focus on meaning.

Hammersley raises the question What's wrong with ethnography? and his answers leave little room for optimism that problems can be easily fixed. Variouslly defining ethnography as synonymous with the "qualitative method" (p. 8), as "those kinds of research . . . that aim both at accurate portrayal of particular phenomena and at a more general conclusion about types of phenomena" (p. 29), and finally defining ethnography as "case study" and "case study" as "partially synonymous with qualitative research" (p. 183), he concludes that the meaning of ethnography is "not specific enough" and hence it should be integrated into a "more appropriate methodological framework for the social sciences" (p. 203). In a brief postscript he asserts that this collection of essays "effectively undercuts the rationale for ethnography, not only as a separate paradigm, but even as a distinct method" (p. 203).

Needless to say, this book is provocative. Such an examination of epistemological, theoretical, and practical issues underlying ethnographic research should encourage reflection and methodological self-consciousness, it rightfully should spark debate. In this way, the book might well achieve its stated goal of encouraging attention to often unchallenged methodological assumptions. Yet, most clearly, this is *not* a book about epistemological, theoretical, and practical problems underlying all forms of research—it is about ethnography. In the first chapter, Hammersley asserts his personal acceptance of the value of ethnographic research, yet by the end, it is only *ethnography* that is facing a "crisis," it is only *ethnography* that is dissolved as a method and perspective. But my comments here are external criticisms. Hammersley did not set out to examine the problems and perils of all methods, he set out to examine the problems of ethnography. And that certainly is the construction he delivers to his readers.

Issues and Alternatives in Comparative Social Research. Edited by Charles C. Ragin. New York: E. J. Brill, 1991. Pp. 225. \$40.00.

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Charles Ragin begins this book with the assertion, "Good comparative social science balances emphasis on cases and emphasis on variables."

He and others warn of the unhealthy effects of tilting the balance, but they also argue that little actual balancing has yet occurred. Critical of schizoid comparative social science caught "between radically analytic, statistical techniques that obscure cases and qualitative-historical methods that immerse the investigator in cases" (p. 3), Ragin focuses this collection of nine papers on making readers sensitive to the problem and suggesting concrete ways to bring about a better balance.

The objective behind this volume is admirable, and all of the chapters contribute to it to some degree. Four of them review substantial subfields, while pointing the way to a new, balanced or synthetic approach to resolve inconsistencies and advance scholarship. Dietrich Rueschmeyer observes that the two radically different traditions of cross-national statistical work and comparative historical studies have produced sharply different findings on the much-studied relationship between capitalist development and democracy. Giving more weight to comparative historical research, he appeals for a strategy called *analytic induction* to test and retest theoretical generalizations through detailed case analyses. David A. Smith likewise finds a sharp methodological divide in the field of comparative urban research, and he proposes a synthetic theory based largely on qualitative research and comparative case studies aimed at constructing integrated explanations to account for seemingly unrelated facts. Timothy P. Wickham-Crowley and Larry J. Griffin and his three coauthors advocate a qualitative comparative approach, respectively, to Latin American revolutions and to trade union growth and decline in large, stable, capitalist democracies.

Clearly this collection is weighted toward an emphasis on cases and qualitative analysis. As Thomas Janoski concludes, "The qualitative theoretical explanation will inevitably be richer and more complex than the quantitative analysis. The quantitative analysis will often appear like a skeleton next to the qualitative analysis" (p. 75). Three of the chapters are concerned with making the most of a case study. According to York Bradshaw and Michael Wallace, a case study helps to inform general theory, such as by pinpointing exceptions to broad arguments; to overcome ethnographic bias, which often affects study of Third World countries, Edwin Amenta advocates five ways to strengthen historical case studies, and Karen Barkey seeks to reconstruct Ottoman village networks as a case study through comparisons with the paradigmatic case of France. Through such strategies, readers are encouraged to combine cases with theory, to bridge the gap with quantitative studies, and to feel that they should not have to choose between either extreme in comparative studies.

While the various authors propose somewhat different approaches for achieving balance specific to their own research interests, the volume reveals considerable consensus about the history and direction of development in comparative social research. The first element in this shared outlook is hostility to modernization theory. Smith puts it most strongly, referring to "debunking the developmentalist assumptions of the old

modernization theory/ecological approach" (p. 39). The renaissance of historical sociology in recent decades is seen as overcoming rather than building on modernization theory. Yet authors seem to open themselves to contradiction when they acknowledge that the modernization vision fits well with various quantitative findings or is a useful model for some regions (p. 159). Second is the favorable view of dependency and world-system theory as a tool for analyzing macrostructural change. Some doubts about this perspective emerge, however, such as the warning that it "cannot be transferred blithely to Africa" (p. 161).

Third, a rather sharp line seems to be drawn between the comparative sociologist and the area studies specialist, including one suggestion that research be subcontracted to assisting country and area specialists (p. 77). Wickham-Crowley adds that he has "shunned the 'area studies' approach which ignores general theory in its rush to establish the supposed uniqueness of each case, each event" (p. 104). Claims that the desired balance will produce historical accuracy and holistic analysis of cases not ripped out of context seem inflated without more attention to area competencies. Fourth, the critique of "mechanical" statistical approaches is clearly more vigorous than that of "descriptive" historical case studies. While various authors insist that case studies are inherently theoretical, at least one comments disparagingly on hypothesis testing for excluding the context and because it must work with crude and ambiguous indicators (p. 31). Portions of the history of postwar comparative studies that appear in many of the chapters also may leave some readers with questions about one-sidedness or about exaggerated claims of what has already been achieved.

What is most valuable in this collection is the shared concern with practical steps toward finding solutions to research problems. John Bynner and Walter Heinz offer an example of this, concentrating, in their chapter comparing labor market entry in England and West Germany, on methodological issues and using quantitative and qualitative methods in tandem. It is helpful to share approaches, to discuss ways to make them more explicit and systematic, and even to find labels such as "qualitative comparative analysis" for some set of methods. In my opinion, however, it would be premature to accept a particular version of what has been accomplished or what works for the general benefit of the field. The main message that we should strive for balance is more compelling than what I take to be the implicit message that we should coalesce behind a particular approach.

Revolution and Rebellion in the Early Modern World. By Jack A. Goldstone. Berkeley and Los Angeles: University of California Press, 1991. Pp. xxix + 608. \$34.95.

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The first half or so of the 17th century was a time of remarkably widespread regime crisis in which inraelite struggles and popular rebellions joined together to challenge governing authorities and sometimes overthrow them. Spectacular instances ranged geographically from England and France to Russia, China, and the Ottoman Empire. Following a calmer interlude, another wave of crises began in the late 18th century. Jack Goldstone has attempted to account for such waves of "state breakdown," in the course of which he develops a remarkably parsimonious explanatory framework for revolutions and quasi revolutions, suggests an ingenious way to join cultural and structural elements of these crises together in a systematic fashion, and provides a model for one way of using comparative research to discover exciting questions.

The engine propelling these instances of crisis was steady population growth, whose multiple and insidious effects outpaced the limited flexibility of institutions in the early modern world in three mutually reinforcing ways. First of all, the demands placed on state services by larger populations utterly defeated the capacity of state managers to overcome the highly traditionalized rigidities built into state taxation systems: revenue crises followed. Second, rising numbers of elite progeny outran the capacities of early modern institutions to expand the availability of suitable elite posts. This led to inraelite struggles for position, the development of social criticism of the follies of the existing order, and reactive defenses of tradition by other elite elements. Third, rising numbers generated mass immiseration through land fragmentation, rising rents, falling wages, and rising food prices. The three processes exacerbated each other as increasingly hard-pressed state managers, elites, and lower classes attempted to solve their problems at the expense of the increasingly resistant others. These processes were not only complexly intertwined but were insidious in the sense that those in command of relevant resources were most unlikely to realize the deep processes underlying the revenue shortfalls, elite dissent, and popular resistance that they had to cope with on an everyday basis. (Not the least of this model's many virtues is that it may spare us many doxy debates over whether revolutions are really elite or mass phenomena.)

Goldstone attempts to make his case in several complementary ways. He presents close analyses of the unfolding of the demographic/structural process leading up to the English and French revolutions. He also examines two crises at considerable geographic remove (China, Ottoman Empire). And he then examines the timing of such crises all over Europe in relation to the timing of periods of sustained population growth.

The analyses of the English and French revolutions include an attempt at quantitative measurement of the stresses implied by the model. The results impressively show this measure to peak at the point of regime collapse. The act of measurement helps reveal one of the great distinctions between what the demographic/structural theory is particularly successful at explaining, namely, the timing of crises, and most other broad theories that may elucidate the nature of the parties in conflict but rarely have much to say about timing. (Marxist theories, for example, are enormously rich in claims about who the antagonists are likely to be in moments of crisis and notoriously poor in explaining or predicting the timing of that crisis.) Goldstone's theory is fundamentally a theory of the timing of events. It provides a framework within which the empirically existing specific conflicts can be displayed, but the theory itself does not tell us much about the lines along which elites might divide nor whether the hard-pressed tax collectors will redouble their efforts to get a bit more from the poor and weak or tackle the entrenched privileges of the powerful.

The boldness of the overall framework is combined with a command of detail of the particular cases that is quite unusual. His account of France's financial difficulties, to take a particularly striking example, is a creative synthesis on its own. Goldstone makes a powerful case for the profundity of a general rigidity that overwhelmed the state's capacity to maintain its peacetime activities (and thereby challenges a thesis of semifortuitous revolutionary causation: "They stupidly fought one war too many").

Since the crisis-generating engine is a process whose ramifications are often only dimly apprehended by all parties and since the restructuring of class and state relations do not have all that much to do with the underlying process, the moral view of revolutions here is essentially tragic: revolutions do not come from the nefarious actions of evil elites or misled masses but from a structural problem that inflicts pain on all corners of society. It is an account of revolution much more remote from the understandings of participants than are, for example, the classical interpretations of Marx and Tocqueville. One can find many elite participants in the French Revolution, for example, who held that the revolution was about freeing new productive forces from the fetters of archaic and constricting practices; many others would readily have agreed that the revolution stemmed from the delegitimation of existing hierarchies under the impact of the state's continual appropriation of responsibilities once carried by elites. It does not seem likely that the Goldstone model easily lends itself to the passionate language of participants. Those who prefer melodrama to tragedy may find Goldstone's new story disappointing.

It is utterly impossible in brief compass to even enumerate let alone reflect on all the many, many insights contained in this thick volume. One idea that I would hope to see occasion much discussion deals with

the divergent paths taken at points of state breakdown. One readily speaks of an English or a French revolution, yet we also speak of an Ottoman "crisis" from the late 16th century into the 17th century and of a "dynastic transition" growing out of a turbulent mid-17th century Chinese situation. Goldstone contends that the basic framework of interlocked fiscal crisis, elite conflict, and mass mobilization characterized the two Asian instances as much as they did the European ones. Indeed, there is a case that structural changes of a sort commonly associated with revolutions—a reorganization of government, alterations in class relations—was as characteristic of these Asian instances, perhaps more so, indeed, than in the English Revolution. The great difference was not, then, in the structural aspects of these crises so much as in the way those structures were experienced by those who lived through them. These experiences were filtered through understandings of the world, which is to say, through culture. Those experiencing the multiple and mysterious troubles in China or the Ottoman lands understood them as a part of an ongoing cyclical rhythm of growth and decay; a time of troubles announced the end of one cycle and the commencement of the next. In the Christian West, on the contrary, the ground had long been prepared for people to see ruptures in the fabric of history. The Old Law had been supplanted by the new and, in the foretold future, history as we know it would come to an end. The multiple and mysterious troubles in England were experienced in such apocalyptic terms. (A particular distinction of the French Revolution was to reformulate such concepts in a secular language, which, no longer bound specifically to Christianity, became the basis for a transnational revolutionary culture.) In this argument, then, cultures function as switches that, at moments of structurally similar crisis, send societies down different tracks. The Ottomans and Chinese expected to see a new cycle of essentially the same old stuff and, despite important structural changes, felt they had essentially revived tradition. The English and the French, on the other hand, experienced their crises as the birth of a new social order and were open to a dynamic view of their own future.

Methodologically, Goldstone is oriented to explaining the rough temporal clusterings of state breakdowns. Comparative research, in Goldstone's hands, is not merely oriented to lining up separate cases for observations on similarities and differences; it is oriented to discovering something of the nature of the fabric in which these cases are embedded. The temporal clustering with which this book is concerned is produced by broad, multicontinental demographic shifts which induce similar outcomes rather widely so that there are eras of state breakdown followed by eras of peace. There are other conceivable mechanisms that might account for temporal clusterings in other phenomena (e.g., interactions among the separate cases). The study of such transnational phenomena is at least as important as conventionally conceived comparative studies, but far less commonly practiced with any rigor; Goldstone's book sug-

gests how much can be mined from very simple tabulations that reveal the temporal aspects of such phenomena. In its vision of large processes and its grasp of smaller details, in the simplicity of its central thesis and the richness of the phenomena that thesis illuminates, this is surely the most interesting general statement on revolutions in a long time.

Guerrillas and Revolution in Latin America: A Comparative Study of Insurgents and Regimes since 1956. By Timothy P. Wickham-Crowley. Princeton, N.J.: Princeton University Press, 1992. Pp. xx + 424.

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Timothy Wickham-Crowley's new book is an outstanding work of comparative and historical sociology and a brilliant contribution to the sociological analysis of revolutions. The book, in fact, is not only the best comparative study of revolutionary movements in Latin America, but also, quite simply, the best comparative study of guerrilla movements ever written.

The scope of the book is truly phenomenal: Wickham-Crowley systematically examines 11 cases of guerrilla insurgency in Latin America in order to discover why only Castro's Twenty-sixth of July Movement in Cuba and the Sandinista Front in Nicaragua were able to seize power. The author concludes that these movements succeeded due to five "favorable conditions" that were only found in combination in Cuba and Nicaragua during the period under examination (1956-90). The first of these conditions is the guerrilla movement itself qua organization; these were generally formed, the author argues, by highly educated middle- and upper-class youths in those countries where university enrollments had increased dramatically. Not all of these movements, however, met the second requirement for guerrilla success: peasant (and/or working-class) support. Wickham-Crowley suggests that such support was forthcoming under a variety of social-structural and historical circumstances. (He concludes, accordingly, that previous attempts to identify "the" source of peasant radicalism have been chimerical.) Popularly supported guerrilla movements also require military strength if they are to be successful: this is the third condition necessary for revolution. Here, Wickham-Crowley argues that the armed force of guerrilla movements is largely derivative of popular support and largely independent, therefore, of outside military aid.

Wickham-Crowley emphasizes, however, that not all popularly supported and militarily strong guerrilla movements have succeeded in seizing power. Such insurgent movements have only done so, he argues, where they have confronted a "patrimonial praetorian regime" (or what Wickham-Crowley calls a "mafiacracy") that has lost U.S. support—

these are the fourth and fifth conditions necessary for a successful guerrilla movement. In other words, while a number of Latin American insurgencies have been popularly supported and militarily strong, only the Cuban and Nicaraguan movements confronted corrupt personalist dictators (Batista and Somoza) who became the focus of cross-class unrest that was not easily contained—thanks, in part, to the dictator's own "patrimonial meddling" in the armed forces. Guerrilla movements in Cuba and Nicaragua were thus uniquely situated to ride a wave of cross-class opposition into power.

This brief summary can hardly do justice to the many intricacies of Wickham-Crowley's immensely informative and many-layered analysis. Still, the book is not without its problems. To begin with, the author has surprisingly little to say about how geography or, more accurately, geopolitics (of an *intranational* sort) may contribute to peasant-based insurgencies. More specifically, the book has no analysis of the geographically uneven infrastructural power, including surveillance capacities, of Latin American states. And yet, to take but one example, the guerrillas of the Farabundo Marti National Liberation Front (FMLN) have been strong in El Salvador's northern departments not simply because of the process of peasant uprooting that has occurred there, which Wickham-Crowley emphasizes, but also because those departments are the least densely populated and most isolated parts of the country and, therefore, particularly well suited for specifically *guerrilla* warfare.

Wickham-Crowley's interpretation of El Salvador raises another problem. He argues that the FMLN failed to seize power because of the type of regime ("collective military mixed with democratic") that it confronted and *not* because of the massive U.S. military and economic aid to that country. He implies, in fact, that such aid is totally "irrelevant" for understanding the FMLN's failure (p. 323, table 12-4). But why not argue that Salvadoran guerrillas failed to seize power *both* because of the type of regime they confronted and because that regime received a tremendous amount of assistance from Washington? Wickham-Crowley, to be sure, has an answer to this question: Guatemala. The Guatemalan regime, he rightly points out, is also of the "collectivist military" type and has also been challenged by a popularly supported guerrilla movement; however, guerrillas have been even less successful in that country than in El Salvador, even though the Guatemalan government largely lost U.S. support. For Wickham-Crowley, this seems logically to prove that U.S. aid simply could not have played a role in preventing the FMLN from seizing power. "Other things being equal" in Guatemala and El Salvador, such a conclusion might well be justified. But other things are not equal. As Wickham-Crowley is fully aware, the Salvadoran insurgency has been much more powerful and popular than its Guatemalan counterpart, a fact that rendered U.S. aid extremely important for the survival of the Salvadoran regime.

Country specialists will undoubtedly weigh in with their own quibbles

and amendments to what Wickham-Crowley has to say about their turf. But what they cannot do, it seems to me, any more than students of revolution and political violence more generally, is to ignore this very important book. Wickham-Crowley has truly set a new standard for the sociological analysis of revolutionary movements.

The Unquiet Woods: Ecological Change and Peasant Resistance in the Himalaya. By Ramachandra Guha. Berkeley and Los Angeles: University of California Press, 1990. Pp. xx + 214. \$29.95.

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The Chipko (Hugging the Trees) movement to defend forest land in the Indian Himalaya is one of the most influential and successful grass-roots movements in the "developing" world. Many of the peasants' and native peoples' movements organizing against large dam projects and commercial lumbering in countries as diverse as Malaysia, Mexico, Brazil, and Canada have been inspired in part by Chipko's successes. In *The Unquiet Woods*, Ramachandra Guha explores Chipko's cultural and historical roots. He argues that Chipko is only the most recent manifestation of the ongoing struggle against commercial forestry in the Indian Himalayan mountains. Contrary to its popular image as a grass-roots environmental movement, Guha contends that Chipko is first and foremost a peasant movement "to defend traditional rights in the forest" (p. xii) and only secondarily (if at all) an environmental movement. In telling us the story of their struggle, Guha examines the effect that different processes of state formation—one colonial and the other traditional—have had on structuring resistance in the two adjacent regions of Uttarakhand. In this work, he argues convincingly that administrative and state forms structure the patterns of protest and resistance.

Guha begins by outlining two distinct paradigms commonly employed to examine lower-class protest: structural organizational (SO) and political cultural (PC). Following in the footsteps of James C. Scott's *Moral Economy of the Peasant* (Yale 1976), Guha's approach is more interpretive, examining differences in language and the mechanisms of protest and relating these differences to a unique cultural context. His version of the PC paradigm, while emphasizing the languages of domination and subordination, does not ignore economic factors in explaining the rise and character of social conflict and the variation that exists between the two regions. Guha believes strongly that social and production relations are defined by ecological characteristics—flora, fauna, topography, and climate—and that, as the forest ecosystem is altered, these relations will inevitably change. Furthermore, these changes will no doubt lead to a gradual reexamination of the dominant ideology, bringing it under in-

creased scrutiny. Guha's "ecological landscape of resistance" is a worthwhile effort to incorporate a sociology of peasant protest with an ecologically oriented study of history.

The area of study, Uttarakhand, is located in the far north of present-day India and borders Tibet and Nepal. Similar to other hill societies in the Andes and Alps, sharp class divisions like those found in the plain's region of India did not evolve because of unique ecological characteristics. Large-scale agriculture was nearly impossible due to the lack of appropriate soils and the paucity of irrigation. Until only very recently, the region remained relatively isolated from much of the Indo-Gangetic Plain by an area of swamps that made access difficult. This isolation, together with various ecological obstacles to the generation of surplus, hampered the emergence of social classes in the region. The same ecological factors were also largely responsible for the unique integration of agriculture with forests and pasture.

Between 1815 and 1949, the British divided the region into two distinct sociopolitical systems: Kumaun and Tehri Garhwal. The eastern region of Tehri Garhwal, of somewhat lesser strategic importance, was ruled by a traditional monarch. The western region of Kumaun, with a valuable commercial trade route to Tibet, was administered by the British East India Company as a colonial territory until 1949. Beginning in the mid-1800s, Kumaun and Tehri Garhwal underwent nearly identical processes of ecological change. The introduction of "scientific forestry," based on the German model of forest exploitation, usurped control of the forests from peasants. New policies were implemented to transform heterogeneous forests to uniform stands of commercially valuable conifers. The new laws introduced in both regions recognized only individual ownership rights, thereby fragmenting traditional communities and threatening the indigenous system of forest management that had maintained these forests for centuries. This substitution of traditional communal practices with a more individualistic system led to the alienation of peasants from the forests and, as Guha demonstrates, only hastened their destruction.

In spite of numerous similarities between the ecological and economic changes that took place in the two regions, peasants resisted these changes in quite different ways. Initially, *dhandaks*, traditionally sanctioned forms of passive protest against a leader who has failed to protect the rights of his people, were common in both Tehri Garhwal and Kumaun. However, while peasants in Tehri Garhwal continued to rebel in a manner that was culturally sanctioned, those in Kumaun gradually adopted a more confrontational approach. Kumaun's colonial administrators increasingly intervened in the lives of their subjects, introducing a rationalized, uniform tax structure and eventually excluding peasants from all forest lands. As these policies were implemented, peasant protest against the regime's actions evolved from the more passive *dhandak* strategy into more confrontational acts of resistance. Finally, the peasants in Kumaun, much to the surprise of their colonial masters, began at-

tacking colonial officials and public buildings. As the resistance movement grew, peasants began burning the forest land that had been expropriated from them and destroying the colonial government's resin depots.

In Tehri Garhwal, where the king was still viewed as quasi divine, peasants protested more passively by choosing to ignore his unpopular laws and blaming the king's "corrupt" officials for any transgressions that occurred. But even his "traditional" authority was increasingly called into question as he implemented policies that further limited peasants' access to the forest and its resources. While many continued to believe that he was misinformed or unaware of his "corrupt" officials' misdeeds, his regime, much like the one in colonial Kumaun, ceased to be legitimate in the eyes of the peasantry. While the character of the rebellion differed enormously in the two regions, peasants' grievances were quite similar.

While Guha focuses primarily on the colonial period from 1815 to 1949, in the last sections of the book he links these earlier struggles against the enclosure of forest land to the rise of the Chipko movement in modern India. He also examines the important role of women, the questionable role of the Congress party, and the effect that several environmental disasters had on the movement's evolution. Finally, he concludes by placing his study of Himalayan peasantry into the larger body of work on peasant protest. At times, the historical data seem a bit too abbreviated, but overall Guha does an excellent job of embedding this work in both the sociological and anthropological literatures on peasant protest, social movements, and the state. This book is a welcome addition to the growing body of research on peasant resistance in "developing" countries.

Military Organizations, Complex Machines: Modernization in the U.S. Armed Services. By Chris C. Demchak. Ithaca, N. Y.: Cornell University Press, 1991. Pp. xi + 202. \$31.50.

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A distinctive feature of American culture is its "can do" attitude, which leads to faith in the power of high technology to solve problems. The United States has consequently sought to make the most of its technological advantage over military competitors, as was illustrated during the Cold War and, more recently, during the Gulf War.

Chris Demchak examines the impact of a complex military technology, specifically that of the M1 Abrams tank, on the U.S. Army. In so doing, she provides an instructive social analysis of the costs and benefits of complex technologies, showing that their impact on organizational effectiveness is double-edged. This is especially so when new technology is introduced into an already complex organization; for example, into the

modern armed services, who start by being sensitively dependent on their logistical tails on the careful maintenance and resupplying of their equipment. Demchak focuses on a key paradox: reliance on complex technology to increase organizational capability and effectiveness can actually lead to the creation of an organization that is in fact more rigid and less effective.

The Abrams tank was developed in response to the Soviet threat in the 1970s. The argument was that this threat could be countered by exploiting the technological edge enjoyed by American industrial capitalism. At the same time, by employing more capital intensive systems, the all-volunteer force could respond effectively to a military threat during a period when the quality of its own recruits was causing some concern and pressure on the defense budget was increasing. The tank was designed to increase force capability and be easier to operate and maintain, while requiring cheaper inventories and fewer outlays in personnel. In short, it would allow the army to do more that cost less.

A key component of the new technology was the centralization of technical knowledge through the use of a "smart machine—dumb maintainer" philosophy (p. 80). The tactical maintainer's lack of knowledge would be offset by the sophistication of the test equipment, and only the most serious technical failures would have to be referred to the higher command level.

The heart of the book is chapter 6, in which Demchak analyzes the consequences of the introduction of the new technology. In an excellent discussion of the unintended consequences of social action she shows how, as in so many other complex technologies, the planners grossly underestimated the maintenance man-hours required to ensure even barely minimum serviceability and how faulty automatic test equipment necessitated additional instruments to diagnose faults in the test equipment itself. Both of these undermined the cost and effectiveness promised by the maintenance philosophy outlined earlier. She documents that large numbers of good parts were wrongly removed from the tanks because maintenance skills were lacking at key points where they were needed for effective organization; the consequence of the performance was hoarding of parts, which led to expansion in the size of inventories. New divisions of labor—that is, complexity—emerged to resolve administrative problems, as did complex webs of dependence between the tactical maintainers and such other points in the supply and logistics systems as the contractors responsible for building the tank.

Demchak also argues that the inflexibility associated with the new technology means that the armed forces will "have proportionately greater problems in trying to operate in wartime" (p. 11) and argues, in relation to the Air Land Battle doctrine (devised in the 1980s for the defense of NATO in Europe) that the very substantial demands on speed and flexibility of movement may be beyond the organization's capability. The Gulf War, however, may not have offered the best test of the perfor-

mance of this complex organization under battle conditions, partly because of the technological gap between the adversaries.

This is a scholarly, well-researched case study that should attract not only military sociologists but also readers interested in the sociology of technology and complex organizations. Demchak admits that her thesis is mainly concerned with examining the effect of technology on organizational complexity rather than on organizational performance. However, given the strategic confusion of the post-Cold War period and the substantial cuts in defense budgets in the United States and other advanced societies, the question becomes, What are the threats that the Air Land Battle doctrine will now have to address? The problems of technological and organizational complexity will surely be compounded by a new complexity in strategic missions.

Democracy at Work: Changing World Markets and the Future of Labor Unions. By Lowell Turner. Ithaca, N.Y.: Cornell University Press, 1991. Pp. ix + 265. \$24.95.

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Lately an abundance of material dealing with changing world markets and transformations of production processes have predicted severe union decline. In a time of intensified international competition, firms have sought to reorganize work to raise productivity. Organized labor's ability to resist or demand a voice in the decision making and implementation of these changes has been curtailed by the declining economic situation and growing unemployment. The declining membership figures of U.S. unions, as well as the fall of the Swedish model, have been cited as evidence for the contention that the time of unions is over. Yet systematic examinations of industrial relations in these changing times have been conspicuously lacking. Lowell Turner's book fills this void.

Through a thorough and detailed investigation of the auto manufacturing industry, which is extremely sensitive to changing world markets and the need to rationalize work processes, Turner shows that the outcomes and the position of unions differ both across firms and across nations.

First, Turner examines several U.S. auto plants where adversarial industrial relations became even more hostile as management forced new work organization, such as work teams, upon a reluctant or resistant union and rank and file. In many cases, such as at the Nummi plant, a Toyota and General Motors joint venture located in Fremont, California, that uses Japanese-style work organization, the cooperation of the workers was achieved only after they had been "tamed" by a two-year closing of the plant. However, at other plants, like the GM plants in Lansing, Michigan, and in Lordstown, Ohio, homegrown solutions to reorganize

work and increase productivity were welcomed by the workers. In these cases the union and the work force were brought into discussions on how to alter shop-floor production.

The differences are accounted for, according to Turner, by management attitudes. In the Nummi plant, managers demanded acquiescence from the workers; otherwise, they would not be rehired. Although workers were assumed to cooperate and work in teams, management did not alter its heavy-handed authoritarian management style.

Turner then turns to the German auto industry, and shows that, although the outcomes in Germany were of a narrower range than in the United States and worker's influence on decision making is institutionalized through legally mandated work councils, there were interfirm differences there as well. At the Volkswagen plant in Wolfsburg, the work councils adopted IG Metall group work concepts and were actively engaged in negotiations with management concerning future changes. At Opel in Bochum on the other hand, the transition to a Nummi model was conflictual, as many contending views were presented and as managers attempted to retain American-style authoritarian management. Nevertheless, the implementation of new shop-floor arrangements in Germany have generally been more consensual than those in the United States. Turner argues that these differences can be attributed to relative union strength, neocorporatist bargaining arrangements, and legal incorporation of workers' interest representation.

In the United States where the labor movement is weak and fragmented, has rarely enjoyed favorable legislation, and has evolved no tradition of neocorporatism, changing world markets have put labor in an even weaker position. Employers can use the threat of unemployment to squelch dissent over new work processes as well as to undermine union activity. In Germany on the other hand, where unions are strong and cohesive, where workers' interests are represented through work councils, and where a debate on the humanization of work preceded the dire need of industry to transform work, discussion of structural changes of work have included workers themselves—and often real attempts to create a better working environment have been made. Because of these factors, German unions have increased their membership during the 1980s.

To lend further credibility to his argument, Turner compares the apparel and telecommunications industries in the United States and Germany, and he takes a brief look at similar changes in the auto industries in Britain, Italy, Sweden, and Japan. Similar institutional arrangements have brought about similar outcomes in these cases as well.

I find this book very comprehensive, well documented, and worthwhile. Lowell Turner manages to discuss industrial relations in a revealing and meaningful way, without slipping into the familiar ideologically laden language of either neoclassical economics or Marxism.

The only thing I find lacking from Turner's account is a historical

review of earlier rationalization debates, like the ones that focused on Taylorism and scientific management. A historical comparison would further reinforce Turner's point. In the Swedish case for instance, scientific management and early rationalization attempts were the cause of intense battles between labor and management. There was little legal protection of organized labor and no institutionalized bargaining model. Later, under the auspices of the Swedish model, when the labor movement was strong and cohesive and protected by law and retraining programs for displaced workers were in place, rationalization and automation in Swedish industries were implemented rather smoothly. *Democracy at Work* is an important contribution to the study of work and unions in industrialized nations. I can recommend this book highly to scholars as well as to persons more practically involved in the complexities of industrial relations.

Japan's California Factories: Labor Relations and Economic Globalization. By Ruth Milkman. Los Angeles: UCLA Institute of Industrial Relations, 1991. Pp. xviii + 130. \$11.00.

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Ruth Milkman's study of Japanese-owned manufacturing plants in California is a timely inquiry on an important problem that has drawn wide media attention though sparse research to date. The book, reporting on a 1989 survey of 50 of the 72 Japanese-owned plants located in California and having more than 100 employees, seeks to dispel a number of myths regarding Japanese direct investment in the United States—that it will add jobs, improve labor relations, and increase the efficiency and competitiveness of the U.S. economy. On all of these issues, Milkman sides strongly with the pessimists—too strongly, in my view. Her broad, negative generalizations regarding the impact of Japanese direct investment on American workers and the U.S. economy are generally unsubstantiated by her findings. They mar what is otherwise a very useful and interesting investigation of the Japanese transplant phenomenon in California.

California contains 20% of the Japanese transplant jobs in the United States, 76% of which are concentrated in four urban counties: Los Angeles, Orange, Santa Clara, and Alameda. In Milkman's view, the choice of California as a site and the pattern of Japanese plant location within the state are largely driven by the search for docile, low-wage, nonunion, immigrant labor. There is a sense in which she is right about this, but her portrait of California as a sort of Third World labor market is certainly at odds with the much-publicized claims of the local business community that high wages and prices, strong unions, and overregulation have made the state uncompetitive.

Apart from autos and metals, Japanese-owned plants in California pay lower wages than their industry average in the state, though the comparisons that indicate this do not take into account the likelihood that these plants are newer, younger, and smaller as well. Milkman acknowledges claims that Japanese firms discriminate in hiring on the basis of gender and race, but she takes no strong position on the issue. Blacks are underrepresented in these plants but so are native-born whites; 56% of the Japanese-owned plants in the state are in the electronics industry, and they mirror the U.S.-owned electronics plants in their low wages, taste for Asian and female labor ("nimble fingers"), and aversion to unions. For reasons that Milkman acknowledges (the strongly adversarial posture of American industrial unions vs. the cooperative stance of Japanese enterprise unions), Japanese firms in the United States do fear unionization, and her interviews effectively highlight their antiunion animus.

Milkman then addresses the interesting question of whether Japanese-owned firms in California practice Japanese-style management and employment relations. Her survey finds relatively few that use quality circles, self-managed teams, and job rotation, and very few have implemented Japanese manufacturing techniques such as just-in-time and *kaizen*. Most, moreover, are not very selective in their hiring, nor have they invested heavily in employee training. The managers she interviewed confided that language, cultural barriers, and poor educational skills precluded implementing Japanese-style methods such as job rotation and quality circles, which call for broad and/or technical training. A number of the plants also had quite detailed job classification systems, a pattern at odds with the broad and diffuse job categories stressed in research on Japanese organization and typical of the Japanese-owned auto industry in the United States. In accord with the usual images of Japanese management, however, she found an aversion to layoffs (particularly among the Japanese managers—the American local managers favored them), suggestion systems, extensive employee ceremonies and social activities, a willingness to deploy people in clean-up and maintenance activities during production downturns, use of temporary workers to buffer regular employees, and a heavy stress on attitude and commitment in merit pay evaluations.

So Japanese companies in the United States practice Japanese methods in some respects but not in others. This is the conclusion of most thoughtful observers and thus comes as no surprise. Milkman professes surprise, however, because her image of such enterprises going into this research was Nummi (the Toyota-General Motors joint venture in Fremont, California) much heralded as a paragon of efficient "lean" production and harmonious union-management relations. Most Japanese-run factories in the United States are not Nummi clones. Like domestically owned plants, they are a diverse lot, many being small operations to which the Japanese company has made a far more limited commitment than in the case of Nummi and the other auto transplants.

That Milkman's disillusionment with Japanese transplants stems in

part from unrealistic expectations is also evident in her conclusion regarding their limited implementation of Japanese management techniques. She acknowledges that "there is some employee involvement and participation in these firms, but the typical goal is to promote communication and trust between workers and management (as part of a union avoidance strategy) rather than to engage workers intellectually in the management of production" (p. 78). What is the implication here? That Japanese firms in Japan pursue the high-minded agenda of making workers full partners in production decisions but that in the United States Japanese employers practice cynical co-optation? The standard by which Milkman evaluates the employment practices of California transplants seems idealized and naive. Though one the business press has propagated in recent years, it is in no way the thrust of a now-rich research literature that has long acknowledged that worker participation in Japan takes place within a well-defined framework of management control (see, e.g., J. Lincoln and A. Kalleberg, *Culture, Control, and Commitment* [Cambridge University Press, 1990]).

Milkman's empirical descriptions of Japanese-controlled manufacturing in California for the most part seem accurate and informative. As I have suggested, her bottom-line conclusions are another matter. Her expectations shaped by the Nummi ideal, she asserts: "If Japanese direct investors are permitted instead to continue on their present course, the result will be a continuing deterioration in the competitiveness of the U.S. economy and in the living standards of its people" (p. 124). At the risk of seeming an apologist for Japanese investors, I find this assertion far too strong and see little basis for it in her empirical work. It panders to the Japan-bashing mood of the country in general and that of American organized labor in particular. She has no evidence that Japanese-owned companies are any more antiunion than comparable domestic firms, and, indeed, her own data show them considerably less likely to lay employees off. She repeatedly asserts that Japanese direct investment has added no new jobs to the American economy, but she has no evidence that this is the case. Since the transplant products made by American labor substitute for imports produced in Japan and other foreign sites, it is hard to see how their presence could have failed to add jobs to the U.S. economy.

In sum, Ruth Milkman deserves much credit for this investigation. Her data genuinely augment our understanding of an issue characterized mostly by acrimonious and muddled discussion on the part of politicians and the press. Sociology should play this role more often. Yet Milkman's animosity toward Japanese firms in the United States is disproportionate to her evidence. Intended or not, the book will thus be viewed as riding the current wave of anti-Japan sentiment and lose credibility as a result. Given its many merits as timely and useful research, this is unfortunate.

Threads of Solidarity: Women in South African Industry, 1900–1980. By Iris Berger. Bloomington: Indiana University Press, 1992. Pp. xiii + 368.

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In *Threads of Solidarity*, Iris Berger's historical analysis moves back and forth between changes in the organization of South African industries and the nature of the labor market on the one hand, and women's labor activism on the other. In doing so she reveals the complex interdependence of race and gender that fashioned and refashioned working-class responses to the development of South African capitalism from the early years of the 20th century to the 1980s. In addition the author is careful to point out the regional patterns of industrial development and urbanization that affected the labor-force participation of women from different ethnic groups and the nature of women's involvement in trade union organizations. The book not only makes an important contribution to our knowledge of South African labor history and industrial transformation; it also reveals the changing ways that gender and race interacted to shape class formation in the specific context of the South African politics of racialism.

Berger rightly refuses to create a simple narrative to describe the historical development of South African labor relations, choosing instead to explore the complexity that existed. She shows that over the course of the 20th century, the industrial work force in South Africa changed from being white and male to being composed primarily of black women and men. But she does not attribute these changes simply to the forces of capital accumulation that lead employers to deskill work and seek out the cheapest laborers, although that is certainly a part of her story. Instead she quite successfully demonstrates that both race and gender shaped the manner in which South African industrialization occurred. In particular, she shows the role of the state, bent on a gendered white protectionism, in guiding industrial development and the nature of the labor market. In other words, gender ideology and racialism together shaped the conditions under which different groups of women and men entered the industrial labor force.

If her story of industrial development and the changing nature of the labor market is a complex one, so also are her accounts of why different groups of women worked in particular jobs, and under what conditions they united to defend their jobs and to better their working conditions. While race often, as might be expected, served to divide workers one from the other, there were points in the history of the South African labor movement when solidarities crossed racial and ethnic boundaries. For example, white women and black men in the garment industry of the 1930s cooperated in their struggles with employers, although policies of racial inclusiveness were later to be abandoned with the development

of nationalism among the Afrikaans population. And, in the midst of the racial politics of the 1970s, and in spite of continuing harassment, "colored" and black women in the canning industry forged a consciously cross-race trade union organization.

Although she emphasizes the features specific to South African society, Berger frames her study in the context of theoretical issues in the historiography of working-class women in other countries. In particular she asks what brings women to be active in class-based organizations, especially in trade unions. She argues that the evidence from the South African case supports the view that women become active in organizations that address their concerns not only with regard to work, but also those that bear on their lives outside the workplace.

A decided strength of the book lies in Berger's clear exposition of why focusing on women changes how we understand labor history. She argues, for example, that in South Africa, women and men had different paths of proletarianization which in turn suggests that theories about the development of capitalism based solely on men's experiences are inadequate.

Along with the many virtues of this book are some problems with Berger's analytical framework. On the one hand Berger sees social and economic structures as constraining—setting limits and creating interests. On the other, she sees women workers making choices and actively shaping their own lives, sometimes reacting against those constraints. Thus she maintains a strict dichotomy between structure and agency. Berger is quite explicit in not wanting to view the women who are the subjects of the book as "victims," insisting that by making choices, their behavior was not the consequence of blind structural forces over which they had no control. Yet, we are told relatively little about how and why people made the choices that they did, and we do not see directly how the choices that they made contributed to emerging structural arrangements. Also, there are points in the book where Berger seems to assume that peoples' interests stem automatically from their racial, gender, and class identities, and so she does not fully investigate how those interests and identities were shaped and made politically relevant. These are, however, relatively minor concerns about what is generally an impressive piece of scholarship.

Job Queues, Gender Queues: Explaining Women's Inroads into Male Occupations. By Barbara F. Reskin and Patricia A. Roos. Philadelphia: Temple University Press, 1990. Pp. xii + 388. \$39.95.

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Despite radical transformations in the industrial and occupational structure in the post-World War II era, changes in the composition of the labor force during the same period, the feminist movement, and antidis-

crimination regulations of the 1960s and 1970s, the notable gains women made in a few occupations previously dominated by men have not reversed the persistence of occupational sex segregation. Reskin and Roos seek to explain this enduring pattern and to account for the handful of customarily male or mixed-sex occupations that underwent a disproportionate increase in women in the 1970s. Their study relies upon census data and 14 case studies from among the 33 occupations in all occupational strata that exhibited disproportionate change. Their investigation examines: (1) whether technological, organizational, and work conditions precipitated change in occupational sex segregation; (2) whether shifts in human capital requirements and labor supply generated change in occupational sex composition; and (3) whether institutions such as unions, regulatory agencies, and the courts facilitated women's entry into male-dominated fields. In addition, they explore the consequences of occupational sex desegregation, particularly with respect to pay equity for women who moved into male-dominated occupations.

They have two major findings. First, sex integration of jobs typically did not accompany occupational-level desegregation. Women and men were segregated into different jobs in each of the occupations they studied, even as women's representation increased in those occupations. Vacancies for women occurred when structural transformations precipitated change in earnings, benefits, prestige, job security, autonomy, or chances for advancement in a specific occupation, thereby lowering men's preferences for those occupations over others. Generally, those vacancies were attractive to women because they offered improvements over other options available. Vacancies also occurred when growth of an occupation created a demand for employees that exceeded the supply. Regardless of the reason for vacancies, women entrants worked in different subspecialties than men, served different clients within firms, worked in different industrial sectors, and were employed at different ranks. In short, women were "ghettoized" within feminizing occupations. Men who stayed retained the more desirable jobs, while women were relegated to lower-status specialties, less desirable work settings, lower-paying industries, and part-time rather than full-time work.

The consequences of ghettoization account for the second major finding of this project: women's earnings in feminizing occupations improved relative to men's only because men's earnings eroded in those occupations. That erosion was indicative of a declining occupation that offers limited career prospects, regardless of gender.

The authors rely upon queuing theory (rather than human capital theory, gender stereotyping, or other accounts emphasizing individual action) to provide a structural explanation for the remarkably enduring pattern and bleak consequences of occupational sex segregation. According to queuing theory, employers' construct a queue that ranks potential employees from most to least desirable. However, individual differences are taken into account only within groups defined by gender and race. Simultaneously, workers maintain job queues, ranking jobs in

terms of attractiveness. When employers' ordering, the relative supply of groups in that ordering, and rankers' (both employers' and workers') preferences overlap, occupational sex segregation is stable. When those preferences diverge due to structural, institutional, or human capital requirements, occupational sex integration occurs. Males' continuously privileged ranking over females in the labor queue facilitates their movement out of occupations losing status and their successful search for alternatives, thus perpetuating the pattern of segregation and relative pay inequity. Queuing theory improves upon other structural explanations of occupational sex segregation (such as models of statistical discrimination) for two reasons. First, it allows for change over time in gender composition of occupations and jobs. Second, it explains the re-segregation that almost invariably follows gender integration. The authors do not, however, fully consider the limitations of studying only those occupations that rapidly desegregated within a 10-year period. Different mechanisms and dynamics may operate within occupations that experience more gradual shifts in gender composition. Presumably more measured integration, if that does indeed ever happen, would be explained by a mix of both structural and individual level mechanisms. I hope that Reskin and Roos's plans to statistically model the determinants of occupational sex composition across all occupations will unlock the conundrum so carefully investigated in their case studies.

Reskin and Roos have provided an important contribution to explaining segregation in the work force. Unlike most other structuralist accounts, theirs attends to preferences and choices of both employers and employees and explains change over time in patterns of gender segregation, integration, and re-segregation.

Gender Inequality: A Comparative Study of Discrimination and Participation. By Mino Vianello and Renata Siemienka. Newbury Park, Calif.: Sage Publications, 1990. Pp. xxii + 360. \$47.50.

Julie Elworth and Szonja Szelényi
Stanford University

This is an ambitious study of gender inequality in four countries—two from the democratic West (Canada, Italy) and two representing socialist regimes (Poland, Romania). The authors collected their own survey data with the expectation that this would ensure comparability across nations and extend their inquiry into “different areas of social life: values, family, work, and public affairs” (p. 1).

Vianello and Siemienka have three objectives in mind: uncover the forces that hinder women's access to political power, examine the extent of women's participation in public life, and compare their status across socialist and nonsocialist societies (p. xi). The first chapter sets the stage for the analyses. In a compact set of eight pages, the authors describe

each country's level of economic development, outline the nature of their political system, and summarize their state policies on matters (i.e., family structure and child custody arrangements) that are most "relevant to women" (p. 15). The rest of the book is devoted to an empirical analysis of gender inequality in the four countries. It offers a lengthy description of gender differences in attitudes (see chap. 2), provides some data on women's involvement in family life (chap. 3) and work (chap. 4), and discusses women's participation in politics (chap. 5).

In several respects, this book is a pioneering contribution to research on women. Conceived in the early 1970s, it was among the first to *initiate* a cross-national comparison of women's status. Moreover, it is also among the first to examine the social position of East European women.

In spite of these virtues, however, *Gender Inequality* is a frustrating book. The 20 years that it has taken to complete the manuscript have left an unmistakable mark on its quality: the theoretical discussions that are advanced by the authors are frequently out-of-date, and their empirical research also shows definite signs of age. In the 1970s, for example, it might have been innovative to conclude a book by saying that it "shattered another stereotype: the 'women = private,' 'man = public' dichotomy, according to which women are secluded at home, taking care of their families, while men are active in business, politics, culture, etc." (p. 242). But, after 20 years of research on women this finding will not surprise anyone, nor will it serve as the central selling point of a new book.

As an analysis of cross-national differences in gender inequality, the theoretical contributions of this study are limited. Granted, the book was designed as an "experimental study," (p. xi) and, as such, it is motivated by a series of working hypotheses rather than a robust theory. Still, readers will be disappointed to find that even these (loosely connected) hypotheses are lost in the structure of the book. The findings in each chapter are arranged in terms of specific items from the authors' questionnaire—rarely do they address substantive issues and never do they converge into a thoughtful summary. In this context, it is not surprising that the authors' closing remarks are not tightly linked to their empirical research. Vianello and Siemienka conclude their book by saying that women must "share everything on a equal footing" with their partners (p. 246), they must resocialize men "to perform domestic chores" (p. 247), they need to develop a "new style and a new perspective" if they are to survive in a man-made world (p. 247), and they should also organize to "assert their rights and experiences as women" (p. 248). How the authors reached these conclusions from the material that is presented in the book is unclear. One thing is certain: these words could have been written without the 240 pages that preceded them.

Space limitations prevent us from listing all the methodological weaknesses in this book, so we will restrict our comments only to those that invite the greatest caution and skepticism. The most serious one of these is the authors' sample design. Vianello and Siemienka used purposive

samples to explore cross-national differences in gender inequality. This sample design has the advantage of being relatively inexpensive. However, it is impossible with such a sample to make generalizations to a well-defined universe, and any conclusions about cross-national differences are, therefore, rendered suspect. But the problems do not stop here, as the analyses are also carelessly executed. The authors use statistical methods without justifying them, they randomly omit key control variables (e.g., age, education, class) from their models, and misinterpret the results in several of the tables (e.g., see p. 63).

Readers familiar with research on gender inequality will not find much current here, and those new to the area would be well advised to turn to more rigorous and theoretically interesting approaches in this field (e.g., see Patricia Roos, *Gender and Work* [New York: State University of New York Press, 1985]).

Women, Men and Time: Gender Differences in Paid Work, Housework and Leisure. By Beth Anne Shelton. Westport, Conn.: Greenwood Press, 1992. Pp. xv + 182. \$39.95.

Julie Brines
University of Chicago

The frenetic pace of daily life in today's America renders the adage "time is of the essence" slightly off-key. For increasingly time *is* the essence, viewed not as something one merely expends on activity but as a resource to be conserved. The pressures brought to bear on this resource are perhaps felt most acutely among working parents of young children. Attesting to these strains are several recent studies that examine the work arrangements of dual-earner couples and document in particular the multiple and often conflicting demands on the time of employed mothers.

Although *Women, Men, and Time* is designed to assess the wider association between gender and time use, the employed mother's "double day" initially motivates Beth Anne Shelton's agenda. To gain leverage on this phenomenon, Shelton addresses several critical questions. To what extent has change in women's and men's paid labor and housework time (or the lack thereof) conspired to produce the double day? Do gaps in time spent on types of work emerge between employed married men and women without children? Between young working singles? Are there leisure-time differences, and do they mirror work-time disparities? By raising these questions and a host of others, Shelton attempts to advance our understanding of how gender articulates with family roles in shaping the allocation of an important personal resource.

The scope of this book extends beyond that of most time-use studies. Using data from the 1975–81 Time Use Longitudinal Panel Study and the 1987 National Survey of Families and Households, Shelton analyzes

trends in time spent on paid work, housework, and leisure, as well as how time use varies by individual and household characteristics. These different dimensions of analysis are bound by a "synthetic" (p. 14) approach, one informed by the New Home Economics, sociological models of power, and perspectives emphasizing sex-role ideology, but proceeding primarily through an appeal to "common sense" with functionalist undertones.

Scholars familiar with research in this area will encounter few evidentiary surprises in this volume. Although men's and women's time expenditures on paid labor and on housework became more similar between 1975 and 1987, significant gaps remain. Moreover, the moderate trend toward convergence in both domains is due primarily to change in women's behavior: while women have substantially increased their employment hours and reduced those devoted to housework, men have made only modest adjustments in the opposite direction. Persisting gender differences in time use stem primarily from a markedly different association between family structure (namely, marriage and parenting, particularly of the young) and the labor time of women compared to men.

What might provoke surprise is the presentation of *prima facie* evidence on behalf of comparative equality in leisure time. To wit, total leisure time changed little between 1975 and 1981, does not differ appreciably by gender in the aggregate, and is significantly (and similarly) associated only with paid work and housework time. But Shelton provides other evidence that complicates the story. Calculations based on her regression estimates indicate that among those who fit a dual-earner profile, wives spend approximately seven hours fewer per week on total leisure than do husbands. Although this deficit mirrors the extent to which dual-earner wives' total work time exceeds that of husbands, the correspondence does not stem from a one-to-one trade-off. Gender differences in the effect of age (albeit statistically insignificant) account for a sizable portion of the dual-earner leisure gap, suggesting the operation of modest family life-cycle effects. In addition, Shelton reports that men spend more time than women on "passive leisure" (primarily, watching TV), and that a large share of nonemployed women's leisure is devoted to the "hobby" of preserving fruits and vegetables. These, among other findings, qualify the image of equality in this sphere.

For those who like numbers, Shelton provides a comprehensive digest of men's and women's time use. But for those who hold a particular appreciation for how numbers are generated, this book will prove frustrating. Substantial case loss from the original sample afflicts several baseline regression analyses, raising unanswered questions about the treatment of missing data. A complete description of the measurement and range of independent variables appears nowhere in the book. And because most of the regression equations interact gender with every other independent variable and therefore constitute nonadditive models, Shelton's repeated reference to the main effects of gender as "direct" can

lead to erroneous interpretations. These shortcomings—along with the absence of significance tests and controls on most of the trend analysis—make it difficult to carefully evaluate a good deal of the evidence.

This is not to argue that Sheldon's analyses are without merit: most of the results ring acceptable on their face. But regardless of whether one approaches this book out of general curiosity or a specialized research interest, the payoff is likely to be limited. Newcomers looking for a culminating storyline might find their search lost amid a thicket of tables and description, followed by more tables and description. Specialists will feel the need to pick and choose among the results for rock-solid new findings. The ample descriptive material could provide a useful stimulus for reflection and further research. But profitable reading will require the selective application of one's time.

The Overworked American: The Unexpected Decline of Leisure. By Juliet B. Schor. New York: Basic Books, 1992. Pp. xvii + 247. \$21.00.

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Duke University

It is little wonder that Juliet Schor's book has struck a responsive chord among mass media pundits. The topic of working hours is timely, given the public's concern over declining American productivity. Schor's other theme, Americans' passion for consumption and their willingness to work longer hours to satisfy it, helps to account for the "greediness" of the 1980s. The book's appeal is enhanced by its sprightly prose and the author's light touch with economic theory. The resemblance in style to another Harvard social scientist, Daniel Bell, is striking. Unfortunately, the resemblance does not stop there, for many of Schor's ideas are quite familiar to academics, such as Bell, who have pondered why leisure is so marginalized in American society. Sociologists looking for something new in this book will probably be disappointed.

The Overworked American is rather like a package tour, led by a very well informed guide who is adept at bringing new life and meaning to ideas that are vaguely familiar but that we now understand clearly because we see them in their proper context. The tour includes a visit to the domestic household, where women, despite entering the paid labor force in increasing numbers, work "second shifts" with only a little additional help from their husbands. It takes in "the revolution of rising expectations" in which the definition of an acceptable standard of living constantly rises, forcing us to work longer hours to sustain it, and includes, as a side trip, a visit with the "harried leisure class," who, in obedience to a "fun morality," have so little time to spare because they are busy making use of their possessions. The tour even includes an excursion into the past to examine the relation between "time, work discipline and industrial capitalism."

The tour is edifying and Schor is careful to credit earlier explorers, but does the book break new ground for the specialist in this area? While many of the statistics are fresh, the myth of the declining workweek was exploded long ago (e.g., Reginald Carter, "The Myth of Increasing Nonwork Time vs. Work Activities," *Social Problems* [1970]). Does the book present new theory? It certainly helps to have these issues, so often discussed in isolation, considered holistically. Hours spent on housework are related not only to ideas about proper gender roles but also to women's opportunities for paid employment and the family's need for more income because of falling real wages. Similarly, hours spent in paid employment are partly determined by the urge to purchase more goods, a desire fueled by the advertising industry and made easier by credit cards, but the "incentive" of overtime or the competitive pressure to put in more hours at the office also play a role because they increase the cost of free time.

But all this, too, has a very familiar ring, because it is predicated on the assumption that "competition in labor markets is typically skewed in favor of employers." This is a refreshingly forthright statement for an economist to make (and welcome news to the AFL-CIO think tank that partly funded the study). Not only does it account for historical variations in work time within the United States (they are largely due to changes in the balance of power between capital and labor), it also explains why European workers enjoy more free time (not only are unions more powerful there but their unions take care to place free time on the collective bargaining agenda). But the fact that the length of the working day is determined in large part by the struggle between capital and labor is hardly news to sociologists, who are less enamored of the "neoclassical view" of the employment contract than economists.

From a sociological point of view, perhaps the most intriguing of Schor's theses is that concerning the "fringe benefit bias," because it uses the structure of employment to explain variations in working hours. The cost of extending benefit packages to new workers does help explain why some workers are overworked while others have no job at all. Schor makes much of this explanation, and it distinguishes her argument from that of Benjamin Hunnicutt, who has already documented how unions have failed to seek free time rather than money in *Work without End* (Philadelphia: Temple University Press, 1988). But she really needs data broken down by occupation or firm to assess its validity. Only about a third of the labor force is covered by the kind of commitment mechanisms she believes makes a difference to holistic distribution, and this proportion is declining as more and more employers change to "cafeteria-style" benefit packages that provide employers less incentive to impose more work (and possibly pay overtime) on existing workers rather than hiring new workers.

Making Fast Food: From the Frying Pan into the Fryer. By Ester Reiter. Montreal: McGill-Queen's University Press, 1991. Pp. x+211. \$29.95.

Dishing It Out: Power and Resistance among Waitresses in a New Jersey Restaurant. By Greta Foff Paules. Philadelphia: Temple University Press, 1991. Pp. x+225.

Robin Leidner
University of Pennsylvania

Despite the prodigious growth of the service sector, social scientists' conceptions of work still generally take manufacturing as their model. *Making Fast Food* and *Dishing It Out*, two lively accounts of low-level food service work, are therefore welcome additions to the literature. In *Making Fast Food*, Ester Reiter studies Burger King both to explore social and economic developments that have reshaped relations between families and the marketplace and to examine the workplace practices typical of the fast food industry. Greta Paules's *Dishing It Out* analyzes the relations among waitresses, managers, and customers in one New Jersey unit of a national chain of restaurants. Each book shows how service work requires researchers to broaden their range of analysis—Reiter's by examining the links between paid service work and unpaid consumption work and Paules's by incorporating customers into the model of workplace control. Both Reiter, a sociologist, and Paules, an anthropologist, use participant-observation to examine issues of autonomy and control in highly centralized corporations, but they reach starkly different conclusions about the capacities of the companies to implement routinization and of workers to resist exploitation.

Reiter's is the more ambitious book. Besides providing a close-up view of the workings of one Burger King outlet in Ontario, *Making Fast Food* uses the rise of the fast food industry in Canada to explore such broad trends as the influx of women into the paid labor force, the expansion of part-time and other contingent forms of work, the ramifications of commercialization for home life, and the replacement of small-scale, local enterprises by offshoots of multinational corporations. In tracing the economic and social developments that have led to the phenomenal growth of the fast food industry since the 1950s, Reiter details the increasing participation of family members in waged work and the increasing penetration of the market into family life. Richard Slye's witty illustrations—surrealist photomontages in which huge hamburgers dominate both landscapes and interior scenes—succinctly express Reiter's main argument. Fast food products symbolize the capacity of capitalism to remake culture and social relations in its own interests and, in the process, to redefine family and community life and debase nature and art.

Much of the book describes and analyzes Burger King's powerful influence on Canadian culture. Reiter documents the swift "fast food inva-

sion" of the country by huge U.S.-based firms using local capital put up by franchisees. Burger King becomes an exemplar of work regimes that combine extreme routinization and tight control of labor with "human relations"-style management techniques. It proves highly successful in deskilling the work force by imposing standardized specifications. Reiter does not assume that workers are passive victims, but she confesses that she "looked and looked, but never found [the] active participation in determining the labour process that Burawoy described. . . . It is not consent that is produced, but acquiescence" (p. 145).

Throughout *Making Fast Food*, Reiter raises broad questions about the kind of world oligopolistic capitalism creates. She evaluates liberal and Marxist ideals of work and contrasts the effects of commodifying domestic work to feminist ideals for the integration of home and community. She does not idealize the past, but looks toward a "feminist and a non-capitalist alternative" that will, "like the fast food industry hype, weave together family, community, and workplace" (p. 173).

Dishing It Out (which, confusingly, has the same title as Dorothy Cobble's 1991 historical account of unionization among waitresses) is an ethnography of one outlet of a family-style restaurant chain Paules calls Route, a step above fast food but several steps below fine dining. "Bon vivant urban professionals do not condescend to dine at Route, which is not greasy enough to be camp" (p. 2). The restaurant where Paules worked and observed is on a busy interstate highway in New Jersey, and she provides a vivid picture of the bustle and banter of restaurant life from the waitresses' point of view. But where Reiter emphasizes the inability of Burger King's workers to resist corporate control of the work process, Paules finds that Route's waitresses are feisty defenders of their own interests.

Like Burger King, Route has a central headquarters that issues meticulous regulations about how every aspect of the restaurant's business should be handled, including every detail of the waitresses' job. In Paules's portrayal, however, waitresses have the upper hand and store-level managers are virtually powerless to impose corporate directives. Although the waitresses have not pursued collective strategies to control the workplace, they do not lack consciousness of their own interests or willingness to act on them. They tell off customers who provoke them, call in sick whenever they feel like it, and suit themselves rather than customers, as when they avoid a messy and irritating task by telling patrons that the milk shake machine is broken. Besides describing various techniques for maximizing earnings, Reiter pays particular attention to the waitresses' psychological resistance to "the symbolism of servitude." She cites William F. Whyte's observation that "the waitress can't help feeling a sense of personal failure and public censure when she is 'stiffed,'" but immediately quotes one of the Route waitresses to the contrary: "[The customers are] rude, they're ignorant, they're obnoxious, they're inconsiderate. . . . Half these people don't deserve to come out and eat, let alone try and tip a waitress" (p. 23). While Arlie Hochschild

stressed the emotional costs of jobs that require workers to produce pleasant personal fronts while under pressure from management and customers (*The Managed Heart: Commercialization of Human Feeling* [Berkeley and Los Angeles: University of California Press, 1983]), Paules's waitresses do not see themselves as compromised by these demands. They regard customers entirely instrumentally, feel no obligation to behave sincerely toward them, and refuse to tolerate treatment they consider abusive or demeaning.

Insisting that waitresses are not powerless, Paules highlights the freedom of interpretation that allows workers to resist potentially demeaning aspects of their jobs and shows how they can exploit weak spots in the corporation's power to control events through standardized practices. But she frequently speaks generally of "the waitress," rather than analyzing the conditions under which workers are more or less able to defend their freedom of action. These waitresses worked in an area undergoing rapid development and consequently experiencing a severe labor shortage, which significantly enhanced their power in the workplace. Their brassy disregard of managerial directives, lack of concern about customers' complaints to superiors, and disdain for promotion to management all reflect the ease with which the waitresses could find new jobs and the difficulty managers had in maintaining an adequate staff.

Paules brings customers more fully into the dynamics of the service workplace than Reiter does, but her treatment of customers is surprisingly one-sided. She applauds the waitresses' efforts to maximize their earnings, autonomy, and convenience while eschewing servility, but does not represent patrons' interest in pleasant service. It is surprising that there is little hint here that waitresses ever enjoy talking with customers. I assume that a somewhat different picture would have emerged in a restaurant that had a more regular clientele than a roadside chain, but Paules does not treat the particular circumstances of this restaurant as a collection of variables.

Both of these books are entertaining and thought-provoking. Their contrasting analyses point to the importance of comparative studies that not only test whether theories and judgments applicable to male manufacturing workers are useful for understanding women service workers, but also investigate the range of variables affecting managerial strategies and workers' power in service industries. Moreover, their incorporation of consumers into the analysis of work, both as participants in paid labor processes and as citizens whose lives are affected by the practices of service organizations, suggests intriguing possibilities for bridging the sociology of work and cultural studies.

Down the Backstretch: Racing and the American Dream. By Carole Case. Philadelphia: Temple University Press, 1991. Pp. 194. \$22.95.

Jack Katz

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Ethnographies can be valuable because of the data they offer to the research community or because of the analysis they develop. The very best ethnographies do both, offering readers an original explanation closely hewn from an unprecedented, close-up description that conveys the voices and local styles of the members of a given social setting. The recent crop of sociological ethnographies suffers less from "going native" and losing a sociological sensibility than from commitments to interpretive sophistication and moral relevance that fail to lead authors decisively outside of the academic community. Thus they fail to bring back some of the stuff of social life, still alive and writhing in the immediacy of members' lived experience.

Carole Case's *Down the Backstretch* is a refreshing reminder that the ethnographer's basic commitment is exploration, an altruistic act of entering a previously inaccessible or superficially glossed social world and bringing back a description sufficiently grounded in members' voices and practical situations of action that subsequent analysts, focusing on currently unanticipated issues, will find valuable data in the research report. Her primary concern is to make accessible to the research community the social world of work behind the public display of horse racing. She succeeds by drawing on interview excerpts and personal observations that portray the owners, trainers, jockeys, grooms, assistant trainers, "hot walkers," exercise riders, and jockey agents in this highly stratified and fully embracing social world.

The "news" she offers is scattered throughout the book. Culture analysts may be interested in the 10-minute, prerace "paddock ritual," where all the behind-the-scenes workers are assembled and presented to the public in a ritual display that appears to be surprisingly meaningful to the participants. Lines of stratification are at first glance clear, but the interactional workings of power are complex. Grooms and other lower members of the social order are dominated as cheap, expedient labor, paid at the discretion of trainers and subject to disruptions of their personal and economic lives as the racing scene moves from track to track. At the other extreme of the hierarchy, owners are regarded by their immediate subordinates as easily manipulated because of their ignorance of horses. The overall image is of power in the middle ranks, with diletantes above and arbitrarily exploited dirty workers below, an institutional system of relations closer to agrarian aristocracy than to the business-school managed contemporary firm.

The distinctive nature of this social world as captured by Case emphasizes the consequences of a migratory work life on the personal lives of those at all ranks, and a surprisingly widespread and deeply felt dedica-

tion to horses and racing. The positive themes of members' commitments becomes the major theoretical problem addressed in the book. The workers who receive less than minimum wage for tending to the animals and their stables and the trainers who are reputed to have become millionaires often share an involvement in the "backstretch" as an insulated, total way of life.

For an explanation, Case turns to Merton's "strain" theory and notions about "the American Dream." It is an ironic testament to the strength of Case's ethnography that she collected and presented the data through a commitment sufficiently independent from her analysis that one can discard Merton's theory while still valuing the study. The author argues that the workers develop their affections for this work world in reaction to their experience of economic failure; but her evidence shows no change over career stage in affection, dedication, or commitment, no difference in commitment among those who do well economically and those who do poorly, and no consistent pattern of dreaming of a big payoff to offset a daily routine that is experienced as oppressive. The dream here is not for a future as a small capitalist with a little farm or business, but the gambler's dream of the big payoff, and the present is not a dull routine but an affectionate involvement. Having convinced us that she has found a distinctive social world, it is difficult for Case to be equally convincing by taking the most standard theoretical formula off the shelf. There appear to be, somewhere in the careers of the people so committed to the backstretch, interesting mechanisms that function in the way suggested by Howard Becker's concept of "involvement," a process by which people get progressively involved in a relationship so that they become indifferent to alternatives. Unfortunately, the marvelous flexibility of Merton's theory (it is as content to find happy as disgruntled workers), plus its narrative power (an author can adapt it with the relaxed assumption that everybody already believes it) once again proves irresistible. One wonders how much American sociology would improve if we insisted, before anyone could again use the American dream as an explanation, that the author check out at least one other country for comparative data.

Equal Parenthood and Social Policy: A Study of Parental Leave in Sweden. By Linda Haas. Albany: State University of New York Press, 1992. Pp. xi + 300. \$59.50 (cloth); \$19.95 (paper).

Marjorie E. Starrels
University of Michigan

Equal Parenthood and Social Policy is an important contribution to the literature on work-family linkages. It represents an ambitious and creative effort to trace the development, successes, and shortcomings of the parental leave program in Sweden, the most financially generous and

flexible in the world. Sweden adopted the program to increase the birth rate, recruit more women into the labor force, and "liberate" men from gender stereotypes.

Linda Haas confirms previous reports that men underutilize the program. She correctly attributes this to structural factors above all else: not only does the gendered organization of the labor force encourage women to emphasize domestic activities as a source of self-esteem, but workplace opposition, financial disincentives, insufficient social support, and structural competition between parents for leave (since only one can stay home at a time) inhibit men's leave-taking. Childhood role models and previous experience with child care are less important than contemporary influences in determining men's decisions, although family histories figure more prominently in women's orientations. She also found that fathers' and mothers' gender role attitudes predict men's behavior and that men are more likely to take leave if they have more than one child.

One of Haas's most interesting observations is that, although most men and women experience leave positively, women report more satisfaction and fewer problems. Thus, she raises an important question: Do women monopolize leave because they like it so much? This question is even more compelling in light of findings that (a) women with nontraditional gender role attitudes desire another leave more than other women, (b) mothers are typically unenthusiastic about fathers' leave-taking, and (c) the amount of time spent breast-feeding is associated with more positive and less problematic leaves for mothers. Women's unique ability to breast-feed may constitute an advantage that is difficult to surmount during a child's first year.

She therefore concludes that leave beyond the breast-feeding period can increase fathers' participation. Additional changes in social policy might include allowing both parents to be home together, thus eliminating structural competition, and an incentive system granting couples more leave when fathers take more time. However, expanding economic opportunities for women could accomplish the same goal more simply by making leave less appealing to women. Haas's conclusion is highly tenable: "Perhaps the best way to ensure that women do not find parental leave exceedingly attractive is to encourage their permanent attachment to the labor force and provide them opportunities for meaningful employment" (pp. 150–51).

Haas convincingly argues that political, economic, and ideological differences between Sweden and the United States create a less fertile environment for a similar leave program here. One might best garner employers' support by demonstrating economic benefits to the firm rather than less tangible payoffs to children, but, at present, evaluation research is scarce (see my "Evolution of Workplace Family Policy Research," *Journal of Family Issues* [13:259–78] for a discussion of this issue). And Haas's finding that financial reimbursement is crucial to men's leave-taking bodes poorly for equal parenthood in the United States. Prospects for paid leave are almost nonexistent, and we could expect few fathers to

take the unpaid leave potentially available through the proposed U.S. Family and Medical Leave Act. In addition, other barriers, such as women's traditional absorption in motherhood, discriminatory pay policies, and inadequate day care, are even more intractable in the United States than in Sweden.

One of the book's few weaknesses is that other methods of data analysis might have been employed. I was curious as to whether more sensitive techniques, such as log-linear modeling and logistic regression, would produce results different from those obtained through correlation and OLS regression. These techniques are more appropriate for bivariate and multivariate analyses of categorical data. I also had some reservations and questions about response categories and coding. Finally, Haas sometimes infers causality where it might not be warranted. For instance, she states that length of breast-feeding tends to reduce fathers' leave time. However, this relationship could be interpreted otherwise. Mothers might become more involved parents when fathers take less leave. One could expect greater sensitivity to competing interpretations: causality can be unidirectional in either of two ways, or it can be reciprocal.

Overall, Haas explores parental leave within its rich social and historical context. The book covers important terrain and explores some of the less understood facets and dilemmas of work-family policy. In addition, Haas raises provocative questions regarding the possibilities for true gender equality in Sweden, the United States, and elsewhere. Her study promises to be a springboard for further focused analyses of components in the social ecology of workplace family policy, such as workplace culture, social support networks, legislative process, and day-to-day decision making and other experiences within families. Because of its breadth, nuance, and strong theoretical and historical underpinnings, the book will be valuable to a wide readership.

Raised in East Urban: Child Care Changes in a Working Class Community. By Caroline Zinsser. New York: Teachers College Press, 1991. Pp. x + 188. \$36.95 (cloth); \$17.95 (paper).

Margaret K. Nelson
Middlebury College

Child care is an area of study in which there is much strong opinion about what is best and what should be done. In refreshing contrast, Caroline Zinsser's ethnographic study of child care arrangements in the small, racially and ethnically heterogeneous city she calls East Urban starts from the voices of those who provide and use child care and seeks to fashion policy in a manner that remains respectful of their needs.

The study focuses initially on "white, working- and lower-middle-class families who were first-, second- or third-generation descendants of mainly Italian immigrants" (p. 5), a group, Zinsser later learned, which

referred to itself as the "born and raised." Gradually, in order to understand fully both the context of the child care decisions of this group and, more significantly, how child care is arranged within a community, Zinsser widens the lens to include representatives of different racial, ethnic, and economic class divisions as well as to include at least a brief glance at licensed day care centers.

Throughout she remains committed to individual voices. Most chapters begin with the recounting of a personal story—how one woman selected her child care arrangements, how another woman began offering care for pay. Zinsser follows these accounts with "interpretive comments" (p. 6) that highlight the ways in which these particular decisions reflect a complex intermixture of cultural biases, economic constraints, individual preferences, and situational factors. Quite appropriately, this analysis is broadened in the end as these discrete ingredients are placed against the background of rapid social change brought about by the movement of increasing numbers of women into the labor force and the concomitant inadequacy of traditional styles of care (by family, by neighbors) to meet the growing demand.

But, in unexpected and uneven ways, Zinsser's analytic comments reveal her own biases and concerns. Although for the most part she is sympathetic to those who provide family day care (and in fact opens her book with the story of one such woman), she also draws on her extensive conversations with these providers to discredit them and to denigrate their services. She acknowledges that family day care providers (in their own terms, "babysitters") may offer care that secures safety and good health, care that is warm and nurturing, and care that is congruent with the values of many of the parents served. Even so, ultimately, she dismisses this care by applying the early childhood educator's term *custodial* (p. 75), and she makes obvious her preference for the care offered by those with professional training. When she has less information she remains dispassionate and nonjudgmental. Having engaged in more guarded interviews with the directors of licensed centers, and having had only limited access to observations of that style of care, her comments focus on such issues as economic constraints, ethnic and racial differences among center populations, and the attitudes of center directors toward parents rather than on the quality of the services provided.

If the issue of quality drops out of these latter discussions, so too does a concern with the needs and interests of providers. In describing family day care Zinsser is sensitive to the providers' working conditions, the loneliness and stress that are part and parcel of this work, and the minimal pay available for doing it. But the care givers disappear utterly in her discussion of licensed centers. Hence we learn nothing about their working conditions, their daily problems, their pay; nor do we learn anything about how the features of their work might differ among the different kinds of licensed centers (public and private, nonprofit and for-profit) she investigated.

Zinsser's use of literature is selective. She is highly knowledgeable

about research on child care, and she appropriately draws on these sources to inform her discussion. She has also done extensive background reading about the history of working-class urban families and, more particularly, the Italian-American families that are her special focus. But she ignores some obvious literature on gender that would put her findings into focus and help unravel their complexities. Thus while recognizing that white working-class communities rely on the willingness of women to care for children (and, as she poignantly shows, the elderly and disabled as well), she does not really look at the costs *to women* of these practices. Many of her descriptions thus take on a romanticized, quasi-nostalgic glow. And she fails to highlight the contradictory nature of work that, on the one hand, takes place within a framework of family norms, and on the other, requires commodification. If she acknowledges, for example, that a provider with professional aspirations may be "animated" by "feelings of service to others," yet need to "balance . . . idealism with the economic realities of day care" (p. 91), Zinsser does not identify the obstacles to finding that balance or analyze the toll of living within a society that presents women with these contradictory challenges.

Even with these problems and omissions, I enthusiastically recommend this book as an entrée into understanding child care issues. It starts with respect for its subjects. It raises challenging and unsettling issues in a highly accessible manner. And it remains true to its insight that "there is no one model of child care that will answer the needs of all families" (p. 168).

Feeding the Family: The Social Organization of Caring as Gendered Work. By Marjorie DeVault. Chicago: University of Chicago Press, 1991. Pp. 270. \$24.95.

Sarah Fenstermaker
University of California, Santa Barbara

Feminist scholars interested in work and gender aspire to the exploration and articulation of how the two operate together: how work relations and gender relations are not only compatible, but often *ideal* partners in the production of social life. *Feeding the Family: The Social Organization of Caring as Gendered Work* contributes greatly to that project. Based on 33 in-depth interviews with (primarily) Chicago women who take responsibility for the provisioning, caring, and feeding of their families, Marjorie DeVault explores the two related aspects of household labor and nurturing, or "caring work," and the variety of social dimensions that bear on them. We get a glimpse of how women speak of the complicated process of planning both provisioning and meals, the ways in which women approach food shopping, and the actual production of meals. Along the way, and by no accident, "families" (and with them gender, class, and generation) are "produced" as well. As DeVault herself puts

it, "In my analysis the social organization of the family setting provides a way of understanding both how women are recruited into the work of feeding, and also how feeding work contributes to women's oppression" (p. 12).

Thus the analysis locates itself within the tradition of those studies of household labor and family that see the social organization of gender, class, and race as crucial to the production of both material relations and their meaning for systems of subordination. DeVault's focus on "feeding"—broadly conceived—may at first glance suggest a narrowness that would limit the scope of the analysis. DeVault's analysis itself reveals why such a focus is instead sociologically astute; as I read I was frequently struck by how powerful a "gateway" feeding and caring work may be to the establishment of conventional gender relations within the household. Household tasks may vary in how resistant they are to innovative reallocation within the family, since some may convey greater threat to the sense of what is right and "natural" in the construction of families. Along with the parenting of small children, feeding work may be the crucial area that best establishes the social organization of traditional family and thus its relations of domination.

The book has some regrettable weaknesses, although they are outweighed by its strengths. The author consciously chose not to organize the book by "independent variable," where respondents or their families are analyzed by age, number of children, race, class, and so on. This approach can have great merit, but when such dimensions proved crucial to the analysis, they were simply noted, and biography seemed an afterthought. There was often little continuity between chapters, and, as a consequence, the work suffered from a sense of disjointedness, as if one were reading separate journal articles. The strong chapters displayed a mindfulness of the necessity for the researcher to get out of the way of her respondents. But in others, respondents seemed nearly absent, and their voices stilled by the author's restatements of their lives and sensibilities. For her analysis DeVault turns to the conceptualizations of Candace West and Don Zimmerman ("Doing Gender," *Gender and Society* [1 (1987):125–51]) in which gender is conceived to be not a fixed attribute but an accomplishment, played out in the daily interactions of men and women, marked by an expectation that participants may be called to account for themselves as appropriately "gendered" men and women. This framework holds great promise for the analysis of the actual workings of gender (or race or class or age, for that matter) as it unfolds with many other human activities. DeVault depends upon it for her notion of feeding work as crucial to the production of "families" themselves, and to the daily construction of women's subordination. Yet, short of periodic invocations, unfamiliar readers are not given a real chance to understand the framework, nor will they see how such a theoretical orientation actually helped make sense of the data. This is disappointing, since the theoretical argument could be a powerful one, and would heighten the sociological import of the book.

The jewel of this work is the chapter "Affluence and Poverty," in which the class relations surrounding feeding activities are given center stage. The skill with which DeVault analyzes the data, especially from her economically impoverished respondents, her sociological insights about caring in the face of poverty and psychological depression, and her ability to extend the past analyses of how gender and class operate together in the course of accomplishing feeding work is worth the price of the whole book. With the entire volume, we are helped to understand how the work lives of women—spent day by day—bring into being relations of caring, of family, and of their own domination.

Women with Alcoholic Husbands: Ambivalence and the Trap of Codependency. By Ramona M. Asher. Chapel Hill: University of North Carolina Press, 1992. Pp. ix + 223. \$29.95 (cloth); \$12.95 (paper).

Norman K. Denzin

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For nearly four decades Joan K. Jackson's 1954 doctoral dissertation, "Social Adjustment Preceding, During and Following the Onset of Alcoholism" and the classic articles from that dissertation ("Alcoholism and the Family," "The Adjustment of the Family to Alcoholism") dominated understanding of the alcoholic family and the wife's place in this family. Jackson's study of the reports of Al-Anon (the support group for significant others of alcoholics) wives produced a functional theory of family crises, emphasizing a seven-stage model of the role realignments, disorganization, and reorganization that occur as the family adjusts to living with an alcoholic. Noticeably absent from the Jackson study was any systematic treatment of the Al-Anon philosophy and its ideological approach to women and the family: an approach stressing "tough love" and a commitment to saving the alcoholic marriage.

Ramona M. Asher's powerful new book builds on, but goes beyond, the Jackson framework. Unlike Jackson, Asher offers a detailed reading of the codependency-Al-Anon literature, and examines how this interpretive framework shapes a wife's understanding of her marriage and her social situation. Like Jacqueline P. Wiseman's *The Other Half: Wives of Alcoholics and Their Social Psychological Situation* (New York: Aldine de Gruyter, 1991), Asher takes a symbolic interactionist (Mead, Blumer) approach to the alcoholic wife and her situation. (Wiseman interviewed 52 women who were married, on average, 11 years to an alcoholic spouse.) The Asher and Wiseman works move Jackson into the contemporary period.

Deploying an experience-distant (Geertz), four-stage career model (early problem identification, problem amplification, proximal treatment, and posttreatment) Asher offers the concept (partially taken from Merton) of definitional ambivalence to describe the incompatible, multiple, and

competing definitions that confront women married to alcoholic husbands. In careful, meticulous fashion, she outlines the interactional contingencies and strategies (sporadic questioning, confronting, self-doubting, self-assertion, reflection) that occur as wives move from denial to acceptance of their situation. She suggests that the wives of alcoholics move through a number of sequential phases that build on one another, including: being married to someone who causes trouble, learning to define these problems as being connected to alcoholism, experiencing definitional ambivalence, learning how to manage ambivalence, seeking help from support groups, and learning how to retrospectively define themselves as codependents in need of treatment.

Two phenomena, according to Asher, are central to a wife's movement through these stages and phases: Al-Anon and the codependency movement. (In places Asher confuses these two movements and their respective ideologies. The codependency movement has now spawned its own groups, CODA, which are different from Al-Anon. Out of CODA has emerged the Adult Children of Alcoholics [ACOA] movement. These new groups are not the same as Al-Anon, and are often antagonistic to the Al-Anon ideologies.) From Al-Anon women learn that alcoholism is a disease that they did not create. From the codependency movement they learn that they have been co-alcoholics, or codependents, dependent on their husband's alcoholism for their own sense of well-being. These two belief systems produce two understandings for the wife of the alcoholic: she has the disease of codependency and hence is sick, and she must seek treatment, for she cannot make her husband better. That is his job. Thus in order for a woman to get better, she must proclaim her sickness. She learns the culture of codependency in Al-Anon where she comes to throw off the negative identities previously attached to being the wife of an alcoholic. (Asher may gloss over this process, for some Al-Anon groups rigorously resist the language of codependency.)

Asher examines the negative consequences of this self-transformation. Codependency fosters a new negative identity for women, asking them to take on a new form of illness, and wifely deviance. It creates a new disorder with ambiguous psychological and behavioral dimensions. This process of medicalization then contributes to an ideology that recommends a life-long process of recovery. For many women this is a no-exit model; they may remain trapped inside Al-Anon or other recovery groups. For others there may be a progressive commitment to a transcendent perspective that constantly seeks new forms of self-development. And some may stand still in recovery, endlessly echoing recovery ideology. Some women may become trapped inside a lifetime search for a new self that can never be found, seeking, without direction, new recovery identities. These individuals may experience a sense of marginalization, or distance from the dominant culture and view themselves as perpetual outsiders.

As a particular type of deviance becomes a middle-class problem, the probability of medicalization increases, in direct relationship to the new

problem's economic profitability. Wives of alcoholics are in this situation, and they may continue to find themselves pulled into the codependency programs Asher describes. There is an additional dimension to this medicalization process. Historically women in this culture have had second-class citizenship. Codependency, some argue, reproduces this process. It is women, not men, who are commonly defined as codependent. If women had more power in this culture, would they be defined as codependents?

This is the negative side of the codependency movement. On the positive side this movement signals a new direction for American women and their relationship to the family system. In the 1950s Jackson's women did not see themselves as suffering from a disease or illness. They were quite content with getting on with the business of raising and running a family. Their fulfillment came from within the family itself.

Asher's (and Wiseman's) wives blamed and faulted themselves for their husbands' problems and perceived themselves as having gone "through three phases: . . . ignorance, discovery, and recovery" (p. 178). In recovery Asher's women strike out on their own, break free of their husband's control, create and discover new careers outside the family, and look back critically on the family system that produced them. These women, unlike Jackson's wives, no longer seek fulfillment entirely within the family. This is what the ideology of codependency and recovery gives them. In this movement lie the kernels of radical social change. If women are being taught to break free from the family and to seek their identities outside the patriarchal-marital bond, then husbands too may be free to no longer reproduce the patriarchal structures they think they have to enact.

In this, as usual, women lead the way, even as they are stigmatized for doing so. Thus the languages, terms, and slogans of recovery from alcoholism (denial, discovery, codependency, "no pain, no gain," "good can come from bad") transcend the specifics of alcoholism. They refer back to something deeper, namely the gendered selves and intimate relationships between men and women that this culture promotes. Asher might have made more of this.

Women, Violence, and Social Change. By R. Emerson Dobash and Russell P. Dobash. New York: Routledge, 1992. Pp. 366. \$49.95 (cloth); \$17.95 (paper).

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As of June 1992, a woman was killed every eight days by a partner or ex-partner in Massachusetts (*Boston Globe* [June 5, 1992]). The American Medical Association declared that domestic violence had reached epidemic proportions and required immediate response from public health

personnel. And, several states considered "stalking laws" as avenues to protect women from male harassment and intimidation. The public increasingly became aware of circumstances well known to feminists—too many women were abused, tortured, and killed by the men with whom they tried to form intimate relationships.

Women, Violence, and Social Change is therefore a timely book. The authors, self-described activist researchers, propose to document the development of the battered women's movements in America and Great Britain. They clearly are advocates for this cause, often underscoring the lifesaving nature of the movement's mission. The book is framed as a sociological inquiry into the growth of the social movements, the relationships of the movements to local and national institutions, the attempts by activists to challenge and alter cultural patterns of misogyny, and the ways in which innovations are thwarted or compromised.

The authors partially succeed with their agenda. The book opens with an overview of the nature and extent of battering. For readers unfamiliar with this problem, the stark and powerful testimonies of battered women will seem like a prisoner's report from Amnesty International. They indicate the need for and urgency of the battered women's movements. More such accounts would have enhanced the remainder of the book.

The next several chapters document the rise of the movements, the essential strategy of offering safe shelter, and the often paradoxical relationships with the state, judicial systems, and legislative processes. The analysis moves back and forth between the United States and Great Britain. In both cases, movement activists confronted the dilemma of working with status quo institutions, which acted as both oppressive mechanisms and routes to change. The reader gains a sense of the strategic debates and the enormity of the task that movement activists faced.

Unfortunately, these chapters also were quite frustrating. Too much material was presented in a manner that obscured, rather than illuminated, movement understanding. Observations and information often were extraneous to or not well integrated into the arguments. For example, the authors note that the socialist tradition is more central to British, than American, feminism. This potentially interesting explanation for some key differences goes undeveloped. The overview of the movement literature amounts to a listing of different models, virtually none of which is referred to again. In discussions on feminist theory and on the rise of the modern state, much of the background material distracts from the presentation. A categorization of shelter types, which confounds ideology, strategy, and authority, is not incorporated into subsequent analysis. The book often reads like a maze in which the directions of the paths are neither apparent nor connected.

The authors are stronger in the final chapters. In chapter 7, "The Therapeutic Society Constructs Battered Women and Violent Men," they critique the individualized, psychological approach often used in the United States to "treat" both victims and perpetrators. Within this framework, women often are blamed for male violence, and men are

viewed as pathetic creatures unable to be held accountable because of psychological or biological factors. The authors rightly criticize mental health professionals for depoliticizing family violence and for engaging in "blame the victim" interventions. This development has undermined the radical nature of the battered women's movement.

In chapter 8, "Knowledge and Social Change," the authors dissect research that claims that women are more violent than men and that the battered male syndrome more accurately reflects domestic violence. Their critique of both the reasoning and methodology of this research provides ample ammunition to dispute such claims, and suggests the ways in which the generation of knowledge influences daily struggles.

The developments described in chapters 7 and 8 are much more prevalent in the United States than in Great Britain. The authors suggest several factors relatively unique to the United States that conspire to blame women and underestimate male violence: the cultural value of individualism, the conservative approach used by many family sociologists, and the "fetishism" over survey methodology within the positivist tradition. In both chapters, the authors' political values contribute to well-crafted and focused arguments.

The final chapter offers a much-needed, succinct comparison between the two countries. Had the entire book been constructed in a similar vein, a clear, comprehensive examination of a vital social movement would have resulted. Though the book falls short in terms of concise sociological analysis, it serves as a testament to the lifesaving work of the battered women's movement.

On Divorce. By Louis de Bonald. Translated and edited by Nicholas Davidson, with an introduction by Robert Nisbet. New Brunswick, N.J.: Transaction Publishers, 1992. Pp. xxvi + 204. \$24.95.

Robert Alun Jones
University of Illinois

In 1792, at the height of the Revolution, the French made divorce legal on the grounds of incompatibility or mutual consent. Napoleon stiffened the conditions to adultery, cruelty, or grave injury, effectively eliminating divorce in the provinces and limiting successful applicants in Paris to about 50 per year. With the Restoration, however, conservatives gathered new strength and, after his dramatic, pivotal speech in the Chamber of Deputies, the aristocrat Louis-Gabriel-Ambroise de Bonald (1754–1840) was commissioned to write a government's report opposing divorce unconditionally. In doing so—as in his earlier speech—Bonald relied upon *Du divorce*, one of several works he had written while hiding from the authorities in Paris between 1800 and 1802. In 1816, divorce was abolished altogether, to be reinstated—in a form more restrictive than the Napoleonic legislation—only under the Third Republic in 1884.

Born to a noble family established in the Rouergue since the 14th century, Bonald was raised in a devout Roman Catholic household. Despite the anticlerical atmosphere of the Enlightenment, his childhood faith never diminished, and later he effected several notable conversions, including those of the Swiss philosopher Karl Ludwig von Haller and Madame Julie Charles, the mistress of Lamartine. He received a rigorous classical, theological, and mathematical education at a school run by the Oratorians at Juilly, entered the Musketeers, where he remained until that corps was suppressed in 1776, and then returned to his hometown of Millau where, as an aristocrat, he was compelled by the local *intendant* to serve as mayor. Elected president of his newly created department at the outbreak of the Revolution, Bonald resigned upon the promulgation of the Civil Constitution of the Clergy, which in 1790 placed the Gallican Church under the control of the state. As the Revolution became more violent, he emigrated to Heidelberg where he composed his *Théorie du pouvoir politique et religieux* (1796), an attack on the doctrines of Rousseau and Montesquieu. As the worst excesses of the Terror cooled, he returned to France, but was soon obliged to hide again, this time in Paris where, in addition to *Du divorce*, he wrote his *Essai analytique sur les lois naturelles de l'ordre social* (1800) and *Législation primitive* (1802). As Napoleon restored order to France, these writings acquired a following, although Bonald—faithful to the royal line—resisted the emperor's patronage. With the Restoration, however, Bonald threw himself again into public life, playing a decisive role in the debate over divorce, and producing two major works—*Recherches philosophiques sur les premiers objets des connaissances morales* (1818) and *Démonstration philosophique du principe constitutif de la société* (1830)—before his death in 1840.

This chronology is background for the most ambitious claim of Nicholas Davidson's introduction to *On Divorce*—his insistence that, Bonald's excesses and Edmund Burke's achievements notwithstanding, it is Bonald to whom conservatives should now turn. Writing in England in 1790, Davidson argues, Burke's task was one of conserving those traditional institutions so clearly threatened by the Revolution in France. Writing in France a decade later, Bonald's problem was that of restoring institutions—particularly those associations, like the family, "intermediate" between the individual and the state—already devastated by reform. And to do this, Davidson insists, Bonald was forced to seek out explanations for those institutions that Burke had left unexplained—in short, to write sociology as well as political theory.

At the core of Bonald's sociology is what Davidson calls the "resonance phenomena of morality"—that is, the Aristotelian notion that, if something is wrong morally, it must have "resonance effects" on the social fabric. Bonald's first four chapters, for example, are devoted to showing that marriage is "naturally indissoluble," because its dissolution would conflict with the "principle" (or natural tendency) of both domestic and public society. The subsequent chapters explore the destructive effects of divorce, on not only the family, but also the state, religion,

education, the economy, and so on. The "peculiar insanity of our time," Davidson insists, is our tendency to see such issues as disconnected elements—for example, to view divorce as a "women's issue"—thus our failure to grasp the more holistic nature of human affairs.

These Durkheimian emphases on the value of intermediate associations and the organic interconnectedness of social phenomena are well taken. Ultimately, however, the primary value of reading Bonald will lie, not in his similarities, either to Durkheim or to our ourselves, but in his differences from us—well preserved, incidentally, in Davidson's careful translation. Whatever our own commitments, we should be grateful to Russell Kirk's Library of Conservative Thought—to which this is a contribution—for conserving a language that most of us no longer speak.

The Resilience of Christianity in the Modern World. By Joseph B. Tamney. Albany: State University of New York Press, 1992. Pp. x + 178. \$16.95.

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University of Warwick

One of the ironies of the sociology of religion is that, for all the fashionable emphasis on religious change and ferment in some parts of the world, sociological books on the topic tend to follow a predictable format. Rates of religious conversion or recruitment are high on the agenda. The changing functions of religious professionals are likely to attract attention, and the fortunes of new religious movements probably loom large in the analysis. The underlying concern is overwhelmingly with the balance between religious commitment and secularization, with the latter prevailing.

Joseph Tamney's *The Resilience of Christianity in the Modern World* is completely different, however, in several respects. First, it is concerned with the conditions affecting the *popularity* of the Christian religion in various places. There is no attempt to explain the essence or the inner dynamics of religious organizations. Instead, the book's focus is resolutely on the factors that affect the extent to which people find Christianity appealing in the modern Western world. The main factor turns out to be the degree of compatibility with modernity, but others include the extent of state support, the breadth of needs served by religious organizations, the overlap between religion and ethnic or national identity, and the capacity to accommodate an alternative tradition and social traditionalism. The general thesis is that, while secularization has undoubtedly taken place, the widely taken-for-granted association between modernization and secularization is not only an excessively simple representation of a complex pattern but also a failure to understand why some forms of Christianity are more popular than others.

A second distinguishing feature of Tamney's book is its insistence on

a sharp, if quirky, delineation of "modernity." The word is not used casually but is deliberately associated with three "ideational sets"—socialism, libertarianism, and the counterculture—which have challenged, and elicited responses from, churches in various ways. The book's central section is an examination of the extent to which Christianity has allegedly accommodated these modern ideas sufficiently to retain the kind of popularity that it had enjoyed in premodern times and places.

The third innovation of Tamney's book is its coverage of Christianity in three very different places—Poland, Latin America, and the United States. His analysis of the relations between the Solidarity movement and Catholicism in Poland¹ highlights the importance of the church's connection with ethnic and national identity as well as its willingness (at least until recently) to accommodate socialist ideals. Although the Catholic hierarchy in Latin America was wary of the socialist and libertarian tendencies of Liberation Theology, popular support for grass-roots communities was strong at a time when state agencies were repressive and when nonelite priests were prepared to indulge ethnic, tribal, and regional adaptations of "official" Catholicism. His study of the United States is different because Tamney selects the counterculture, not socialism, as the main challenge to Christianity here. His argument is that U.S. Christianity, especially mainline Protestantism, has declined in popularity because it has failed to accommodate adequately the counterculture's four main themes of libertarianism, the alternative tradition, self-realization, and the affluence ethic. The main exceptions include the Jesus movement and the Charismatic movement (which adapted parts of the alternative tradition), ritual renewal movements (which reflected aspects of libertarianism), and, more surprisingly, fundamentalism (which adapted the communal emphasis of the alternative tradition). By contrast, the Christian Right was popular until the mid-1980s precisely for *resisting* the cultural aspects of modernization and for protecting social traditionalism. Nevertheless, Tamney is careful to specify the limits to the popularity of all forms of U.S. Christianity. His conclusion is that, although "future societies will be religious" (p. 149), the popularity of organized religion will generally decline because modernity imposes conditions that cannot be met without stifling Christianity.

Tamney's book is brief and programmatic. It contains very little information not already known to religion watchers, and its scholarly apparatus is slight. For example, references to journalistic writings are frequent and those to heavyweight sociological treatises conspicuously absent. He uses evidence to illustrate rather than to test his theoretical scheme. Yet, the overall effect is positive because the book succeeds in tackling some comparative questions and avoiding the stale debate about secularization. Even though many of its generalizations are questionable, that is a price worth paying for a refreshingly novel and provocative interpretation of modern religion. Graduate students might find it too elliptical, but teachers of the sociology of religion will find Tamney's general approach refreshing.

God's Warriors: The Christian Right in Twentieth-Century America. By Clyde Wilcox. Baltimore: Johns Hopkins University Press, 1992. Pp. xx + 249. \$34.00.

Erling Jorstad
St. Olaf College

God's Warriors is a systematic, well researched, and clearly argued analysis of a favorite, if widely misunderstood, movement of the 1980s, the new Christian Right. Clyde Wilcox of Georgetown University presents a carefully nuanced, thoroughly documented history and interpretation of that phenomenon and his own muted but thoughtful explanation of why highly religious citizens become active in the secular world of elective politics. The results stand as the most persuasive study among the several such works on the now-defunct Christian Right currently available.

Drawing on his doctoral dissertation and on several of his published case study monographs, Wilcox weaves together crisply worded answers to five questions he sets out in the Introduction: Who were the movement's supporters during the 1970s and 1980s? How did they differ from other conservative political activists? What were the results of their involvement? What was the extent of the unity among the several groups involved? and What potential for future political mobilization resides in their ranks?

To answer these, often using highly sophisticated behavioral and statistical analyses (definitely not data for the untrained reader), Wilcox offers four arguments, which in essence are his answers to these questions. First, the Christian Right consisted of several, often hostile religious groups, creating a situation with more discontinuity than continuity for organized political action.

Second, and this is the key argument in the work, their support for candidates such as Pat Robertson was not based on "psychological abnormality, social-status anxiety, or other sources of strain" (p. 224). Christian Rightists voted as they did because of their explicit religious and political values. Contrary to several earlier behavioral-scientific interpretations, Wilcox argues that Christian Right behavior was rational because it did not "result from pathological psychological forces"; further, it indicated these voters were supporting groups espousing their own explicitly defined values and beliefs (p. 224).

Third, for all the media hype given them, Christian Rightists really were a negligible voting bloc in the 1980s; they were able to do very little to mobilize heretofore apathetic evangelicals into voting, or convert evangelical Democrats into voting Republican. Fourth, Wilcox uncovers solid evidence that many evangelical blacks found their way into Christian Right citizen organizations, a situation not systematically studied before this.

The book starts with a careful analysis of the first "Christian Right" organization of the era, the Christian Anti-Communism Crusade led by

Fred Schwarz. Wilcox goes on to show why it fell short of its goals, but helped to create at least an awareness among evangelicals of the potential that organized politics presented for implementing their moralist agenda.

Then in the heart of the work, the author takes apart earlier theoretical explanations of Christian Right behavior—those of personality makeup, alienation, social status, symbolic politics, denominational affiliation, prior political loyalty, and geographic explanations. Using highly sophisticated statistical evidence, already scrutinized as it appeared earlier in social science journals, Wilcox makes his case for understanding the phenomenon largely as that of informed voters selecting candidates on the basis of supporting those office seekers who offered positions compatible with the electorate's most cherished religious and moral values. If anything, the author repeats his major thesis perhaps a little too often, but the reader has no doubt where the author stands.

Of immediate interest to readers of this journal are two items: the viability of this form of research and evidence and, more broadly, an attempt to explain why religious and political conservatism seem to have such an affinity for one another. On the first, Wilcox carefully follows the canon of behavioral/statistical research, showing in his specialist's vocabulary the sources of his interpretation. On the latter, he allows himself to be more speculative. Establishing Christian Right loyalty to Biblical teachings, he shows the Bible is often silent or at least ambiguous about convictions the Christian Right hold most dear. He offers several speculative answers to this question, acknowledging that no final answer is as yet available.

In sum, this work is the most helpful analysis we have yet on this fascinating phenomenon. Its conclusions about the value of earlier sociological theories as explanations for political behavior may not be the kind of interpretation some other specialists may endorse. Yet for me, the case is persuasive, based as it is on solid research and an explicit analysis of how religion and politics interact.

Fundamentalisms Observed. Edited by Martin E. Marty and R. Scott Appleby. Chicago: University of Chicago Press, 1991. Pp. 872. \$40.00.

Benton Johnson
University of Oregon

This is the first in a series of six volumes to be published by the Fundamentalism Project, which was funded by the American Academy of Arts and Sciences for the purpose of investigating contemporary fundamentalist movements in various parts of the world. The first volume is intended as a descriptive survey of a wide variety of movements.

The book consists of 14 chapters on movements in the Americas, the Middle East, and South and East Asia. Four chapters deal with Islamic movements, three with Christian, and two with Jewish movements. The

remaining five treat movements within Hinduism, Sikhism, Theravada Buddhism, Confucianism, and the traditions of Japan. There is virtually nothing about European fundamentalism and nothing at all about fundamentalism in sub-Saharan Africa, the former Soviet Union, or mainland China. Although none of the contributors to the volume are themselves fundamentalists, all are seasoned investigators of the movements they describe, and most make a serious effort to avoid partisan judgments. The essays are highly informative, well written, and accessible to a readership of educated nonspecialists. Many justly deserve to become standard reference works. The editors have contributed a concluding chapter and a brief user's guide.

The term fundamentalism as applied to religion originally referred to the movement that arose in the first decades of the 20th century in opposition to the liberal drift in theology within the old-line Protestant denominations of North America. In the 1940s, Talcott Parsons and others began using the term in a more general way to refer to any religious and political movement that resists what Max Weber called the process of rationalization, or what recent theorists variously call the process of Westernization, modernization, or globalization. Liberal internationalists like Parsons regarded fascism as a virulent form of political fundamentalism; today many in the West regard the new brands of militant Islam as a virulent form of religious fundamentalism. The Fundamentalism Project is a scholarly reflection of a fear that these and similar religious movements may impede progress toward a liberal world order.

The contributors do not agree on all the identifying marks of fundamentalism. Some, for example, restrict the term to politically mobilized movements, whereas others extend it to include quietistic and retreatist movements as well. A few authors express doubts that the movements they write about are unequivocally fundamentalist at all. Even the editors make a distinction between "pure fundamentalists" and "first or distant cousins."

There is agreement, however, that the movements described share several important features. All attempt to draw strict boundaries between themselves and outsiders, all engage in a selective retrieval of elements of tradition from a sacred past, all make innovations in doctrine and practice, and all accept to some degree the scientific, technological, and organizational manifestations of modernity. As for selective retrieval, Protestant fundamentalists militantly oppose abortion but most have quietly dropped their traditional opposition to birth control, and some have no objection to light social drinking. As for innovation, the politicization of the martyrdom motif in Shi'ite Islam, which was indispensable to the success of the Iranian revolution, was a striking departure from tradition. As for modern science and technology, militant Sikhs now ride motorcycles and carry revolvers, and in Israel a small cadre of geneticists and livestock farmers is attempting to produce the anomalous red heifer whose ashes, as tradition decrees, will be needed to purify the Israelites when the moment arrives to ascend the Temple Mount.

Moreover, all these movements were founded and are led by males, all attempt to create a following of religiously articulate, energetic people living disciplined and uniform lives, and most have a core constituency that is largely urban and middle class. In almost all cases, the major themes of the movements were devised by lay or clerical intellectuals a generation or two before their actual founding. In an effort to mobilize support, many movements, especially in Asia, oppose both traditional religious elites and traditional popular practice, thereby threatening the continuity of a nuanced, text-based elite culture, on the one hand, and a syncretic, "superstitious" mass culture, on the other. In addition to promoting a distinctive, group-focused identity, all the movements vehemently oppose some alien "other" that is perceived to threaten the integrity of sacred tradition. Among Christians and Jews secularism and liberalism are the enemy; in Asia and in Muslim Africa the other is usually the West itself, especially its supposed hedonism, materialism, and immorality.

Succeeding publications of the Fundamentalism Project will trace the effects of fundamentalist movements on their host societies, explore in detail what these movements have in common, explain why they have arisen, and discuss their implications for public policy.

This large volume, though a bit unwieldy, is sturdily bound, attractively printed, and generally free from misspellings and other errors of usage. Unfortunately, a few solecisms, such as "congerly," "publically," "mitigated against," and "the reigns of government" escaped the attention of the editors.

Representing Belief: Religion, Art, and Society in Nineteenth-Century France. By Michael Paul Driskel. University Park: Pennsylvania State University Press, 1992. Pp. 282. \$48.50.

Julia Bernard
University of Chicago

This book is about an unfortunately neglected area within art-historical research on the modern period, written by a scholar whose knowledge about his topic is impressive. As soon as one has read its title, however, some potential difficulties are already evident, for the "belief" that religious art is presupposed to "represent" here was of course a problematic quantity during the era in question, and in effect this author conceives the period under examination as encompassing en bloc an entire century—one characterized by rapid, pronounced change. Michael Paul Driskel solves the former difficulty by equating religion with the institutional Catholic Church and concentrating largely on its most reactionary elements, while he precludes the latter issue by focusing on the 1830–70 segment within that 100-year framework and attempting via an abstract schema to apply the same organizing terms throughout. Operating under

the double aegis of social-scientific impartiality and art-historical revisionism, this book, by limiting its range of inquiry, inevitably articulates a conservative position.

Specifically, Driskel's chosen approach to a potentially vast body of textual material and corpus of images is informed by the discipline of structural linguistics, as well as supported at various points by premises drawn from the theory of Michel Foucault. Referring to the framework of oppositions by means of which language is construed to convey meaning in the former, the author structures his discussion with two propositions. First, Driskel isolates two general stylistic tendencies—the "hieratic" and the "naturalistic"—which he opposes to each other. Thus he argues that during about the first half of the century, the hieratic (symmetrical, static, linear, frontal) mode was associated with a reactionary political stance and rigid Catholic dogmatism, while during that same period the naturalistic (painterly, narrative, realistic) orientation was, by contrast, representative of a progressive, secularizing, and egalitarian sociopolitical stance.

The former "category" is further associated by Driskel with an "*arrière-garde*" or mainline artistic orientation, while the latter quantity is linked with an avant-garde aesthetic affiliation. He then posits that towards the century's end the opposition represented by these two pictorial modes within the genre of religious art *reversed* itself. That is, that a version of the hieratic style became associated with a fin de siècle artistic avant-garde, at the same time that the naturalistic tendency came to be adopted by a conservative group within the aesthetic realm as well as society. Clearly the primary limitation of such a schematic arrangement lies in that this pair of terms is conceived of by Driskel as having simply exchanged significations (which are presumed to have the same meaning 100 years later) with each other, so that the possibility of any evolution is denied. (A similar problem is presented by his central religion vs. progress dichotomy, as some of the more interesting phenomena of this period are efforts to *reconcile* that opposition: Saint-Simonianism, e.g., or the occult sciences.)

Not surprisingly, then, since it is concerned with the religious art of the 19th-century's last two decades, this book's final chapter is its least convincing. Composed of quotations from isolated sources, which are often misleading because taken out of context and unbalanced while other, equally important tendencies (such as that of occultism) are underplayed, this chapter entitled "Under a New Sign" strings such material together employing generalizations and juxtaposing several disjunctive arguments whose implications are not fully understood. The author also selects only those fin-de-siècle avant-garde artists whose work fits his hieratic paradigm—predictably as this is, in general, a text about "discourses" rather than images, so when pictures do not reflect what is said about them he often makes excuses instead of rethinking. This unsatisfying account of the century's conclusion culminates with what amounts

to a retraction of the basis of Driskel's whole argument: "Amid this explosion of signifieds, drawn from all manner of religious traditions and mystical writing, the hieratic mode no longer retained any efficacy as a vehicle or a system of signs to designate and differentiate any specific ideology or system of beliefs" (p. 252).

Similar admissions of the untenability of his own premises punctuate this author's text at regular intervals, throwing into question his belief in the analytical framework with which he so resolutely began ("both art and the social reality in which it is produced *are* organized systematically and, no matter how incomplete the results may be, it is the charge of the social historian to uncover and analyze those organizing structures" (p. 18), yet somehow these do not prompt a reevaluation and he merely proceeds. But the most serious reservation one finally has in response to this study has to do with its conservatism: generated by the larger space given to the art designated as hieratic by Driskel (only one long chapter out of six is devoted to its rival trend), in conjunction with his negative evaluation of the naturalistic tangent's contributions (condemned as marked by a "fundamental contradiction" and "ambivalence"). This impression is furthered by the revisionist orientation of his whole project, as becomes clearest when Driskel points out with relish that in many ways there was no difference between the productions of fin-de-siècle avant-garde and the academic Salon painters—ignoring that by that point in time, the latter had been influenced by the former.

It would be unfortunate if all efforts to employ a "value-free" sociological approach to art history were to share such a reductive and reactionary agenda.

The Emergence of Liberation Theology: Radical Religion and Social Movement Theory. By Christian Smith. Chicago: University of Chicago Press, 1991. Pp. vii + 300. \$35.00 (cloth); \$14.95 (paper).

Marie Augusta Neal, SND
Emmanuel College

In a carefully researched and methodologically integrated exploration of liberation theology perceived as challenging the social, political, and economic structures of most, if not all, Latin American states, Christian Smith claims that liberation theology, unlike other theologies learned and speculated on in university settings, is itself a social movement to effect radical change in the social, political, and economic institutions of Latin American society. He concludes that it falls into the category of "elite-initiated revitalization movements that aim to force drastic redirections in the strategies and resources of established institutions" (p. 234). Smith's primary interests are sociological. He is testing the effectiveness of Doug McAdam's political process model of social movements, in preference to

breakdown deprivation theory or resource-mobilization theory, because he perceives it as more effective for examining a historical reality in which what he calls "insurgent consciousness" plays a central part.

Suggesting that the emergence of liberation theology has a significance equal to the Protestant Reformation of the 16th century and the Great Awakening in 17th-century America, Smith promises to demonstrate how this theology is, in fact, a social movement aligning the institutional Catholic Church with the poor with the intention of securing their liberation from poverty and oppression. He predicts it will spread not only throughout Latin America but also into India, the Philippines, Africa, and then back to North America and Europe, creating in the process a new level of church participation at the grass roots but at the high price of arrests, torture, and deaths of committed bishops, priests, sisters, and laity.

Smith expresses two clear interests: (1) finding an answer to the question "How and why did liberation theology emerge when and where it did?" (p. 6), and (2) investigating whether a theoretical model can be used effectively with historically accurate data derived from records, statistics, and interviews to reconstruct causal sequence in social movements. While reading the first three chapters of his book the reader becomes aware of how consistently the author adheres to these two tasks. In reading part 2, one can easily forget the theory at will and simply pursue the story as it unfolds through time, even though the author continues to use his chosen model effectively.

Smith promises a thorough analysis of the historical setting of liberation theology and provides it in an interesting way by vividly reviewing the span of the history of the Catholic Church in Latin America from the initial colonial encounter with Native Americans in the 15th century, through its repudiation by forces for independence in the 19th century, to its striking revitalization sequenced through the pressures from communism's challenge to these secular democratic states. Finally he focuses his extended analysis on the influence exerted by a rich array of dedicated mid-20th-century theologians who are learning and teaching a new theology. This theology not only announces that the land belongs to the people, but converts those very people to the conviction that God mandates them to claim it as their own, through a biblically rooted critique of the church's historical and now sinful allegiance to unjust social structures.

Guided by the McAdam model of social science, Smith hypothesizes that social change occurs in a social system when there is a peculiar blend of three variables: (1) a political environment characterized by expanding political opportunities, (2) organizational strengths, for example, the presence of a strong indigenous organization, here the entrenched Catholic Church, and (3) "insurgent consciousness," the category he uses to identify the newly developing liberation theology. The key factor in his analysis is the reality of the continuing exploitation of the dispossessed by a society characterized by pockets of fabulous wealth, its possessors' claiming rights and privileges beyond any human need, while the majority of

the people unnecessarily live below subsistence. This reality, claims Smith, cannot withstand a theology learned in an interactive setting, whether in workshops away from home or at home in the thousands of Base Christian Communities (BEC) which have sprung up all over Latin America. Here at the base, pastoral agents, priests, sisters, and laity, trained by a small number of peripatetic liberation theologians traveling across the whole continent and risking torture, imprisonment, exile, and death, work together in solidarity to keep the movement going.

This book has an extensive, rich bibliography and includes, in interviews and meetings, the major actors in this liberation movement. For this reason, it is an excellent source for pastoral ministers as well as for sociologists. The former will discover how pragmatically a sociologist approaches the holy and may be disenchanted; the latter will be challenged to try to add a variable that measures faith commitment and will probably conclude that it cannot be operationalized. Both will be puzzled by the last section of the book wherein Smith becomes very cautious in predicting the future of liberation theology, given the current uncertainties of the future of Marxist socialist states, thereby suggesting to me a flaw in his analysis related to that unmeasured variable. This study is an essential addition to our understanding of the current involvement of the church in human liberation.

Corpus Christi: The Eucharist in Late Medieval Culture. By Miri Rubin. New York: Cambridge University Press, 1991. Pp. xv + 432. \$59.50.

Michael P. Carroll
University of Western Ontario

Miri Rubin tells us that this book started life as an investigation of the Eucharistic feast of *Corpus Christi*. Very quickly it became something much broader: a study of the development of Eucharistic beliefs and practices during the European Middle Ages. These beliefs and practices, Rubin suggests, constituted a symbolic system of central importance in medieval culture, and, as such, could be—and was—used to give meaning to individual lives. This symbolic system, however, did not mean the same thing to all people, and describing the various meanings that the Eucharist had for various groups is a major goal of the book.

The book proceeds by considering the development of official views on the Eucharist (chap. 1); the various texts and images used to transmit these beliefs and rituals to the local clergy and the laity (chap. 2); the origin and development of the feast of *Corpus Christi* and its associated activities, like preaching, processions, dramas, and so on (chaps. 4 and 5); and the views of the Eucharist held by different groups (chap. 6).

Early in the introduction (p. 2), Rubin criticizes a well-known article on the Mass by John Bossy, on the grounds that Bossy's analysis is an ideal-type formulation that does not take into account the way in which

the Mass was actually experienced by the laity. We are told that *this* work is meant to be a different sort of exercise, one in which the concern is to tease out from available sources an ethnographic account of Eucharistic belief and ritual that is both local and personal. Finally, Rubin talks of the need to contextualize any interpretation of Eucharistic meanings, recognizing that Eucharistic discourse might be used for quite different purposes by kings, by women in convents, by city patricians, by rural villagers rising in rebellion, and so on.

The emphasis on *not* seeing popular religion through the lens of official dogma, on studying religious experience as it is lived at the local level, and on contextualizing the study of religion—all these are the defining elements of a new paradigm in the study of popular religion that has emerged over the past 25 years mainly through the efforts of scholars like Carlo Ginzburg, Natalie Davis, Gabriele De Rosa, and a range of others. In enunciating these emphases, Rubin is clearly suggesting that this book should be considered a part of this new tradition. Unfortunately, it is not, and the reason for this, I think, is that Rubin is a scholar who endorses the logic of this new paradigm but who is still trapped within the limitations of intellectual history.

The difficulties are evident in chapter 1. The first part of the chapter is a detailed account of the theological debates over the Eucharist from the 9th to the 15th centuries. The discussion here is well organized and informative. When Rubin turns to the study of ritual practice and behavior, however, it is neither organized nor informative. In most cases, the discussion consists of observations and anecdotes drawn from many different areas of Europe *and* from many different historical periods. The implication is that the same things were happening in quite different countries at quite different times and so the author only needs to present snippets of information in order to illustrate this homogenous situation. But this approach ignores the very thing identified as important in the introduction, namely, the need to *contextualize* statements about religious belief and ritual by relating these things to particular local environments.

Similarly, the author's programmatic statements notwithstanding, this is not really a study in the "experience" of the Eucharist by the laity. Chapter 2, for instance, is entitled "Beyond Design: Teaching and *Reception* [my emphasis] of the Eucharist." In fact, although Rubin pulls together an impressive amount of information that does illustrate how Church officials sought to shape Eucharistic beliefs, there is little if anything about how these beliefs were received and held by the laity. Chapter 6 does discuss, in a purely descriptive way, the different meanings that the Eucharist might have had for different groups, but the focus here is on Church art and the written records left by mystics and heretics. It seems doubtful that such sources can tell us very much about the religion experienced by the vast majority of the laity.

Much scholarly labor was expended in locating and organizing the primary materials on which so much of Rubin's discussion is based. There

is no question but that this book would be a valuable resource for anyone concerned specifically with Eucharistic cults and devotions in Europe. Nevertheless, and despite the promises made in the introduction, it is for the most part a study of how the Eucharist was regarded by various social and religious elites. Whether the Eucharist had the same meaning for the vast majority of the population in different areas of Europe during the different centuries under consideration is unclear, and anyone interested in this question will have to find the answer elsewhere.

Gendered Spaces. By Daphne Spain. Chapel Hill: University of North Carolina Press, 1992. Pp. xviii + 294.

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We typically overlook the importance of spatial arrangements. The way we arrange space becomes taken for granted, invisible, and seemingly natural. But spatial arrangements, within which our daily lives are created, both embody and generate the rules and consequences of social life. Daphne Spain does us a significant service, therefore, when she endeavors to make visible the relation of space to gender, knowledge, status, and power.

Daphne Spain examines spatial institutions and their association with gender stratification across time and across cultures, using a wide variety of examples. She argues that, historically and cross-culturally, women and men have been spatially segregated, that spatial segregation has limited women's access to knowledge that men have used to produce and reproduce power, and that spatial segregation thereby reinforces women's lower status. To measure relative status, Spain chooses three variables: control of labor, control of property, and public/political participation. She utilizes all three variables in historical and cross-cultural versions, using, among other data sources, ethnographic accounts and the Human Relations Area Files.

Spain focuses primarily on three "spatial institutions": the family, education, and the labor force. She begins with nonindustrial societies, looking first at family dwellings. Women and men, she notes, can be separated from one another in dwellings as small as one-room huts. Examining examples of societies with segregated dwellings (e.g., Islamic *Purdah* societies), integrated dwellings (e.g., Balinese houseyards), and exceptions (Navajo anomalies), she finds the predicted pattern. The greater the segregation of the sexes, the lower the status of women. Spain next examines education in a nonindustrial version, that of men's ceremonial huts where "knowledge important to public status is passed among men and from one generation to the next" (p. 67). And indeed, those societies with ceremonial huts, she finds, are associated with a lower status for women, as are those where the division of labor is more spa-

tially segregated (e.g., Eskimos of North America) than integrated (e.g., the Bari of South America).

The focus shifts to spatial institutions in the United States, beginning with domestic architecture—from gentlemen's country homes to southern plantations to bungalows to contemporary designs. She finds historically a gradual reduction of gendered spaces in American homes, correlated with women's changing status in the United States. New home designs typically include, for example, a multipurpose family room/kitchen combination, integrating spaces where women and men were previously separated. Education, too, has historically been divided into gendered spaces; Spain reviews the Dame schools, Masters' schools, academies and seminaries, and women's colleges. The move to coeducation again paralleled elevations in women's status, although gender segregation remains, in classrooms and in choices of discipline. Finally, there is workplace segregation: women have been more likely to work in household production/domestic service while men labor outside the home, with corresponding differences in relative status.

Unlike American homes and educational settings, however, gendered spaces in contemporary workplaces have not changed remarkably over the past century. Although occupational segregation is declining, 31% of all women in the labor force can still be found in only three occupations: teacher, nurse, and secretary. Also it is important that women more often work in "open-floor" jobs—subject to greater scrutiny and therefore greater control—while men more often work behind closed doors; higher status is linked to the greater control of one's own space. And "when women and men do not share the same workplace," Spain argues, "women do not receive information that can be translated into higher status—in the form of higher wages, for example" (p. 227).

Spain believes that spatial integration—degendering space—is necessary for gender equality. "Space is organized in ways that reproduce gender differences in power and privilege. Status is embedded in spatial arrangements, so that changing space potentially changes the status hierarchy and changing status potentially changes spatial institutions. . . . Gendered spaces provide the concrete, everyday-life grounding for the production, reproduction, and transformation of status differences" (p. 233). She acknowledges that race and class add layers of complexity and need further examination, as do additional cultural institutions such as religion and the military.

Although a self-conscious feminist, Spain devotes only a few sentences to the recent work by feminist scholars, particularly anthropologists, who have been rethinking the understanding of status so that we might not privilege typically masculine activities by their very definition. Spain's form of analysis, then, is quite traditional. Some conceptual ambiguities between causal and correlational arguments exist; the breadth of focus means that the insights gained from the nuances of space have to be glossed over; the interdisciplinary nature of the work means that the book may please no discipline thoroughly. But the interdisciplinary effort

Media Events: The Live Broadcasting of History. By Daniel Dayan and Elihu Katz. Cambridge, Mass.: Harvard University Press, 1992. Pp. xi + 306. \$29.95.

David I. Kertzer
Brown University

Social scientists interested in contemporary politics have been remarkably slow to pay serious attention to the impact of television on the political process. We are generally long on lamentations and short on theoretically informed analyses. In *Media Events*, Daniel Dayan and Elihu Katz attempt to rectify this failure—produced, I suspect, partly by intellectuals' disdain for television and partly by the suspicion in which media (or communication) studies are held among practitioners of the more established social-science disciplines.

Media Events focuses on the political impact of the live broadcasting of preplanned historic events, from Sadat's journey to Israel and Pope John Paul II's visit to Poland to John Kennedy's funeral and the wedding of Prince Charles and Lady Diana. As the authors note, such events have attracted the largest audiences of any kind in the history of the world. In their analysis, they weave together Weberian, Durkheimian, and anthropological theory, as well as the literature on the sociology of communication.

In the authors' view, television has transformed political participation in fundamental ways. They focus not only on the passive role that television fosters for the citizenry, but on a host of other effects as well. Media events have become central in political integration, they argue, our new "high holidays of mass communication" transporting us to "the sacred center of the society" (pp. 1, 8). The reverent tone of the broadcasters and the sense of communion created by the simultaneous viewing of the event by tens of millions of citizens are "reminiscent of holy days" (p. 16). Here Durkheim's views are evident, along with a dose of anthropologist Victor Turner, whose notion of "communitas" is employed by the authors.

Given the current upsurge of scholarly interest in the production of collective memory, *Media Events* is of special relevance, for so much of today's collective memories, from the American moon landing to the funeral for John Kennedy, are in fact produced by television.

A major organizing principle of the argument is a typology of media events that divides them into three categories: (1) contests involving epic struggles in either politics or sport (Olympics), (2) conquests, viewed as charismatic missions, and (3) coronations, rites of passage of larger than life political figures. Here the authors acknowledge their debt to Max Weber's three types of authority: rationality, charisma, and tradition. As usual, while such a typological approach is useful in pointing out common features, it becomes rather artificial in pigeonholing events whose complexity tends to be downplayed.

Dayan and Katz see media events as having a major effect on political life, not only in creating a sense of solidarity, but also in fostering certain views of political reality. First of all, they tend to personalize politics, in the sense of emphasizing the heroic nature of certain political leaders and downplaying the complex forces that affect the political process. The authors cite critics' views that such dramatizations simply serve to distract the citizenry from the world's real problems, and they agree that television depoliticizes society, both by keeping people at home and by contributing to "the false illusion of political involvement" (p. 59). However, they do not take a completely negative view of the role television plays, arguing that, in free societies, media personnel are independent of political officeholders, and are thus free to undermine elite attempts to manipulate the public. I find this claim questionable, and even the authors note that media events that criticize the political establishment are rare at best.

The authors do express concern however, about more subtle effects of media events, including the undermining of political intermediaries in relating the mass of citizens to the political elite. For example, they claim that Pope John Paul II's use of media events as he travels around the world has the effect of robbing local bishops of their splendor, allowing the faithful to interact with the Church's highest leader directly. More generally, they see this "disintermediation" as a negative development, weakening "representative and grass-roots institutions" (p. 216).

One obstacle to Dayan and Katz's approach in judging the political impact of the live broadcasting of political events is their restriction of media events to those that are staged, that is, that have official organizers and a timetable. One has only to think of the impact of the broadcasting of the recent Los Angeles riots (following the Rodney King police brutality trial) to recognize that television can have an impact on people's political beliefs quite different from the sort seen in royal weddings and state funerals. A broader view of media events, then, might have led to rather different conclusions. But, despite such limits, *Media Events* represents an important stimulus to future research, a challenge to social scientists to bring the study of television into the mainstream of political analysis.

Sport in Australian Drama. By Richard Fotheringham. New York: Cambridge University Press, 1992. Pp. 268. \$59.95.

Wiley Lee Umphlett
University of West Florida

Today we tend to view sport and drama as separate, unrelated entities, actually as two opposing forms of leisure activity. Richard Fotheringham's book on sport's impact on Australian drama reveals how this relatively recent dichotomy came about. According to Fotheringham,

"the historical record suggests not that sport and theatre were different entities with surprising similarities and interpenetrations, but a single concept of leisure behaviour which gradually bifurcated into separate but nevertheless linked and associated forms" (p. 5). After tracing the social and economic interrelationship of sport and drama in Australia from 1768 to 1930, Fotheringham explains how a growing social consciousness in recent years incited many contemporary playwrights to criticize society by constructing plays around the confrontational aspects of sport and drama. But the earlier times of the sporting tradition in Australian drama reveal that for commercial theatre to survive it was "essential . . . to cultivate its connections with sport" (p. 27).

The tradition of the sporting narrative in Australian drama dates from the production of Dion Boucicault's *Flying Scud* in London in 1866. Among the most popular of 19th-century plays, this prototypical horse-racing melodrama "codified many of the images and incidents which are still central to the paradigm from which sporting narratives are constructed" (p. 61). As the first successful attempt to combine the spectacle of sport with a dramatic story, *Flying Scud* would be imitated dozens of times over the next 50 years. Less than six months after its opening, the play was performed in Melbourne where it was a huge success. This play's effect on Australian society can be measured in terms of its audience's identification with the basic ingredients of the story line, which revolve around the villainous machinations to prevent the hero and his horse, Flying Scud, from winning the Derby. In the play's pervasive ideology, audiences could recognize "the need for a simpler, more comprehensible social world; one in which the forces for social unity triumph over those creating divisions" (p. 85). In dramatizing how sport and its inherent ethical sense could provide this kind of utopian world, Boucicault had "stumbled on to a symbol of society which audiences embraced with a passion" (p. 85).

The proliferation of the sporting story in popular fiction and drama during the second half of the 19th century resulted from the growth of mass leisure activities during that time and the development of a knowledgable "social sub-group," that is, a sporting community. Thus sport became not only an important part of social life in Australia, it was actually symbolic of it. By this time, too, the sporting arena, particularly the racetrack, was looked upon as "an allegorical site of the social struggle of good *versus* evil" (p. 16). In sporting drama this opposition was played out against a backdrop that celebrated the salubrious attractions of country life in contrast to the seductive features of urban life, an inheritance from the English idealization of the pastoral scene and its receptiveness to sporting activity.

The sport of horse racing had been transplanted from England (where the joint exploitation of sport and drama had begun) to Australia during the early years of the country's settlement. In the mother country, travelling companies of actors had arranged their schedules to coincide with carnival holidays and popular sporting events in the provinces, such as

horse-racing weekends. Similarly, professional theatre in Australia found it financially feasible to promote its productions around a sports and drama calendar. This practice extended from the earliest times to the Depression years of the 1930s when the economic link between sports and drama was severed and never reconnected. The fact that horse racing was the most popular sport in early sporting drama was due in large part to its "ability to subsume different interests and different groups into a complex, heterogeneous but harmonious image of society" (p. 19), the very thing that contributed to the success of *Flying Scud*.

By the start of the 20th century, Australia's nationalistic fervor was enhanced by international success in sports, as the exploits of champion athletes in boxing, swimming, and team sports turned them into overnight celebrities. By contrast, the professional actor had an uphill struggle to gain favor in the eyes of society, and to accomplish this many actors associated themselves with sport. The stage also capitalized on the popularity of award ceremonies following the outcome of a notable sporting event. As a community forum, the theater instinctively realized its ability to accommodate the promotion of athletic achievement. The next step saw sports champions, particularly boxers, come on stage as actors. During the period of 1890 to 1910, many of them became more popular than the professional actors themselves. With the advent of narrative film, sports heroes were transformed into screen heroes in an entertainment form that still carries on the popular tradition of the sporting drama, "although further paradigmatic shifts have occurred to accommodate changing social mythologies" (p. 121).

Recent Australian literary drama has sought to break its ties with British theater and use sport as a localizing vehicle to satirize social attitudes toward such issues as gender and class divisions. In David Williamson's *The Club* (1978), for example, institutionalized sport is considered as one of the "dividing forces within Australian society" (p. 205), especially because of its effects on the relationship between the sexes.

Sport in Australian Drama is a welcome addition to the growing list of books that view sport as a reliable source for understanding the complex problems and changing attitudes of society. Through his strikingly original approach, Fotheringham has presented a highly informative and insightful analysis of Australian leisure culture.

The Politics of Medical Encounters: How Patients and Doctors Deal with Social Problems. By Howard Waitzkin. New Haven, Conn.: Yale University Press, 1991. Pp. xvi + 311. \$32.50.

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Indiana University

This readable, interesting book raises many intriguing issues concerning doctor-patient interaction. More than anyone else, Howard Waitzkin has

argued trenchantly that many medical problems that people experience are social rather than biological in origin, and that, therefore, social change rather than pharmacological therapy is needed. This book represents both old and new concerns of its author. That is, a number of chapters have been published in article form over the years, but this monograph is more integrated and comprehensive than any of the other collections. I shall first give some background for the book, then summarize its main arguments, and last, raise my major concern.

Since the early 1970s, Waitzkin has conducted an exceptional amount of both quantitative and qualitative research on interaction in medical settings. The perspective (as developed, for instance, in "Medicine, Superstructure and Micropolitics," *Social Science & Medicine* [13A (1979): 601-9] was that the doctor-patient relationship is a micropolitical sphere in which physicians help maintain patterns of domination and subordination characteristic of advanced capitalist societies. They do so by controlling information and discourse, using the Western scientific "medical model" to invalidate patients' "ethnomedical" explanations of problems and, relatedly, to avoid using understandable lay terms in favor of jargon and technical language, which mystify the processes and experiences in which a patient is embedded. Since they certify their clients' entry into the "sick role," physicians are persuasive in keeping members of the working class in their social structural position. This they do by stressing individual responsibility (even when the cause of a disorder, such as unemployment or alienation, is arguably social), and offering treatments that return the patient to the workplace as soon as possible while deflecting criticism from current social institutions. Thus, physicians exert both social and ideological control on behalf of the larger society or, at least, of its hegemonic classes.

The author's early work was Marxist in orientation, with sprinklings of other theorists such as Althusser, Habermas, and Braverman. In *The Politics of Medical Encounters*, Waitzkin's basic message has not changed. What has changed are two things: the overall frame in which the author conveys his message and the way in which he analyzes transcripts of doctor-patient conversations.

The frame in which Waitzkin's message now appears derives less from Marxist theory and more from poststructuralist literary criticism. Of particular importance to Waitzkin's eclecticism is Foucault, along with liberal dosages of Terry Eagleton and Frederic Jameson, all of whose primary contributions to the message have resulted in a deepened appreciation of what Waitzkin calls the "unintentionality" of professional social control. Since control operates at the edges and margins of discourse, it is necessary to analyze not only what participants to conversation say but also what they *do not* say. That is, Waitzkin proposes that the analyst must explicate certain kinds of absences in doctor-patient interaction, for it is in these absences and their relation to spoken text that both doctors and their patients become subject to the "grip" of ideology and the consent that it engenders (e.g., pp. 40-41).

Besides altering the frame of his message, Waitzkin has become more systematic in the explication of his own "critical theory" of medical discourse and in his methods of analysis. The theory is that there are underlying structures of medical discourse consisting of six "islands." In brief terms, Waitzkin argues (*a*) that society presents many difficult conditions for ordinary life. As a consequence, (*b*) patients experience troubles, albeit privately, and (*c*) seek help from medical professionals, who (*d*) attempt to render patients' troubles in the vocabulary of medicine. Usually the attempt is successful, but there are times when social issues from a patient's everyday life erupt into the discourse, creating (*e*) "countertextual" tensions, which are suppressed through physicians' gestures of dominance, such as interruptions and patterns of emphasis and deemphasis. Then, (*f*) doctors revert to familiar technical solutions and counsel patients to return to their workaday roles, which leaves the society and its overall organization intact.

In a chapter "on method," the author proposes, among other criteria for analyzing medical discourse, that a theory such as this can operate as a procedure for interpreting carefully transcribed conversation. Indeed, in the rest of the book, Waitzkin examines a number of doctor-patient encounters and shows how these encounters are structured according to the six islands of medical discourse. While also exhibiting a sensitivity to negative cases, he documents a large number of social problems in chapters on men, work, and the family; women, work, and the family; aging; "self-destructive" behaviors, such as drugs and sex; and emotional difficulties. According to Waitzkin's analysis, doctors push the social contexts and processes that produce these problems (and their manifestations in patients' symptomology) to the margins of medical discourse. As in his earlier work, Waitzkin stresses that this results in the reproduction of ideology and control.

My concern with this book, naturally enough, reflects a particular analytic bias. In his chapter on critical theory, Waitzkin proposes that his approach "addresses weakness in the fields of sociolinguistics, conversation analysis, and discourse analysis, by emphasizing elements of social context and how they come to be excluded or marginalized." In fact, the book does not address their "weaknesses" so much as it simply leaves these fields behind. Consequently, many insights that these various perspectives would offer in regard to doctor-patient episodes are ignored, and this results in a considerable impoverishment of the analysis. There is much more going on interactionally in doctor-patient discourse besides the suppression of social context, and Waitzkin often misses activities that are demonstrably important to the participants, even if they are not so in regard to his theory. If space permitted, it would be worth responding in print to an invitation implicit in Waitzkin's chapter on method. He recommends that transcribed excerpts should be published along with authorial interpretations, so that a "reader can evaluate the interpretation" (p. 63). Many episodes beg for analyses that are alternative or at least complementary to Waitzkin's own.

My hope is that in future work, this author might incorporate sociolinguistic, discourse, and conversation analytic studies that would add to his own research. As one example, I find proposals about what is "absent" in medical dialogue particularly troublesome, since there is no rubric in this book for adjudicating what is significantly absent from among the myriad of possible absences. This is a problem that conversation analysis, in particular, has addressed. As another example, when Waitzkin (p. 273) recommends that doctors be less interruptive of patients, his discussion could have gleaned insights from H. B. Beckman and R. F. Frankel ("The Effect of Provider Behavior on the Collection of Data," *Annals of Internal Medicine* [101 (1984):692-96] and C. West (*Routine Complications: Troubles with Talk Between Doctors and Patients* [Bloomington: Indiana University Press, 1984]) regarding how such interruptions regularly occur and what might be done about them.

My biases and reservations aside, there is much to recommend this book. From someone who has been both a practitioner of medicine and a serious student of doctor-patient interaction for over 20 years, it is an excellent compendium—a coherent, unified, and systematic statement of theoretical, methodological, and empirical concerns he has articulated in otherwise disparate pieces of work. I found the last chapter on "Changing Discourse, Changing Medicine," which provides comparative accounts of doctor-patient interaction in other countries, to be particularly helpful because (in the Cuban case) it provides for what a health care system that moves "contextual issues from the margin to the center" (p. 272) might look like.

Inheriting Madness: Professionalization and Psychiatric Knowledge in Nineteenth-Century France. By Ian Dowbiggin. Berkeley and Los Angeles: University of California Press, 1991. Pp. x + 217. \$34.95.

Barry Glassner

University of Southern California

Is there a structure to history, or even to any temporal and geographical segment of history? Do people and groups make history through their intentions and interactions, or does history unfold haphazardly? These questions, which beg many others, are not new, and as *Inheriting Madness* illustrates, they remain unresolved.

In accounting for why French psychiatrists took one approach rather than another to the interpretation and treatment of mental illness at various times between 1835 and 1900, Ian Dowbiggin emphasizes first one of these three opposing questions and then another. He lists every factor he can turn up that might help to explain why psychiatrists acted and thought as they did at a given time. He writes of competition and overlap between institutions such as the church, the press, and the arts;

intrainstitutional interactions between physicians; and unexpected changes in political ideologies and cultural climates in France.

Conventional history written by professional historians is largely about unearthing documents other historians have not already targeted and arranging evidence from these documents in a chronological order to imply a connected chain of events. *Social* history, which Dowbiggin means to write, differs from the conventional approach by, at least, the author's use of sociological ordering principles of one sort or another. In social history, evidence is arranged to reveal social structure, rather than on the basis of a chronology of events.

But whenever Dowbiggin commits himself to a model or theoretical perspective regarding society, or even to any of the factors he cites as important, he does so indecisively. He says of nearly every factor he mentions that it "may" or "likely" had an effect, or served to enhance, accelerate, or slow up something already happening—not that it was causally or structurally significant.

To be sure, at times Dowbiggin sounds like he has a theoretical perspective, a Foucauldian one: "The isolation of the insane in asylums also enhanced the physician's moral authority by entrusting him with the surveillance of their conduct and thinking" (p. 131). But he overtly dismisses Foucault elsewhere: "there is a seamless quality to Foucault's model that, like many sociological models, fits historical reality poorly" (p. 170).

Ultimately, what the book offers sociologists who are interested in the history of psychiatry is historical data on how French psychiatrists adopted particular beliefs and practices in order to survive professionally and financially and how this created points of economic connection as well as competition across social institutions during the period under study. Particularly interesting is Dowbiggin's analysis of how psychiatrists contributed to discourses about the nature of democracy and the decline of French culture in the postrevolution period. He points out that notions of hereditary weakness served to explain why some people were predisposed to behavior at odds with the state or the elite.

The data presented in *Inheriting Madness* can also be read as further evidence that there is a generative disparity within the discourse on mental illness—namely, the moral versus the medical. These positions are at odds not only over the ontology of mental illness, but more sociologically, over where to put people who are deranged, and over the proper role of those who treat the deranged. Moralists propose that people deemed mentally ill be put in luxurious environments. Dowbiggin quotes a psychiatrist in the 1840s suggesting as an antidote to mental illness, "the life-style that ordinarily accords the best with the health of the intelligence: a retirement home, quiet distractions, pleasant travels, a select and infrequent social life, books that are easy to read; in a word all the conditions of life a calm and healthy-minded person would take pleasure in" (p. 50).

In that view, those who administer to the deranged are little different

from the staff of a spa. The opposing view is advocated by those who would medicalize, who join a psychiatrist in 1855 who looked forward to "the day, finally, when medicine itself could say: 'Alienation is a disease like all the others'" (p. 71). From this perspective, the deranged should go to hospitals and be treated by doctors.

Obviously, that difference of opinion has continued in various guises throughout the 20th century in debates over psychoanalysis, institutionalization, and psychotropic medications. A question that continues to cry out for a theorized, empirically grounded answer from social historians is—Under which conditions do moral rather than medical views come to prominence?

Melancholy and Society. By Wolf Lepenies. Translated by Jeremy Gaines and Doris Jones. Cambridge, Mass.: Harvard University Press, 1992. Pp. xvi + 253. \$39.95.

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This is a wonderful and, at the same time, a frustrating book. Each chapter is a gem of erudition and elegance, moving easily through centuries of history, and through a variety of intellectual disciplines. The writings of La Rochefoucauld, Kant, Proust, Gehlen, Adorno, and a host of others are woven together with analyses of the cultures of the French absolutist court and salon, the German bourgeoisie, utopian fantasies, and many other groups and ideas. Virtually anyone interested in the sociology and history of culture will find insight after insight in these chapters.

Nonetheless, as a whole, the book has an idiosyncratic feeling. This begins when the author tries to set up the general theoretical structure of the book by juxtaposing some of Merton's writings on anomie with Robert Burton's 1621 work, *Anatomy of Melancholy*. It is rare to see relatively unknown, early 17th-century works used in this way, and this is a tribute to the fascinating way in which the author mixes sources in the quest for new insights. The problem with Burton's book is that it flares off in a number of directions and does not provide a solid core of concepts and definitions that might be used in looking at melancholy over the centuries in a number of different historical contexts. Lepenies's book has the same problem. There is clearly supposed to be some systematic relationship between melancholy, boredom, and powerlessness, but I was never able to grasp exactly what that relationship is. This problem is compounded by the tendency of the author to make some extraordinary assertions that would seem to me to be crucial to his theoretical task, but which remain largely undeveloped. For example, he claims "solitude is *the topic of our age*" (p. 62, italics in the original). I am not sure that many social scientists would agree with this claim, but if it is made, it

clearly should be developed in detail as part of the theoretical structure of melancholy. Instead, it largely disappears a few pages later as the analysis moves elsewhere.

Even though the theoretical structure of this book is underdeveloped, it is an irresistible but melancholy read that should not be missed.

Pain as Human Experience: An Anthropological Perspective. Edited by Mary-Jo DelVecchio Good, Paul E. Brodwin, Byron J. Good, and Arthur Kleinman. Berkeley and Los Angeles: University of California Press, 1992. Pp. vii + 214. \$35.00.

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Few topics stretch the limits of social scientific understanding as does physical pain. The authors of *Pain as Human Experience* handle the challenge with distinction. They are former participants in the Harvard Program in Medical Anthropology's "Friday Morning Session" (p. 15) sponsored by NIMH and have conducted various NIMH or NSF supported pain research projects. The editors (four of the six authors are editors) bring these studies together in one place to address the "*experiences of chronic pain*, in the context of American culture and society" (p. 14). The result is a sophisticated and engaging book that takes us into subtle phenomenal worlds that ordinarily escape the attention of social scientists.

The organizing perspective of the volume is medical anthropology. All of the authors have done previous fieldwork outside of North America (including work in China, Iran, Turkey, and various Latin American and South American countries), and much of their work has been in the area of pain and pain experience. There are intermittent references to this previous work for cross-cultural comparisons throughout, but the main body of the book addresses chronic pain in America. In the introductory chapter, the editors compellingly spell out what anthropology has to offer this field that is otherwise dominated by medical and psychiatric models. They make it clear that anthropologists do not simply shed new light on old phenomena, adding to whatever was already known about them, but that they reveal entirely new phenomena—cultural phenomena. And the very method of revealing these phenomena disrupts the presuppositions and findings contained in medical and psychiatric literature.

The book works best where the authors pay explicit attention to the subtleties of their subject matter (e.g., the editors' introductory chapter and the epilogue), including the practical anomalies sufferers unwillingly face every day and the theoretical paradoxes encountered by researchers and theorists attempting to describe them. The core of the problem is the very *identity* of pain as a thing in the world. It is an objective fact for

sufferers even as the larger world of which it is a part does not contain it—within the larger objective world, pain is radically subjective. For sufferers pain is a fact emanating from within their own embodied subjectivities, an object that will not be objectified. Byron J. Good (chap. 2) captures this in a case study, arguing that pain shatters the subject/object dualism endemic to Western culture; he draws heavily from phenomenology to show how pain disrupts the essential foundations of experience—pain “unmakes” the world, he argues convincingly. Linda C. Garro (chap. 5) likewise pays uncompromising attention to these theoretical ironies in her detailed case histories of two pain sufferers told from their perspectives. Garro’s pain narratives capture the “ontological assault” (p. 103) of chronic pain, but they are also products of the assault. Their details are constant objects of revision within the narratives themselves; there are no socially sanctioned methods for sufferers to judge the accuracies of their stories.

Similar paradoxes are at work in Jean E. Jackson’s wonderful ethnography of a chronic pain clinic (chap. 6). Treatment programs, which are largely psychologically oriented, involve ontological struggle between staff and patients regarding the “real” nature of the problem. Paradoxes work themselves into these programs in staff messages to patients—for example, pain is real if one feels it but it will retreat if one denies it legitimacy. Treatment success is often judged by the degree to which patients are persuaded to adopt new definitions of pain reality, although they seldom adopt the staff’s line uncritically.

Paul E. Brodwin (chap. 4) also offers a case study to make the point that pain has some features of language, namely that it is a *medium* of meaning and communication. Thus its meaning does not derive from somewhere else, and it may even express messages inexpressible in words (p. 94). There are some problems with this thesis similar to those involved in such ideas as “body language” and “music as a language,” and some of these problems are highlighted elsewhere in the book where pain is shown to be obdurately resistant to linguistic objectification. Could it then objectify itself? However, Brodwin’s point, drawn in part from the psychological notion of “secondary gain” with the added component that sufferers *know* what they are doing, could be that pain shares with language the following: its meaning for sufferers turns on how they use it and on what they can do with it. Arthur Kleinman (chap. 7) draws upon a similar notion, showing how pain can be a resource for sufferers to express other kinds of social suffering, including political and economic suffering.

Less sophisticated, but interesting in their own right, are Mary-Jo DelVecchio Good’s two case studies (chap. 3) that show how work can ameliorate pain suffering. I say less sophisticated because this chapter is almost bereft of the theoretical insights running through the other chapters. I say interesting because it counters some conventional wisdom that work exacerbates chronic pain. Good is cautious, however, about generalizing her findings.

All in all, this is a major contribution to an important topic. Some readers may be put off by the reliance on case-study material, but the cases are masterfully handled and integrated into general discussions, and readers will come to know the sufferers' lives—their interfaces with the wide social world, their ruminations on cause and effect, their journeys through medical labyrinths—in tragic detail. The authors vary with regard to the importance they attach to psychological test results and DSM diagnoses, but these notions never take center stage. This book will be highly appealing to anyone interested in pain, lay and professional, across the wide range of health professions, as well as to sociological theorists and phenomenologically oriented sociologists.

Into the Valley: Death and the Socialization of Medical Students. By Frederic W. Hafferty. New Haven, Conn.: Yale University Press, 1991. Pp. xix + 234. \$28.50.

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University of Pennsylvania

Within the sociology of medicine, there is a rich tradition of ethnographic research. Frederic Hafferty's commendable study, *Into the Valley: Death and the Socialization of Medical Students*, continues and extends this tradition in two nonobvious ways. First, it brings together two subtopics usually kept separate—death and dying and the socialization of medical students—so that the resulting work is not just a contribution to medical sociology but to the sociology of emotions as well. Second, the volume provides a good case study through Hafferty's discussion of whether qualitative research is cumulative, and of how a line of research is built upon, extended, and refined. Both contributions widen the appeal of the book beyond the rather narrow specialization that the subtitle suggests.

The empirical focus of this book is tight and sharp. Hafferty follows a group of first-year medical students through three separate exposures to death and dying: a visit to a hospital for the terminally ill, the dissection of cadavers in anatomy lab, and the unexpected, untimely demise of an elderly man who was a volunteer for a class on interviewing skills. Hafferty concentrates on the earliest experiences of first-year students, assuming that it is these experiences that have the greatest impact on students' emotional development. Hafferty is adept at displaying this impact through a dissection of student coping styles. This catalog of coping strategies leads naturally to questions of specialty choice and practice style but these questions are for another day.

Hafferty first analyzes the visit of a group of students to a hospital for the terminally ill. This visit occurs before the students' exposure to the anatomy lab. Hafferty sees this field trip as mediating the students' transition from lay to professional culture. He uses the trip to announce some of the major themes of the book: the students' limited ability to

communicate their emotional experience, the short "half-life" of that experience, and the perceived conflict between scientific and humanitarian norms in medical practice.

The largest portion of the book is devoted to the students' anticipation and experience of the anatomy lab. Hafferty begins with a discussion of "cadaver stories . . . apocryphal narratives that describe medical student's pranks involving cadavers or cadaver parts. Although these stories are quite graphic, manifestly gross, and often sexist in content, most medical students find them hilarious" (p. 55). Hafferty provides both a large collection and a straightforward functional analysis of cadaver stories. The clarity of Hafferty's exposition provides an ironic contrast to the twisted subterfuge of the story/joke form students employ to communicate emotional and normative uncertainty.

When Hafferty enters the anatomy lab, he is revisiting familiar terrain empirically and theoretically. Nonetheless, the discussion is fascinating. Hafferty documents the existential questions the cadaver provokes in students. Who was this person? What kind of life brought them here? Would I allow myself to be carved up this way? Hafferty notes a gendered difference in coping with the anxieties of the anatomy lab, with women more able to manage than men. He also discovers that habitual absenteeism is correlated with an inability to cope with a cadaver. Finally, Hafferty links previous discussions of uncertainty to the idea of sociological ambivalence, thereby deepening both concepts.

The third exposure to death that Hafferty analyzes involves the unanticipated death of an elderly man during a class on interviewing skills. Like the shooting that Sudnow witnessed in an emergency room while gathering data for his study on the social organization of dying, the untimely demise of patient Kilwauski reveals the fragility of the organizational and professional routines for normalizing death. Mr. Kilwauski was a ready volunteer for a course on interviewing. During his interview, Mr. Kilwauski began to cough. The physicians running the interview neither fetched him water nor made inquiries about how he was feeling. Rather, they plunged ahead with their task at hand: showing proper interviewing style. Eventually Mr. Kilwauski's retching made this impossible. He staggered first to a corner of the room and then was assisted outside of it. In the corridor, he collapsed and died. Hafferty describes this, the institution's stumbling response, and the students' attempt to make sense of Mr. Kilwauski's death. Hafferty is particularly incisive in showing how this event isolated the few students who saw it from their peers and generally reinforced a set of "stoic" feeling rules.

This is a good book, finely crafted. It builds upon and extends a tradition of research. Rather than get bogged down in old debates—Is it anticipatory socialization or situational adjustment?—Hafferty forges some judicious and innovative syntheses. But there is more as well. Every ethnography gives a sense of the ethnographer. Reading this work made me want to meet Frederic Hafferty. This is a brave work. Death and dying are not easy to observe firsthand. It could not have been easy

to get up each morning, go to the field, write up the notes, and sustain emotional equipoise. This is not only a brave work, it is a fair and balanced one that respects the life world of the medical student subjects as well as the prior work of others on cognate topics. This is also a modest work (in the sense that Hafferty does not waste time with self-congratulatory prose) but one that makes a large contribution, nonetheless. It will be profitably read by students of field methods, sociology of emotions, and sociology of medicine.

Overcoming the Odds: High Risk Children from Birth to Adulthood. By Emmy E. Werner and Ruth S. Smith. Ithaca, N.Y.: Cornell University Press, 1992. Pp. xiv + 280. \$42.50 (cloth); \$15.95 (paper).

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The objective of the research presented in *Overcoming the Odds: High Risk Children from Birth to Adulthood* is to "trace the long-term effects of childhood adversity on the adult lives of men and women who were exposed to poverty, parental discord or psychopathology, and perinatal stress; and . . . to examine the long-term effects of protective factors and processes that led most to a successful adaptation in adulthood" (p. 3). Thus, the authors attempt to shed light on two areas often ignored in the traditional literature on "deviant" life events, (1) the existence of "protective" factors in the lives of high-risk children and adolescents and (2) the possible effects of early risk and protective factors on the lives of individuals as they move through the life course into adulthood. Emmy Werner and Ruth Smith examine these issues by using data from the "Kauai Longitudinal Study" (KLS)—a data set containing information collected over a 32-year period from a sample of men and women born on the Hawaiian island in 1955. In fact, this is the fourth in a series of books by the authors based on data from this cohort. Nonetheless, despite the importance of the research questions and the potential richness of a longitudinal data set, there are fundamental methodological flaws in this study that limit the authors' ability to adequately address the questions they propose.

The authors begin with a general description of earlier findings from the KLS and a brief review of major findings from other cohort studies. Next, summary information concerning education, work, and family histories as well as stressful life experiences (e.g., financial and relationship difficulties) are presented for the KLS sample. The lives of the "resilient" (yet "high risk") children are discussed in chapter 4, while chapters 5–7 report the possible long-term effects of teenage motherhood, juvenile delinquency, and adolescent mental health problems for the subjects as they reached 32 years of age. In chapters 8 and 9 the authors describe how childhood and adolescent experiences may increase or decrease the

likelihood of "successful adult adaptation." Finally, the authors conclude with policy implications and directions for future research.

Generally speaking, many of the findings in this book concur with those from other studies. For example, the authors report that many teenage mothers' educational and vocational achievements improve as they age, the majority of juvenile delinquents do not have criminal arrest records as adults, and most adolescents with histories of mental health problems do not have public records of psychiatric problems as adults. Additional analyses correlate risk and protective factors in youth with a major dependent variable—"quality of adult adaptation" (measuring respondents' "self-evaluations" and public records of divorce, arrest, and psychiatric disorders, pp. 258–59). While some differences are found according to gender, the authors report that adult adaptation is related to the subject's history of school problems, delinquency, mental health problems, low socioeconomic status, birth of a sibling before the subject is two years of age, and family structure.

Because the book lacks a significant discussion of variable construction, measurement and analytical issues, and data limitations, it is impossible to interpret the meaning of these patterns adequately. For instance, the "adult adaptation" measure sums many separate indicators of varying scales. The reader cannot determine details such as how missing data are handled or how particular cut-off points are chosen for this key measure. The analyses and presentation of "path" models is unclear (e.g., the coefficients and measures are not described and control variables are not apparent from the diagrams). Independent and dependent variables are sometimes confounded—an artifact the authors should have considered when they report that their analytic model correctly classified 100% of the cases (e.g., p. 165). Other instances of research carelessness can be found (e.g., the authors repeatedly refer to "SHAZAM," a statistical software package, as a "logit regression model"). Unfortunately, these (and other) shortcomings in the methodology along with the lack of clarity in the reporting of results limit the usefulness of this research.

The longitudinal study is further hampered by the fact that key issues of selection and spuriousness are not addressed. Several events mentioned as key turning points in the lives of high-risk men and women (such as going to a community college or joining the armed forces) are discussed as protective factors in and of themselves when they, of course, may indicate selection processes. "Self-esteem and efficacy" enhancement is argued in the final chapter to be "the key ingredient in any effective intervention process" (p. 205). While "self-esteem" (measured at age 18) is correlated with "adult adaptation," the possibility that this relationship may be spurious and therefore of limited use in intervention programs is not investigated. Given their recommendations, the authors should have recognized the literature that finds that self-esteem enhancement programs have not been proven effective in reducing delinquency, substance abuse, pregnancy, or school failure (e.g., Joy G. Dryfoos, *Adolescents at Risk: Prevalence and Prevention* [New York: Oxford Univer-

sity Press, 1990]). Finally, in addition to the methodological difficulties mentioned above, the restricted investigation of both structural and cultural factors makes this essentially descriptive research of limited use to most sociologists, criminologists, and demographers interested in explaining patterns in the life course.

Children in Poverty: Child Development and Public Policy. Edited by Aletha C. Huston. New York: Cambridge University Press, 1991. Pp. x + 331. \$44.50.

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This is a timely and important book. Roughly 13.4 million U.S. children today are officially poor as defined by the U.S. Office of Management and Budget. Indeed, one in five American children lives in poverty, a figure exceeding that of virtually all other Western industrial societies. The economic situation of children is even worse among racial or ethnic minorities; 45% of African-American and 38% of Hispanic-origin children are poor. Across the political spectrum, there is now consensus that these poverty rates are unacceptably high, that a growing proportion of poor children are "at-risk," and that our future depends on effective intervention strategies that both reduce poverty and minimize its detrimental developmental and social consequences.

This is a central lesson of this highly useful collection of original papers first presented in 1988 at a conference on "Children and Poverty" at the University of Kansas. It is an intelligent book that provides perhaps the most comprehensive single source available today on the subject. The chapters are organized around three central questions: (1) Why are so many children growing up today in poverty? (2) What are the effects of poverty on various developmental outcomes? and (3) How can public policies be formulated to prevent or ameliorate the debilitating effects of poverty on children? The contributing authors comprise an eclectic but distinguished group, representing the fields of sociology, economics, public health, child development, and education. In general, the contributors take a dispassionate, data-based approach to the subject; the book is not prone to hyperbole.

Each of the contributors adopts a "child-centered" approach; that is, the authors evaluate research and policy from the perspective of poor children rather than from that of their parents. Each chapter reviews state of the art studies of the developmental consequences (e.g., academic competence, adolescent childbearing, physical and mental health) of persistent child poverty, and also evaluates existing social welfare and family support programs directed at poor children. The chapter by Sara McLanahan, Nan Astone, and Nadine Marks typifies this approach. They evaluate the multiple pathways through which poverty in female-headed fam-

ilies is reproduced from generation to generation. They find that children growing up in a mother-only family have higher rates of high school dropout and premarital childbearing. These negative outcomes apparently result from single mothers' low income rather than from the lack of effective parenting skills or from living in poor neighborhoods. The authors then recommend policies designed to increase mothers' earnings (e.g., through pay equity policies and subsidized child care), private transfers (e.g., enforced child support from noncustodial fathers), and public assistance (e.g., refundable tax credits).

Other noteworthy chapters focus on the health status of poor children (Lorraine Klerman), on mother's employment, formal child care, and children's poverty experiences (Deborah Phillips), and on the effectiveness of compensatory education programs for poor children (Craig Ramey and Frances Campbell and Henry Levin). These chapters (and the others) cohere nicely around a common theme and are of uniformly high quality. The credit goes to Aletha Huston, who does a commendable job of weaving together common themes in the introductory and concluding chapters. For example, she identifies several barriers to effective intervention: individualism and family privacy (i.e., beliefs that government should not intrude into the family), public policy based on short-term economic criteria (rather than on promoting children's welfare), and conflicting notions about the appropriate roles for single mothers (i.e., should they be self-supporting or be full-time child care providers?). She then outlines a policy agenda that includes establishing programs that supplement family income, promote health care, and provide child care and education.

My only criticism of this collection is that a "child-centered" approach is less successful in identifying and evaluating policies that address the proximate causes of child poverty—and these are often lodged in the experiences of poor children's parents. For example, in an informative overview of the changing economic environment of children, Greg Duncan attributes the increase in child poverty rates partly to the rise in female-headed families. The poverty rate of families with children would have increased only one percentage point (i.e., from 11% to 12%) between 1970 and 1986 if family composition had not changed after 1970. If the growth of female-headed families is fueling the rise in child poverty rates, what can be done to "strengthen the family" or to encourage two-parent families or to discourage nonmarital childbearing or to reduce divorce rates? These are contentious questions often left unanswered by the authors, and this is unfortunate given the growing debate about welfare reform that encourages so-called responsible family behaviors (e.g., "wedfare" or AFDC payment schedules that discourage nonmarital fertility). An increasing share of children today (i.e., about 27%) are born outside of marriage, and nearly one-half will experience a parental divorce during childhood. These children face a very high probability of poverty. But a child-oriented approach alone will not solve this poverty problem—at least not in the short term.

In sum, this book is mostly about welfare and social policies designed

to end the cycle of poverty from parental to filial generations. From a policy standpoint, this is a useful orientation because high poverty rates today among children portend an uncertain economic future for America. This collection offers hope that we can find the political will to solve the problem of persistent poverty, that liberals and conservatives will work together to implement effective intervention strategies, and that the beneficiaries of this will be today's poor children who, as adults, will no longer live in poverty. This book has clearly set an agenda for policy-related research for many years to come.

Hidden Anxieties: Male Sexuality, 1900–1950. By Lesley A. Hall. Cambridge, Mass.: Polity Press, 1991. Pp. viii + 218. \$49.95 (cloth); \$21.95 (paper).

John H. Gagnon

State University of New York at Stony Brook

This is an oddly touching book. I do not believe that my reason for saying this is because I am a man reading about the pains of men, but it is, at least in part, because the individual voices that are heard are at such odds with the ideology of a triumphant hegemonic masculinity and patriarchy. They are voices of ignorant men who are deeply puzzled by the conflicts between what they have learned about masculinity and sexuality and what they desire in their affectional and sexual relations with women.

Lesley Hall has done the study of gender and sexuality a service by examining the letters sent by Englishmen to Marie Stopes in response to her writings on sex, love, marriage, birth control, and reproduction. Stopes was one of that remarkable generation of middle-class women born before the First World War who attempted to change the gendered affectional/sexual world that they received from the 19th century. Stopes was well known in international sexual reformist circles (as well as being a supporter of suffrage) during the second decade of the 20th century. In 1918 she published her first book *Married Love: A New Contribution to the Solution of Sex Difficulties*, which was the first of a series of best-selling works on similar topics that were published and republished until the 1950s. *Married Love* sold 500,000 copies by 1925 and one million by 1950—other books sold in similar numbers and were widely translated into languages other than English.

These books were focused entirely on married heterosexuality and eminently respectable in their tone and goals. Other than her emphasis on birth control for both the purposes of increased sexual pleasure and family planning, which attracted the hostility of the Roman Catholic Church, her work met with widespread approval from both her readers and from the medical profession. Her advocacy of simultaneous orgasm in coitus has often been parodied, but she also supported noncoital practices as appropriate sources of sexual pleasure for the married couple.

The enthusiasm of her readers led to a voluminous correspondence, a large proportion of which was written between 1920 and 1940, asking for further advice. It is the letters from the men who wrote on which Hall reports in this volume. Hall's analysis of the letters occupies the last three of the book's six chapters in which selections from the letters are classified into various types of "anxieties" (e.g., the lack of concordance between the erotic desires of women and men, premature ejaculation, penis size, the loss of "manhood" with aging). The first three chapters provide a framework for the letters by examining textual evidence about the ways in which sexuality (with the primary focus on men) was being reconstructed in the last decades of the 19th and the first decades of the 20th centuries.

The collection of letters written by men to Stopes represent processes of self-selection that we will never understand. What they have in common are their fears and their ignorance (the desire of many to be clean vessels unsullied by sexual thought or deed is particularly striking) and their wish to please the women that they have married or are going to marry. It is difficult to know how many such men there were, but it is possible to argue that they represent a new "sexually respectable man" that was being constructed in the late 19th century. Perhaps a measure of their numbers is the evidence from Kinsey (another nonrandom data set) that, of the college-attending men interviewed by his research group during the 1940s, a third were virgins when they married; of those men who had experienced premarital intercourse, one-quarter had done so with only one partner.

The book is not without its weaknesses. Perhaps the most important is the failure to address, even speculatively, how such sexually respectable men could have been acculturated. What had changed in the sexual learning environments of young men at the end of the 19th century that transformed the sexuality of men into its modern form? Knowing that prostitution was suppressed, that masturbation was condemned, that physicians were ignorant and gave bad advice in their books (as well as in person) is one or two steps away from the textures of everyday experience. Where is a boy's own story in the late 19th century? Hall, as a self-labeled social constructionist, tells us little about the *social* practices that created these anxious men and their anxious pleasures. Despite this criticism this is a valuable sourcebook of new voices attesting to the importance of understanding the ways in which men were gendered at the beginning of the modern era.

The contemporary triumph of respectable manhood with its anxious sexual desires is perhaps signaled by the following coincidence. As I began to write this review I received a copy of a revision of a widely popular book on the problems of male sexuality and how to treat them (Bernie Zilbergeld, *The New Male Sexuality: The Truth about Men, Sex and Pleasure* [New York: Bantam Books, 1991]). The title of the opening chapter is "The Making of Anxious Performers."

Law, Sexuality, and Society: The Enforcement of Morals in Classical Athens. By David Cohen. New York: Cambridge University Press, 1991. Pp. xii + 259. \$49.95.

Christie Davies
University of Reading

David Cohen has written an interesting and illuminating comparative social anthropological account of the enforcement [as it is called in the title] and reproduction [as he prefers to call it in the text] of morality in classical Athens. His study covers, *inter alia*, adultery, homosexuality, and impiety, using a great variety of both classical and secondary sources. His work will prove of great value to those sociologists unbigoted enough to recognize that we are all the heirs of Athenian achievement, but whose own knowledge of Greek is on a par with Shakespeare's. The strength of Cohen's analysis lies in his mastery of a wide range of sociological, social anthropological, and historical studies of traditional communities throughout the Mediterranean region including Italy, Spain, North Africa, and modern Greece. From this he has derived both a portrait of a common Mediterranean tradition and a sense of the gap between formal, "ideal" and official, moral standards and the "real" standards of everyday practice in these and other societies. He has used these insights to gain a new understanding of the varied but limited documents that are the basis of our knowledge of classical Athenian society.

Three of Cohen's more striking conclusions are worth citing. First is his statement that "the Athenian law of adultery did not aim at regulating adultery as a form of sexual misconduct, but rather as a source of public violence and disorder" (p. 124). Second is his undermining of "the accepted notion of Athenian men as only interested in courtesans, prostitutes and boys, and Athenian women as isolated, passive" and uninterested "in sexual attachments" (p. 170). Third is his finding that sexual roles in Athens "were defined in terms of a male/female dichotomy and judged by norms that were felt by some to be at once social and natural" (p. 187) so that it can no longer be asserted with any confidence that the modern dichotomy of heterosexual/homosexual or the view that homosexuality is unnatural were unknown in ancient Greece.

Cohen has modestly given the sociologists and anthropologists whom he has taken as his exemplars much of the credit for insights that are really the product of his own shrewd examination of particular sources. In consequence he is too apt to cite and rely on their views that it is better to regard moral rules as processes and resources rather than as social facts and constraints. In point of fact *either* viewpoint can be justified depending on which aspect of which society is being examined and what questions are being asked. In particular the very different questions of how and why demand quite different approaches to the available data. Also it is at least possible that some societies have a stronger sense or tradition of disinterested punitiveness than others. Thus

Cohen is unjustified in suggesting that his revelations concerning the practicalities and complexities of everyday moral interaction not just in Athens but, by implication, universally can undermine the intellectual position of those leading American or British judges who uphold or even invent laws that purport to "enforce" private morality. Black-letter lawyers may on occasion be fools but they are often very clever fools. Many of the judges who expound the law as if it were an absolute, eternal, and unambiguous ethical code have after all risen to their present eminences after lifetimes as successful attorneys massaging and manipulating the moral appearances of their clients.

It is also worth making the point that many of Cohen's most illuminating comparisons occur when he drops the slurred and ugly language of a sociology that "reproduces," "articulates," "mediates," and "ideologises" and makes direct contrasts between the laws and rules of communities separated by space or time. Cohen is right for instance to note the paucity of Athenian criminal legislation on sexual matters in contrast to other societies that have imposed the death penalty on those found guilty of forms of immoral conduct that were seen as sins against God. It would also be interesting to know why, to quote the author, "Plato made Pausanias characterize Athenian law as many-hued intricate, and difficult to understand in contrast with other Greek cities which either prohibited or permitted paederasty in a straightforward way" (p. 180). David Cohen has subtly explained the complexities of Athens, but if Pausanias/Plato was right, how would he (Cohen) account for the direct and straightforward quality of the other cities' codes? Cohen might also usefully have turned his argument round and asked questions about the interpretation of modern Mediterranean societies, and in particular why, despite many cultural similarities, they also differ greatly. Why, for instance, is homosexual behaviour far more unacceptable in some modern Mediterranean societies than in others? But I am being unfair; I am asking the author to write a new and different book, when he has already provided the world with an excellent work to be judged on its own terms.

Social Structure and Testosterone. By Theodore D. Kemper. New Brunswick, N.J.: Rutgers University Press, 1990. Pp. x+271. \$35.00.

Lionel Tiger
Rutgers University

In the long run, possibly the most serious strategic error collectively made by the scientists of sociology was an overenthusiastic embrace of Durkheim's *Rules of Sociological Method*. That this acceptance essentially abjured what became defined as reductionism is well and good in principle, but in fact, it permitted a complacent divorce between sociology and the natural sciences. Comte, Spencer, and even Weber were aware of the robust connections between social science and the natural

sciences. But under the pretext of avoiding the simplifications that biology was held to induce when the special complexity of human behavior was under study, sociology and the other social sciences developed theoretically and empirically outside the rich precincts of the natural sciences. To this day most universities permit an administrative distinction between natural and social science that also reveals an intellectual one—a distinction pointless in theory (Does it mean social behavior is not natural?) and destructive in practice. The net result is that a sociologist can receive a doctorate from nearly all major programs in the discipline while focusing almost exclusively on one animal, us, and usually also on one time period, the modern. But *Homo sapiens* is an ancient species and we have been living in a defined specific way for 1.6 million years, at least, and, genetically, we are also closer to chimpanzees than they are to the rhesus macaques.

While a number of scholars have sought to bridge the gap between biology and sociology, the way has not been easy. In my own case, my doctoral dissertation at the London School of Economics on bureaucracy and charisma in Ghana was approved for publication only on the stunning condition that I remove several pages in which I referred to the possible biological components of politics and cited Weber's comment on charisma that it moved "imperceptibly to the biological" but that within the "narrow limits" of what we did, sociologists should try to understand it as best we could. And when in 1966, with Robin Fox, I published an article in *Man: The Journal of the Royal Anthropological Society* called "The Zoological Perspective in Social Science," the hot response revealed that this was not a subject for normal placid colloquy.

The problem remains but there are more efforts at solution and one fascinating and skillful example is Kemper's sophisticated examination of the relationship between human social structure and the secretions of testosterone that flow through the bodies, mainly, and most dramatically, the bodies of men but also those of women, although less copiously. Kemper accounts for some striking work that supports his principal theoretical assertion, which is that for too long it has been assumed that the lines of influence on behavior went from the body to external social events. No, says Kemper, not so fast, because much of the time social events affect the body in definite and discernible ways. He employs the incidence of testosterone as a measure of this, and the sturdiness of his claim is suggested by the simple finding that when men have sexual intercourse their levels of circulating testosterone increase markedly but when they contrive an identical bodily event by masturbating, their testosterone levels do not budge.

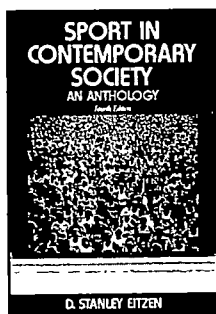
Kemper discusses a real not metaphorical relationship between testosterone and dominance. This he is not only permitted to do by the wealth of work on this well-defined relationship in other species but also at best compelled to do because of the highly suggestive findings on the link among people. For example, there is the now-well-known phenomenon that when men begin a program as subdominants, say as first-year Ma-

rine cadets, their testosterone levels are vanishingly low. However by the fourth year, these are elevated and there is a clear and genuine relationship between social structure and internal secretion.

There is also the extraordinary potential impact on young males of the surge in T levels that accompanies adolescence. While the possible Margaret Meadian ambiguities of adolescents' roles in American life and elsewhere may be a source of the difficulties many adolescents face, the law of parsimony commends the major hormonal turbulence of that phase of the life cycle as a salient factor. Certainly the controversial and visible impact of hormones on both male and female athletes attests to the reality of their impact. It is also ironic that whereas 25 years ago feminist discussion of hormones was largely verboten, acres of trees are being devoted to printed disquisitions on the hormonal components of the menopause and its meaning.

Kemper is interestingly and provocatively intrepid when he tries to show a link between social class, testosterone levels, and rates of male heterosexual intercourse. In effect, various data indicate that lower class males engage in more sexual activity than higher class when both groups are young but with increasing age the rates are reversed. Kemper's interpretation is that the efficacious physicality of lower class male life stimulates testosterone secretion while in later life the structural advantages of higher status convert into testosterone levels and sexual behavior. Is this plausible, or as plausible as more conventional sociological explanations?

I am not personally convinced about this specific proposal. Nonetheless the overall project is irreplaceably worthwhile, the use of data is responsible and thoughtful, the writing and organization highly competent, and most important, the scientific initiative and seriousness are exemplary. The discipline needs work that has the aggressive realism and professional fertility that Kemper's does. He has shown how knowledge of biology increases the complexity of assessing behavior and indeed reduces the oversimplification that uninformed sociologism permits. Reductionism? Yes. Onward and inward.



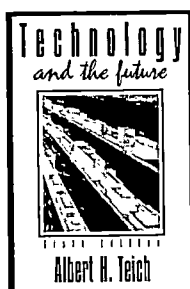
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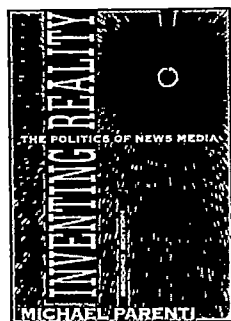
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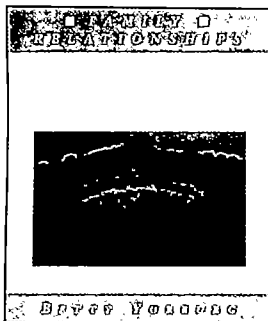
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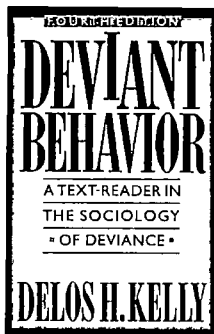
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SYMPOSIUM ON INTERGENERATIONAL TRANSMISSION

*Differential Fertility and the Distribution of Traits: The Case
of IQ—Preston and Campbell*

Comments on Preston and Campbell—Coleman, Lam

Reply to Coleman, Lam—Preston and Campbell

ALSO

*Toward a New Paradigm for the Sociological Study of U.S.
Religion—Warner*

*Narrative, Event-Structure Analysis, and Causal
Interpretation—Griffin*

The Dynamics of Organizational Rules—Zhou

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CONTENTS

Symposium on Intergenerational Transmission

- 997 Differential Fertility and the Distribution of Traits: The Case
of IQ
SAMUEL H. PRESTON AND CAMERON CAMPBELL
- 1020 Comment
JAMES S. COLEMAN
- 1033 Comment
DAVID LAM
- 1039 Reply
SAMUEL H. PRESTON AND CAMERON CAMPBELL

Also

- 1044 Work in Progress toward a New Paradigm for the Sociological
Study of Religion in the United States
R. STEPHEN WARNER
- 1094 Narrative, Event-Structure Analysis, and Causal Interpretation
in Historical Sociology
LARRY J. GRIFFIN
- 1134 The Dynamics of Organizational Rules
XUEGUANG ZHOU

Book Reviews

- 1167 *Social Change, Social Welfare, and Social Science* by Peter Tay-
lor-Gooby
ANN SHOLA ORLOFF

- 1169 *The New Politics of Poverty: The Nonworking Poor in America*
by Lawrence M. Mead
MARTHA VAN HAITSMAN
- 1171 *E. Franklin Frazier Reconsidered* by Anthony M. Platt
DEAN E. ROBINSON
- 1173 *The Visible Poor: Homelessness in the United States* by Joel Blau
PETER H. ROSSI
- 1174 *The Rational Public: Fifty Years of Trends in Americans' Policy Preferences* by Benjamin I. Page and Robert Y. Shapiro
W. LAWRENCE NEUMAN
- 1176 *Latinos and the U.S. Political System: Two-tiered Pluralism* by
Rodney E. Hero
JOHN A. GARCIA
- 1178 *Discrimination and Congressional Campaign Contributions* by
John Theilmann and Al Wilhite
ALAN NEUSTADTL
- 1180 *The Responsibilities of Wealth*, edited by Dwight F. Burlingame
MARTIN J. BURKE
- 1182 *Inventing the Nonprofit Sector and Other Essays on Philanthropy, Voluntarism and Nonprofit Organizations* by Peter Dobkin Hall
MAYER N. ZALD
- 1184 *Strategic Bankruptcy: How Corporations and Creditors Use Chapter 11 to Their Advantage* by Kevin J. Delaney
THOMAS CRUMP
- 1185 *The Structure of Corporate Political Action: Interfirm Relations and Their Consequences* by Mark S. Mizruchi
JAMES BURK
- 1187 *Responsive Regulation: Transcending the Deregulation Debate* by
Ian Ayres and John Braithwaite
ANNE M. KHADEMIAN
- 1189 *The Market Experience* by Robert E. Lane
NEIL FLIGSTEIN
- 1191 *The Rise of Historical Sociology* by Dennis Smith
GEORGE STEINMETZ

- 1193 *William Dorsey's Philadelphia and Ours: On the Past and Future of the Black City in America* by Roger Lane
ALICE O'CONNOR
- 1195 *New Philosophy of Social Science: Problems of Indeterminacy* by James Bohman
MICHAEL G. FLAHERTY
- 1196 *Back to Sociological Theory: The Construction of Social Orders* by Nicos P. Mouzelis
PAUL COLOMY
- 1198 *Modernity and Self-Identity: Self and Society in the Late Modern Age* by Anthony Giddens
PHILLIP E. HAMMOND
- 1200 *Language and Symbolic Power* by Pierre Bourdieu
JOHN D. KELLEY
- 1201 *Myth as Foundation for Society and Values: A Sociological Analysis* by Pierre Hegy
BARRY SCHWARTZ
- 1203 *Virtual Reality* by Howard Rheingold
J. TIMMONS ROBERTS
- 1206 *The Future of the Mass Audience* by W. Russell Neuman
ROGER SILVERSTONE
- 1208 *Images of Postmodern Society: Social Theory and Contemporary Cinema* by Norman K. Denzin
EMANUEL LEVY
- 1210 *Microsociology: Discourse, Emotion, and Social Structure* by Thomas J. Scheff
CANDACE CLARK
- 1211 *Mental Health, Race, and Culture* by Suman Fernando
DHARMA E. CORTÉS
- 1213 *The Imprint of Time: Childhood, History, and Adult Life* by M. E. J. Wadsworth
GARY D. SANDEFUR
- 1214 *Language, Society, and the Elderly: Discourse, Identity and Ageing* by Nikolas Coupland, Justine Coupland, and Howard Giles
ALLEN GRIMSHAW

- 1216 *The Other Half: Wives of Alcoholics and Their Social-Psychological Situation* by Jacqueline P. Wiseman
JOHN M. JOHNSON
- 1218 *The Sphinx in the City: Urban Life, the Control of Disorder, and Women* by Elizabeth Wilson
MABEL BEREZIN
- 1220 *Crime, Policing and Place: Essays in Environmental Criminology* edited by David J. Evans, Nicholas R. Fyfe, and David T. Herbert
PAUL BRANTINGHAM
- 1222 *The Cycle of Juvenile Justice* by Thomas J. Bernard
MARK D. JACOBS
- 1224 *The Gang as an American Enterprise* by Felix M. Padilla
MALCOLM W. KLEIN
- 1225 *Blacks in the White Establishment? A Study of Race and Class in America* by Richard L. Zweigenhaft and G. William Domhoff
ELIJAH ANDERSON
- 1227 *Education and the Social Construction of "Race"* by Peter Figueroa
CLARA E. RODRIGUEZ
- 1229 *Giving Up on School: Student Dropouts and Teacher Burnouts* by Margaret Diane LeCompte and Anthony Gary Dworkin
AARON M. PALLAS
- 1231 *Social Paralysis and Social Change: British Working-Class Education in the Nineteenth Century* by Neil J. Smelser
DAVID LEVINE
- 1233 *Vancouver's Chinatown: Racial Discourse in Canada, 1875–1980* by Kay J. Anderson
PETER KWONG
- 1235 *Student Protests in Twentieth-Century China: The View from Shanghai* by Jeffrey N. Wasserstrom
TONY TAM
- 1237 *Offspring of Empire: The Koch'ang Kims and the Colonial Origins of Korean Capitalism, 1876–1945* by Carter J. Eckert
GI-WOOK SHIN

- 1239 *The Territorial Imperative: Pluralism, Corporatism, and Economic Crisis* by Jeffrey J. Anderson
JOHAN P. OLSEN
- 1241 *The Politics of Irish Social Policy, 1600–1990* by Fredrick W. Powell
MICHELE DILLON
- 1243 *Economic Rationalism in Canberra: A Nation-building State Changes Its Mind* by Michael Pusey
D. GALE JOHNSON
- 1244 *The Accidental Proletariat: Workers, Politics and Crisis in Gorbachev's Russia* by Walter D. Connor
MICHAEL D. KENNEDY
- 1247 *What Is to Be Done? Proposals for the Soviet Transition to the Market*, edited by Merton J. Peck and Thomas J. Richardson
THOMAS CUSHMAN
- 1249 *Professions and the State: Expertise and Autonomy in the Soviet Union and Eastern Europe*, edited by Anthony Jones
VICTOR V. GOLUBCHIKOV
- 1251 *Labor Parties in Postindustrial Societies*, edited by Frances Fox Piven
CHRIS HOWELL
- 1253 *Hidden Technocrats: The New Class and New Capitalism*, edited by Hansfried Kellner and Frank W. Heuberger
JOSEPH H. MICHALSKI
- 1256 *Post-Military Society: Militarism, Demilitarization and War at the End of the Twentieth Century* by Martin Shaw
GREGORY McLAUCHLAN

IN THIS ISSUE

SAMUEL H. PRESTON is the Frederick J. Warren Professor of Demography and chair of the Department of Sociology at the University of Pennsylvania. His most recent book (with M. Haines) is *Fatal Years: Child Mortality in Late Nineteenth Century America*.

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XUEGUANG ZHOU is an assistant professor of sociology at Cornell University. His research interests include the role of the state in the social construction of occupations and the state-society relationship in China. James G. March, Martin Schulz, and he are currently working on a book about the evolution of organizational rules at Stanford University.

Errata

Kelley Mikelson's name was misspelled in the acknowledgment note to Stanley Lieberson and Eleanor Bell's "Children's First Names" (98:511-54). We regret the error.

A printer's error that occurred after the approval of final proof for the September 1992 issue caused table 2 in Charlotte Steeh and Howard Schuman's "Young White Adults" to be labeled as table 3. The body and notes of the table are correct as presented (see 98:354). The correct version of table 2 is printed here in its entirety.

TABLE 2

COHORT AND PERIOD EFFECTS, CONTROLLING FOR YEARS OF EDUCATION, REGION OF RESIDENCE, AND GENDER, 1984-90, FOR COHORTS
COMING OF AGE FROM 1960 TO 1990

| RACIAL POLICY QUESTION | CONSTANT ^a | COHORT | PERIOD | INTERACTIONS | |
|--------------------------------------|-----------------------|---------|----------|-----------------------|--------------------|
| | | | | Cohort | Period |
| Implementation: | | | | | |
| NO GOVERNMENT GUARANTEE ^b | 1.15 | .009 | .237*** | ... | ... |
| OPEN HOUSING ^b | 1.25 | .009*** | .006 | -.006* (North) | ... |
| AID TO MINORITIES ^b | 3.33 | .012* | -.064** | ... | ... |
| AID TO BLACKS ^b | 2.51 | .027 | -.056 | -.042** (North) | ... |
| HELP BLACKS ^b | 1.67 | .008** | .018 | ... | ... |
| FEDERAL JOB INTERVENTION | 1.78 | .004 | -.004 | ... | ... |
| BUSING ^b | 1.12 | .015*** | -.003 | -.003* (Education) | 0.024** (North) |
| FEDERAL SPENDING | 1.85 | .002 | .013 | ... | ... |
| SPENDING ON BLACKS ^b | 1.63 | .019** | .046* | -.003* (Period) | ... |
| Affirmative action: | | | | | |
| IN EDUCATION | 1.91 | .003 | -.011 | ... | ... |
| IN JOBS ^b | 1.79 | .005 | -.046 | ... | ... |
| SHOULD OVERCOME PREJUDICE | 1.89 | -.001 | -.153*** | ... | ... |

NOTE.—Variables: cohort = 1-31 (1960-90); period = 1-7 (1984-90).

^a The reference group is Southern males.

^b The analysis includes only cohorts coming of age from 1960 to 1985 for the NES and 1960 to 1987 for the GSS.

* $P < .025$.

** $P < .01$.

*** $P < .001$.

SYMPOSIUM ON INTERGENERATIONAL TRANSMISSION

Differential Fertility and the Distribution of Traits: The Case of IQ¹

Samuel H. Preston and Cameron Campbell
University of Pennsylvania

A recurrent fear during the past century is that the mean IQ level of populations will decline because persons with lower IQ scores have above-average fertility. Most microlevel data demonstrate such fertility differentials, but population IQ levels have risen rather than fallen. In this article, a simple two-sex model shows that negative fertility differentials are consistent with falling, rising, or constant IQ distributions. Under a wide variety of conditions, a constant pattern of fertility differentials will produce an unchanging, equilibrium distribution of IQ scores in the population. What matters for IQ trends is how the IQ distribution in one generation relates to the equilibrium distribution implied by that generation's fertility differentials. Intuition fails in this important area because it does not account for the macro structure within which micro results must be interpreted.

Many observed traits are inherited, in the sense that the likelihood of observing a particular value of the trait among offspring is a function of its value among parents. Heritability reflects some combination of shared genes and shared environment. Whatever the mixture in any particular circumstance, it is widely believed that higher reproduction rates by those who score lower on a particular measurable trait will, *ceteris paribus*, lead to a reduction in the mean value of that trait from one generation to the next.

¹ We would like to thank James Crow, Robert Retherford, Robert Pollak, Timothy Guinnane, Andrew Foster, Rob Mare, Daniel Vining, Randy Bulatao, and Mari Bhat for comments and suggestions. They are not, however, responsible for any errors in the paper. Correspondence may be sent to Samuel H. Preston, University of Pennsylvania, Population Studies Center, 3718 Locust Walk, Philadelphia, Pennsylvania 19104-6298.

Commonsensical as this notion may be, it is often not correct. In this article we present a simple and plausible model of trait inheritance in which fertility differentials do not produce systematic trends in the distribution of that trait. The model is applied to the inheritance of IQ, a characteristic whose fertility differentials have elicited commentary and analysis for more than a century (Anastasi 1956; for recent analyses, see Vining [1986] and Retherford and Sewell [1988]). However, it is also applicable to a wide variety of heritable social traits, such as educational attainment, persistent poverty, and political and religious affiliation.

The intuition that differential fertility will change population characteristics appears to be based on the assumption that a population consists of closed subpopulations, each of which reproduces at its own rate. If one subpopulation maintains higher rates of reproduction than all others, then that subpopulation will grow larger and larger and will eventually constitute 100% of the population. But a strict analogy to isolated populations is incorrect in the case of IQ and many other traits, since changes in scores from parent to child are very common.

Intergenerational mobility among classes vitiates analogies to isolated subpopulations. When intergenerational mobility exists, persistent reproductive differentials in reference to a particular characteristic will, under the fairly general circumstances that we describe, produce an equilibrium distribution in which the mean value of a trait, and other distributional measures, are unchanged from generation to generation. The mean score on a trait will rise or fall from one generation to the next according to whether and how the current pattern of reproductive differentials differs from those in the past, rather than whether that pattern varies from a uniform pattern of reproduction.

In this discussion, we first describe the elements that are required in order to derive the distribution of IQ in one generation from its distribution in the previous generation. These elements involve assortative mating, differential fertility, and the process that relates the IQ of a child to that of his or her parents. We then describe the data that are used to illustrate the IQ inheritance process in the United States. The role of fertility differentials is investigated under two assumptions about assortative mating. First, all individuals are assumed to marry within their IQ group, and, second, marriage is assumed to be random with respect to IQ. The first assumption leads to a mathematical proof of equilibrium in the IQ distribution, which is illustrated with U.S. data. The second does not lend itself to a proof, but we establish equilibrium properties through simulations with a considerable degree of generality. We then relax one important assumption about the fixity of the IQ inheritance process and show that results—the effects of differential fertility on trends in the IQ distribution—are robust to that assumption.

OUTLINES OF A MODEL

The model is developed purely in terms of phenotype, in this case IQ score, rather than in terms of genotype. It is a two-sex model that permits various forms of endogamous mating, differential fertility by husband-wife IQ combination, and an outcome (i.e., child's IQ) that varies according to father's and mother's characteristics. Reproduction is assumed to occur once per generation and at the same age for all IQ groups.

Central to our initial model is the assumption that, for a given combination of father's and mother's IQ, the probability distribution of children's IQ will remain fixed through time (a Markov assumption). This assumption implies that the inheritance matrix is an equilibrium with respect to the mixture of environmental and genetic influences that determine an offspring's IQ, given that of his or her parents. Parents with a particular IQ combination are thus assumed to provide the same distribution of genotypes and environments for their children in each generation.² Subsequently, we explore the behavior of inheritance systems when this assumption is relaxed.

By selecting a Markov process, we assume that reproductive behavior is related only to absolute IQ and not directly to one's relative position within the IQ distribution. Our treatment of reproductive differentials differs from the approach more commonly taken in the genetics literature (cf. Roughgarden 1979). In standard models, it is typically assumed that some process of selection, either natural or artificial, is operating. In this selection process, the presence of specific traits determines success at reproduction. Within any given generation, those who are best endowed with the specified traits will consistently have higher reproduction rates than those who are not as well endowed. Reproduction therefore always depends on relative position within a specific generation's distribution. A particular class could change from having above-average fertility to below-average fertility depending on its position relative to the mean.

Such models may be appropriate when an external agent is breeding to maximize the level of a particular trait, for example, milk output in dairy cows or endurance among race horses. They may also be appropriate when genotypes are competing for niches in a fixed environment with limited resources. In the absence of these constraints and pressures, however, there is no compelling reason why human reproduction must be modeled as such a competitive process. We assume that various IQ

² A Markov assumption with differential fertility has been used in the social science literature by Preston (1974) to deal with the heritability of occupation and by Lam (1986) to deal with the heritability of income class. However, both of these analyses are based on inheritance from one sex alone, which is clearly an inappropriate assumption in the case of IQ and many other traits.

groups are free to choose their family sizes in the absence of such competitive pressures. This assumption seems consistent with practices in contemporary Western societies.

The basic ingredients necessary for studying the reproduction of the IQ distribution in a population are

M , a marriage rule that combines the distribution of adult males by IQ and the distribution of adult females by IQ into a distribution of reproductive unions ("marriages") according to the IQ of both partners;

R , the mean number of surviving children born for each male-female IQ combination represented in reproductive unions (survival is to the age of reproduction); and

H , the function that converts the IQ of mother and father into the IQ distribution of offspring.

Given M , R , and H , it is possible to determine the IQ distribution of reproductive-aged adults in one generation from its distribution in the previous generation. By making assumptions about the future paths of M , R , and H , it is possible to derive the IQ distribution in all future generations from its distribution in an initial generation.

THE DATA

For most of the examples in this article, we use an empirically derived inheritance matrix (H) based on data from a Minnesota population presented in Reed and Reed (1965).³ This study appears to provide the largest available collection of joint IQ scores for both parents and children. From 1911 to 1918, investigators in Minnesota collected detailed data on 549 retardates who were patients at the state school and hospital at Faribault, Minnesota. The investigators also collected data on the relatives and in-laws of the patients. In 1949, researchers began a follow-up to the original study. They collected psychological data on descendants of 289 of the original retardates, as well as the descendants of the retardates' relatives and in-laws. The data were organized into pedigree charts representing a total of 83,529 people.

Whenever possible, the data collected included IQ. Because intelligence testing was common by the time of the follow-up, there were in fact 1,016 families in the later generations in which IQs were recorded for both parents and at least one child. The IQ tests were always administered to individuals before marriage and usually through their school system (Higgins, Reed, and Reed 1962). The data on these 1,016 families form the basis for our empirical H matrix, which allows us to produce probability distributions for children's IQ based on father's and mother's IQ. We were able to construct an IQ inheritance matrix with seven IQ

³ We would like to thank Paul Taubman for bringing this data set to our attention.

classes: 74 and below, 75–84, 85–94, 95–105, 106–15, 116–25, as well as 126 and above.

Even though relationship to a retardate was a prime criterion for inclusion in the Minnesota data set, there is no overrepresentation of retardates in our sample of families. The IQ distributions show that only 3.9% of the fathers, 2.1% of the mothers, and 2.9% of the children were retardates. Researchers apparently gathered data on enough of the retardates' distant relatives that the retardates themselves constituted only a small portion of the final sample. The presence of retardates in the data therefore does not seem sufficient to warrant special measures in dealing with the data or interpreting the results.

We used the distribution children's IQ \times parents' IQ to construct the *H* matrix, which converts parents' IQ into a distribution for their offspring. Specifically, for each of the 49 parents' IQ class combinations (7×7), we calculated the proportion of children who ended up in each of the seven IQ classes. These proportions make up the cells of the *H* matrix described later. We therefore do not assume a specific functional form for the relationship between parents' IQ and children's IQ.

Data were missing for two of the 49 IQ class combinations. In table 1, which presents the distribution of children according to parents' IQ classes, there are no children born to marriages between highest IQ class males and lowest IQ class females. Nor are there children born to marriages between second-highest IQ class males and second-lowest IQ class females. For the purposes of analysis, the distribution of children's IQ in these cells was assumed to be identical with the cells in which parents' IQ classes were reversed.

The *H* matrix is too large to reproduce here, but the mean and variance of children's IQ according to parents' IQ in tables 2 and 3 are sufficient

TABLE 1
TOTAL OFFSPRING BY IQ CLASS OF PARENTS

| IQ CLASS OF FATHER | IQ CLASS OF MOTHER | | | | | | |
|--------------------|--------------------|-------|-------|--------|--------|--------|-------|
| | 74 or Lower | 75–84 | 85–94 | 95–105 | 106–15 | 116–25 | 126 + |
| 74 or lower | 54 | 40 | 19 | 29 | 19 | 5 | 5 |
| 75–84 | 7 | 19 | 22 | 44 | 27 | 10 | 2 |
| 85–94 | 11 | 12 | 48 | 107 | 78 | 39 | 11 |
| 95–105 | 16 | 40 | 94 | 186 | 166 | 100 | 17 |
| 106–15 | 1 | 22 | 60 | 167 | 143 | 77 | 17 |
| 116–25 | 10 | 0 | 20 | 62 | 58 | 28 | 11 |
| 126+ | 0 | 5 | 2 | 26 | 15 | 16 | 19 |

SOURCE.—Reed and Reed (1965).

TABLE 2
MEAN IQ OF OFFSPRING BY IQ CLASS OF PARENTS

| IQ CLASS OF FATHER | IQ CLASS OF MOTHER | | | | | | |
|--------------------|--------------------|-------|-------|--------|--------|--------|-------|
| | 74 or Lower | 75-84 | 85-94 | 95-105 | 106-15 | 116-25 | 126+ |
| 74 or lower | 74.4 | 90.3 | 103.3 | 101.1 | 97.5 | 97.8 | 113.6 |
| 75-84 | 82.4 | 89.9 | 102.2 | 101.3 | 102.5 | 113.9 | 109.0 |
| 85-94 | 91.1 | 99.6 | 102.4 | 106.0 | 107.8 | 108.3 | 109.4 |
| 95-105 | 93.8 | 99.2 | 106.0 | 108.2 | 108.9 | 111.6 | 113.6 |
| 106-15 | 104.0 | 102.0 | 104.1 | 109.1 | 112.9 | 113.4 | 116.1 |
| 116-25 | 98.3 | 113.9 | 101.9 | 110.9 | 114.2 | 108.9 | 115.8 |
| 126+ | 113.6 | 107.4 | 113.0 | 113.1 | 114.9 | 115.2 | 123.5 |

SOURCE.—Reed and Reed (1965).

TABLE 3
VARIANCE OF IQ OF OFFSPRING BY IQ CLASS OF PARENTS

| IQ CLASS OF FATHER | IQ CLASS OF MOTHER | | | | | | |
|--------------------|--------------------|-------|-------|--------|--------|--------|------|
| | 74 or Lower | 75-84 | 85-94 | 95-105 | 106-15 | 116-25 | 126+ |
| 74 or lower | 19.7 | 17.6 | 13.0 | 16.0 | 13.0 | 22.7 | 10.8 |
| 75-84 | 15.6 | 12.0 | 10.0 | 16.6 | 12.0 | 9.5 | 3.0 |
| 85-94 | 16.1 | 10.1 | 16.7 | 10.2 | 10.5 | 12.3 | 4.8 |
| 95-105 | 16.7 | 11.2 | 11.2 | 11.4 | 11.0 | 11.1 | 6.3 |
| 106-15 | 13.0 | 12.0 | 18.7 | 12.0 | 10.8 | 12.4 | 13.8 |
| 116-25 | 12.0 | 9.5 | 13.2 | 11.5 | 9.2 | 22.2 | 10.7 |
| 126+ | 10.8 | 17.2 | 9.0 | 9.6 | 11.0 | 13.3 | 13.0 |

SOURCE.—Reed and Reed (1965).

to suggest some of its important characteristics. The assumption we used for filling in the two empty cells seems to be a safe one: mean IQ of children is largely symmetric with respect to parents' IQ combination. In other words, the mean IQ of children born to class *i* males and class *j* females is generally within a few points of the mean for children born to class *j* males and class *i* females. The variance matrix is also remarkably symmetric, except in situations where one of the spouses is in the highest IQ class.

ENDOGENOUS MATING

A general demonstration of the properties of a process of IQ inheritance requires the use of mathematics. We first report a general result applicable to the situation of perfect endogamy, in which male and female par-

ents are always drawn from the same IQ class. Let us recognize n IQ classes in a population. There are thus n^2 combinations of husband's and wife's IQ. Construct a row vector M , whose elements are the number of marriages between a man of class i and a woman of class j . Assume that this vector is arranged as

$$M = [m_{11} m_{12} \dots m_{1n} m_{21} m_{22} \dots m_{2n} \dots m_{nn}],$$

where m_{ij} is the number of marriages of males of type i to females of type j .

Each of these combinations has a reproduction rate, r_{ij} , which represents the mean number of surviving offspring produced per ij couple. Survivorship is to the age of reproduction. Let R be a diagonal matrix whose diagonal elements are r_{ij} and whose off-diagonal elements are zero:

$$R = \begin{bmatrix} r_{11} & 0 & & & \\ 0 & r_{22} & & & \\ & 0 & \dots & & \\ & & 0 & r_{n-1, n-1} & 0 \\ & & & 0 & r_{nn} \end{bmatrix}.$$

The number of surviving children born into a particular father-mother IQ class is c_{ij} , and the row vector representing this set of numbers,

$$C = [c_{11} c_{12} \dots c_{1n} c_{21} c_{22} \dots c_{2n} \dots c_{nn}],$$

can be found by premultiplying R by M :

$$MR = C.$$

The inheritance matrix, H , converts one's father-mother IQ combination into one's own IQ. This matrix has dimensions of $n^2 \times n$ and will be represented as

$$H = \begin{bmatrix} h_{111} & h_{112} & \dots & h_{11n} \\ h_{121} & h_{122} & & \\ h_{131} & \vdots & & \\ \vdots & & & \\ h_{1n1} & & & \\ h_{211} & & & \\ \vdots & & & \\ h_{2n1} & & & \\ \vdots & & & \\ h_{nn1} & \dots & h_{nnn} \end{bmatrix},$$

where h_{ijk} is the proportion of children of parental type ij who have an IQ score in class k . The product of C and H determines the IQ distribution of surviving offspring, which will be a row vector,

$$Q = [q_1 q_2 q_3 \dots q_n],$$

where q_k is the number of surviving offspring with IQ of k . Therefore, the entire process of determining the IQ distribution of generation $t + 1$ from the marriages of generation t can be represented as

$$\begin{matrix} M^t & R^t & H^t & \\ 1 \times n^2 & n^2 \times n^2 & n^2 \times n & \end{matrix} Q^{t+1} = 1 \times n. \quad (1)$$

Equation (1) is an identity for a particular generation of marriages and their offspring. Modeling features arise in discussing the dynamics of this system from generation to generation. For this discussion, it is necessary to postulate a process by which the offspring of generation $t + 1$ form the marriages represented by M^{t+1} .

Assume that half of the offspring are male and half female and that the distribution of IQ is identical for the two sexes in each generation. If all marriages occur between persons of the same IQ class, then there will be simply $q_i/2$ marriages between persons of type i , and no marriages when $i \neq j$. Therefore the complete process of projection of marriage vectors from generation t to generation $t + 1$ can be represented as

$$M^t R^t H^t N^t \times 1/2 = M^{t+1},$$

where N is an $n \times n^2$ matrix with elements of unity where the column number is equal to the row number squared, and zero everywhere else:

$$N = \begin{bmatrix} 1 & 0 & 0 & 0 & \dots & 0 \\ 0 & 0 & 0 & 1 & \dots & \vdots \\ \vdots & & & & & 0 \\ 0 & 0 & \dots & \dots & 0 & 1 \end{bmatrix}.$$

Designate the product $(R^t H^t N^t)/2$ as P^t . Let us assume that reproduction rates by IQ class of husband and wife remain constant over time, that the IQ inheritance matrix remains constant, and that the mating rule remains constant. Then,

$$P^t = P,$$

$$M^t P = M^{t+1} \text{ for all } t > 0,$$

and

$$M^t P^y = M^{t+y}. \quad (2)$$

The behavior of the system shown in equation (2) has been extensively explored in the mathematical demography literature, where M usually represents an age distribution and P a Leslie matrix of fertility and mortality rates. Under certain circumstances, the process represented by (2) will converge to a unique stable proportional distribution, that is, to a distribution whose elements are proportional to one another from one time period to the next. The condition under which this stabilization occurs is that P be a primitive matrix. Primitivity requires that some power of P have only positive elements (Keyfitz 1977; Parlett 1970).

The product, RH , will be a primitive matrix in the first generation if reproduction rates are nonzero in every husband-wife IQ class, which is surely a tolerable assumption; and if all elements in H are positive, which implies that there is some positive probability that a child born into a particular father-mother class will wind up in any particular IQ class himself. This condition is stronger than necessary to achieve primitivity, however; all that is necessary is that a direct descendant of an ij couple some generation hence eventually be able to reach any particular IQ class. If this relatively weak condition is met, then primitivity of RH is assured (Keyfitz 1977). Such a condition appears very plausible in the case of intelligence.

The mathematics of complete endogamy can be simplified by recognizing that if no marriages transpire between persons of different IQ classes, then the marriage matrix can be reduced from $1 \times n^2$ to $1 \times n$; there is no need to account for marriage possibilities that do not and never will exist. Note that what we can do here amounts to reducing a two-sex process to a one-sex process; since all marriages occur within one's class, the classes can be seen (in formal terms) as reproducing asexually. In this simplified case, we have the following dimensions for the IQ inheritance process:

$$\begin{array}{ccccccc} M^t & R & H & N^* & \times 1/2 & M^{t+1} \\ 1 \times n & n \times n & n \times n & n \times n & & 1 \times n \end{array} =$$

Since the product, RH , is a primitive matrix, and since N^* is the identity matrix, P is necessarily primitive. Therefore, we have shown that a unique, stable, equilibrium distribution will necessarily be established under complete endogamy. In equilibrium, of course, whatever fertility differentials by IQ are present in the R matrix coexist with an unchanging distribution of IQ through time. The equilibrium distribution has adapted to those differentials in such a way that intergenerational flows into and out of each IQ class exactly counterbalance one another.

Primitivity of P assures that, regardless of the initial distribution, the

equilibrium distribution will contain a positive proportion in each class. The equilibrium, therefore, is not a trivial corner solution in which the whole population comes to reside in one IQ class. If, however, there is no mobility between classes (H is an identity matrix), then P will not be primitive but will equal $R \times \frac{1}{2}$. As this matrix is raised to higher and higher powers, the entire population will (asymptotically) come to reside in the class with the highest reproduction rate—as long, that is, as someone was in that class initially.

EMPIRICAL EXAMPLES WITH COMPLETE ENDOGAMY

We present examples of the behavior of an inheritance system with complete endogamy in figures 1 and 2. The examples use the H matrix derived from the Reed and Reed data with seven IQ classes. According to the above proof, choice of initial distribution is irrelevant to equilibrium distribution. We therefore select an initial distribution not for its realism but for its ability to help demonstrate properties of the model. In our initial distribution, 70% of the population is in the lowest IQ class and 5% is in each of the other six IQ classes.

In both examples fertility is additive across father's and mother's IQ class. In figure 1, fertility is positively correlated with IQ. The number

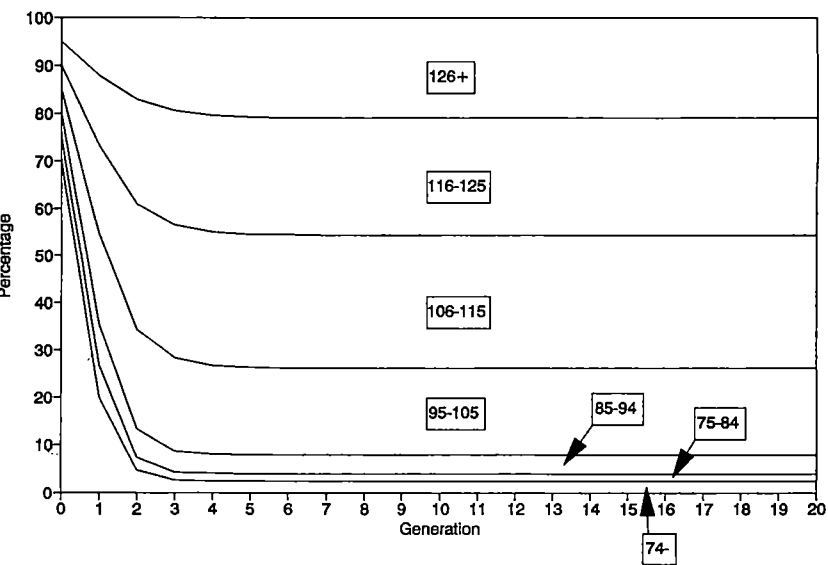


FIG. 1.—IQ distribution over time: high IQ—high fertility (complete endogamy)

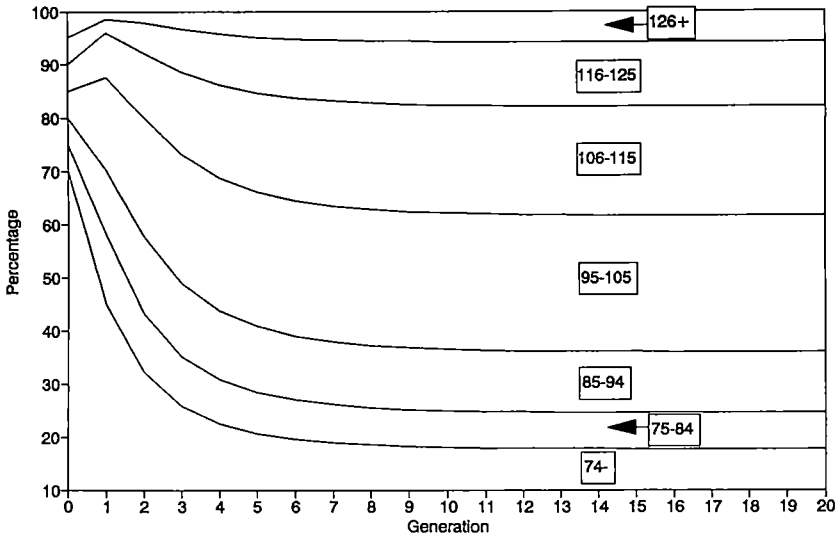


FIG. 2.—IQ distribution over time: low IQ—high fertility (complete endogamy)

of surviving children in a marriage between a male of IQ class i and a female of IQ class j is given by

$$R_{ij} = 0.50 + 0.25i + 0.25j,$$

where i and j range from zero to six, with zero as the lowest IQ class and six as the highest.

The mean and median IQ of the population in figure 1 increases over time, reaching an equilibrium distribution within eight generations.⁴ In those eight generations, median IQ moves from the lowest category, 74 and below, to the third highest category, 106–115. The proportion of the population in the low IQ categories declines precipitously, while the proportion in the high IQ categories correspondingly increases.

In figure 2, fertility is negatively correlated with IQ. The number of surviving children is given by

$$R_{ij} = 3.50 - 0.25i - 0.25j.$$

Even though IQ is negatively correlated with fertility in figure 2, mean IQ still increases substantially from its initial to its equilibrium value. We are surprised to discover that the population reaches equilibrium

⁴ Equilibrium in this case is defined as occurring when the proportion in each class is repeated (out to five decimal places) in two consecutive generations.

more slowly than in figure 1, even though the equilibrium distribution is closer to the initial distribution than was the case in figure 1. Furthermore, changes in the proportion in each IQ class are not monotonic: the proportion of the population in the two highest IQ classes dips in the first generation but subsequently recovers as the medium IQ classes upon which the high IQ classes depend are fed from the lowest IQ classes.

Although the IQ distribution improves for several generations in these two examples, it could just as easily have deteriorated. Had we chosen an IQ distribution in the initial generation that was more favorable than the equilibrium distribution (higher proportions in the highest IQ group) we would have observed a gradual deterioration in the IQ distribution as it approached its equilibrium state.

In this and other empirical applications, our intent is to illustrate the properties of inheritance systems using realistic data. We are not endeavoring to make predictions about IQ distributions or to precisely identify equilibrium properties. To achieve these goals, we would need to pay more attention to the appropriate number of IQ classes with an eye toward minimizing within-cell heterogeneity. This number is presumably larger than the seven classes that we have employed. While a recognition of more classes would be likely to change the numerical details of our examples, it would not alter the basic properties that we have identified. In the case of completely endogamous mating, at least, that outcome is assured by the formal result that we have established. To restate that result: there is no necessary association between fertility differentials by IQ and trends in the IQ distribution. Any persistent pattern of fertility differentials will eventually produce an equilibrium distribution of IQ in the population, that is, a distribution that is constant from generation to generation.

RANDOM MATING

We have established that, under complete endogamy, the process of IQ inheritance that we have proposed will converge to an equilibrium distribution. Unfortunately, we have not been able to establish a comparable formal result for the situation of random mating, where the probability of marrying a person in a particular IQ class is independent of one's own IQ and depends only on the proportion of potential mates who are in the specified IQ class. We will show that an equilibrium distribution occurs under a plausible random-mating scenario, but the demonstration constitutes a proof neither of existence nor of uniqueness of equilibria.

We may anticipate the results in this section and the next by stating that, in all of the scenarios that we investigate, differential fertility leads

to an equilibrium in which the IQ distribution is reproduced from one generation to the next. In two instances, however, the equilibrium is not unique. That is, a particular regime of fertility differentials is compatible with different steady-state outcomes. The existence of multiple equilibria means that initial conditions, including fertility differentials in the past, can affect the steady-state distribution of IQ in the population. When an equilibrium is unique, no such dependency exists. Thus, the possibility of multiple equilibria gives greater scope for fertility differentials to affect levels and trends in IQ distributions, since differentials in one generation can drive the system toward one of two very different outcomes. However, the two instances in which multiple equilibria arise are of more theoretical than practical significance. One occurs when the H matrix is not primitive, and the other when patterns of fertility differentials show highest fertility levels at the extremes of the IQ distribution. When this latter pattern of fertility differentials is combined with the empirical Reed-Reed H matrix, unique equilibria always result.

There have been several investigations of simplified random-mating systems with differential fertility in the genetics literature. In particular, Hader and Liberman (1975), following the work of Penrose (1949) and Bodmer (1965), deal with the distribution of genotypes for two alleles at a single locus. Offspring are produced by random mating among genotypes, with differential fertility by mating type. Fertility is symmetric: fertility levels in the 3×3 table representing father's and mother's genotype are symmetric with respect to both vertical and horizontal movements away from the center cell (Aa, Aa).

Hader and Liberman (1975) show that there are between three and seven equilibrium distributions of genotype, with the number depending on the relative fertilities. Most but not all of the equilibria are stable, and at least one is a nontrivial interior solution with positive fractions in each class. In this system, the equilibrium reached depends on the initial distribution of the population. If the entire population begins as AA, for example, then that will be the equilibrium state.⁵

Bodmer (1965) provides a similar demonstration of multiple equilibria

⁵ It should be noted, however, that there is less intergenerational mobility between classes in this model than we might expect in the case of intelligence. In fact, one-third of the elements in the inheritance matrix (H) for the one-locus two-allele process are zeros. Thus if both parents are AA they cannot produce either an Aa or an aa offspring. Similarly, if both parents are aa they cannot produce either an AA or an Aa offspring. The presence of these absorbing states plays a key role in producing multiple equilibria. It is unclear from this example whether multiple equilibria would appear if there were no absorbing states. It is also unclear whether corner solutions would appear if there were no absorbing states.

for the two allele—one locus problem when fertility is multiplicative (the fertility of an ij union is the product of the fertility of type i males, assumed constant across type of female, and of type j females, assumed constant across type of male). Were intelligence carried on two alleles at a single locus, rather than being polygenic (and environmentally influenced), these results would be directly applicable to our problem. As they stand, they are primarily useful as a formal demonstration that equilibria can exist under a simplified process with random mating and differential fertility.

Pollak (1990) proves the existence of an equilibrium under much more general conditions. The number of classes of males and females is unlimited and need not be the same for the two sexes; mating can occur under a very general rule, the basic requirements of which are that (1) the number of unions involving males i or females j cannot exceed the total number of such persons in the population and (2) the function that maps the vectors of available males and females by type into the number of matings must be homogenous of degree one and continuous. Fertility differentials by father's and mother's type produce at least one equilibrium under these general conditions; Pollak also demonstrates circumstances under which multiple equilibria arise.

In the absence of theoretical results that are directly pertinent to IQ inheritance with random mating, we explore the properties of such a model through simulation. Results using seven IQ classes demonstrate that primitivity of the RH matrix is insufficient to guarantee the uniqueness of equilibria under random mating. That is, multiple equilibria can arise under certain fertility regimes. However, primitivity of RH in combination with a regime of additive fertility always produces, in our examples, unique equilibria under random mating. As explained earlier, primitivity means that a direct descendant of an ij couple some generation hence must eventually be able to reach any particular IQ class.

We arrived at these conclusions after experimenting with an H matrix of the following form:

$$\begin{array}{ll}
 h_{ijk} = 1 - e & i = j = k \\
 = e & i = j = 0, k = 1 \\
 & i = j = 6, k = 5 \\
 = e/2 & i = j, i \neq 0, i \neq 6, \\
 & k = i + 1, i - 1 \\
 = 1/2 & i \neq j, k = i, j \\
 = 0 & \text{otherwise.}
 \end{array}$$

In this simplistic inheritance matrix, a child whose parents come from different IQ classes has equal chances of ending up in either IQ class and no chance of moving to any other class. However, a child whose parents are both from the same IQ class has a probability $e/2$ of being in the next higher IQ class, $e/2$ of being in the next lower IQ class, and $1 - e$ of being in the same IQ class. When both parents are in either the lowest class or the highest class, then the child has probability e of ending up in the neighboring class. Higher values of e are thus associated with greater variance in children's IQ outcomes, given the IQ of their parents. The advantage of using this particular H matrix for theoretical purposes is that, by reducing e gradually, we can change the H matrix slowly from primitive to nonprimitive and discover at what stage in this process multiple equilibria arise.

When e is zero and there is no differential fertility, the IQ distribution is simply reproduced each generation. In that case, multiple equilibria clearly exist since every initial distribution is an equilibrium.⁶ In the case of differential fertility with $e = 0$, we have also found instances in which there are a limited number of multiple equilibria. For example, if fertility is additive, then the equilibrium population is generally found to reside entirely in the highest IQ class which had a nonzero initial distribution.

If e is nonzero, however, the RH matrix becomes a primitive matrix. If primitivity combined with random mating were an insufficient condition for the existence and uniqueness of equilibria, then we would expect that as e approaches 0, but does not reach it, multiple equilibria would at some point become apparent. That is, there should be some boundary that divides H matrices into two classes: those that are close enough to an "identity" matrix to produce multiple equilibria, and those that are sufficiently primitive to produce unique equilibria.

As we see in table 4, nonzero values of e , combined with additive fertility, always yield unique equilibria.⁷ Even very small values of e

⁶ Algebraically, the number of persons who are in IQ class 1 in generation $t + 1$ will be

$$\begin{aligned} q_1^{t+1} &= (q_1^t)^2 + 2q_1^t q_2^t \left(\frac{1}{2}\right) + 2q_1^t q_3^t \left(\frac{1}{2}\right) + \dots + 2q_1^t q_n^t \left(\frac{1}{2}\right) \\ &= q_1^t (q_1^t + q_2^t + \dots + q_n^t) \\ &= q_1^t. \end{aligned}$$

⁷ For this table, a distribution is regarded as an equilibrium if the proportion in each IQ class is reproduced to the seventh decimal place in the next generation. To search for equilibria, for each model we tried 210 initial distributions which represented all possible ways of distributing quarters of the population among the seven IQ classes. Thus the first initial distribution had the entire population in the first category. The second initial distribution had three-quarters of the population in the first category

TABLE 4
EQUILIBRIUM IQ DISTRIBUTIONS: RANDOM MATING UNDER DIFFERENT
FERTILITY REGIMES

| LEAKAGE (<i>e</i>) | % IN IQ CLASS AT EQUILIBRIUM | | | | | | |
|-------------------------|------------------------------|-------|-------|--------|--------|--------|-------|
| | 74 or Lower | 75-84 | 85-94 | 95-105 | 106-15 | 116-25 | 126 + |
| Uniform Fertility | | | | | | | |
| .10 | 11.0 | 15.6 | 15.6 | 15.6 | 15.6 | 15.6 | 11.0 |
| .20 | 11.0 | 15.6 | 15.6 | 15.6 | 15.6 | 15.6 | 11.0 |
| .30 | 11.0 | 15.6 | 15.6 | 15.6 | 15.6 | 15.6 | 11.0 |
| .40 | 11.0 | 15.6 | 15.6 | 15.6 | 15.6 | 15.6 | 11.0 |
| Additive Fertility | | | | | | | |
| .10 | .0 | .0 | .0 | 1.6 | 15.8 | 40.9 | 41.7 |
| .20 | .0 | .0 | .3 | 5.7 | 21.1 | 38.3 | 34.6 |
| .30 | .0 | .0 | 1.1 | 8.9 | 22.7 | 36.2 | 31.0 |
| .40 | .0 | .1 | 2.3 | 11.0 | 23.4 | 34.6 | 28.7 |
| Centrifugal Fertility | | | | | | | |
| .10 | 44.7 | 41.5 | 13.0 | .8 | .0 | .0 | .0 |
| | .0 | .0 | .0 | .8 | 13.0 | 41.5 | 44.7 |
| .20 | 37.0 | 39.7 | 19.5 | 3.7 | .1 | .0 | .0 |
| | .0 | .0 | .1 | 3.7 | 19.5 | 39.7 | 37.0 |
| .30 | 33.1 | 37.8 | 21.9 | 6.7 | .5 | .0 | .0 |
| | .0 | .0 | .5 | 6.7 | 21.9 | 37.8 | 33.1 |
| .40 | 30.6 | 36.2 | 22.9 | 9.0 | 1.2 | .0 | .0 |
| | .0 | .0 | 1.2 | 9.0 | 22.9 | 36.2 | 30.6 |

result in a unique equilibrium. In the case in which fertility is uniform over all IQ class combinations, the equilibrium distribution is in fact unaffected by the level of *e*. In the case where fertility is additive and favors high IQ couples,⁸ the equilibrium distribution becomes more and more concentrated in the higher IQ classes as *e* decreases. On the basis of this example, it seems that primitivity of *RH* combined with random mating and additive fertility are sufficient for the existence and uniqueness of equilibria.

These results do not constitute a formal proof that primitivity of the *RH* matrix and an additive fertility regime are sufficient conditions for the existence and uniqueness of equilibria in the case of random mating.

and one-quarter in the second. The 210th initial distribution had the entire population in the last IQ category.

⁸ In this example, the fertility of a marriage between a male of class *i* and a female of class *j* is given by $R_{ij} = 2.2 + 0.1 \times (i + j)$.

TABLE 5

TYPES OF EQUILIBRIA UNDER RANDOM MATING REGIME, WITH VARIATION IN TYPE OF IQ INHERITANCE MATRIX AND TYPE OF FERTILITY REGIME

| VALUE OF e | FERTILITY REGIME | | |
|---------------------------|------------------|----------|-------------|
| | Uniform | Additive | Centrifugal |
| Zero | Multiple | Multiple | Multiple |
| Nonzero (primitive) | Unique | Unique | Multiple |

But they are strongly suggestive. It remains possible of course that there is some RH that is primitive but will not produce unique equilibria even with additive fertility. But it is hard to imagine a form for RH that would be less likely to produce unique equilibria than the above matrix when e is small.

Primitivity of the RH matrix and random mating alone are insufficient to assure the uniqueness of equilibria. Table 4 presents results for the case of what we refer to as centrifugal fertility, where the fertility of a marriage between a male of IQ class i and a female of IQ class j is given by

$$R_{ij} = 2.0 + 0.1 * |6 - i - j|.$$

By this assumption, fertility increases with distance from the minor diagonal. According to table 4, there are at least two distinct and very different equilibria for each value of e . In this case, leakage between classes in the H matrix cannot compensate for the effects of differential fertility in the R matrix to propel the distribution to a unique equilibrium.

Table 5 summarizes the results of the simulations with respect to the uniqueness of equilibria. When H is a primitive matrix and fertility is constant or changes monotonically with IQ, unique equilibria are produced. This result applies even when the "leakage" from IQ classes of parents to IQ class of offspring is very small. However, multiple equilibria can result when H is primitive and fertility is highest at extreme values of IQ. In this latter instance, which is probably rare empirically and has not been the subject of eugenic discussions, small changes in initial conditions, including initial fertility differences, appear capable of producing very large differences in the ultimate distribution of IQs.

RANDOM MATING WITH THE REED INHERITANCE MATRIX

We now turn to the model under random mating using the empirical inheritance matrix derived from Reed and Reed (1965). Even if we cannot characterize the behavior of the random-mating model analytically, we

can, through empirical investigations, at least understand its behavior under the range of conditions relevant to human experience. Our results are therefore meant to illustrate properties of the system under a plausible set of conditions.

As expected from the previous section, the use of the Reed-Reed inheritance matrix with random mating and additive fertility always produces unique equilibria. With the empirically derived *H* matrix, IQ distributions at equilibrium in the random-mating model prove to be less sensitive to fertility differentials than in the endogamous mating model. Figure 3 plots mean IQ at equilibrium obtained in the endogamous model and the random-mating model under a range of additive fertility regimes. Shifting from a fertility regime favoring high IQ to a regime favoring low IQ, mean IQ in the random-mating scenario declines by only a few points, whereas mean IQ in the endogamous scenario declines more substantially. Presumably, the greater mixing of IQ cases engendered by random mating is responsible for the greater robustness of mean IQ to changes in fertility differentials.

When applied to the Reed-Reed inheritance matrix, centrifugal fertility does not yield multiple equilibria. Table 6 presents equilibrium distributions in which the fertility of a marriage between a male of IQ class *i*

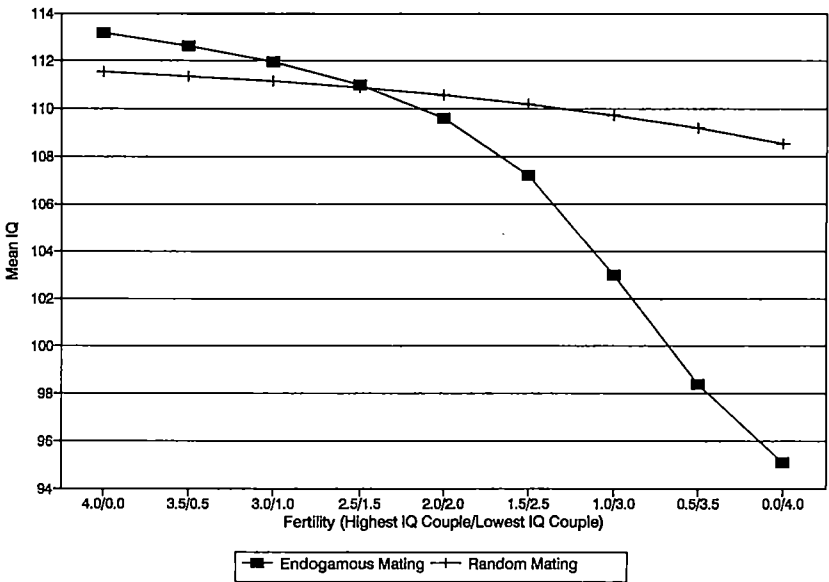


FIG. 3.—Mean IQ at equilibrium: sensitivity to differential fertility

TABLE 6
RANDOM MATING WITH THE REED INHERITANCE MATRIX
UNDER CENTRIFUGAL FERTILITY

| <i>k</i> | % IN IQ CLASS AT EQUILIBRIUM | | | | | | |
|------------|------------------------------|-------|-------|--------|--------|--------|------|
| | 74 or Lower | 75-84 | 85-94 | 95-105 | 106-15 | 116-25 | 126+ |
| 0 | 1.2 | 1.1 | 5.2 | 23.5 | 32.2 | 25.4 | 11.2 |
| .20 | 1.2 | 1.1 | 4.9 | 22.6 | 31.8 | 26.1 | 12.3 |
| .40 | 1.2 | 1.0 | 4.8 | 22.0 | 31.4 | 26.5 | 13.0 |
| .60 | 1.2 | 1.0 | 4.7 | 21.6 | 31.2 | 26.8 | 13.6 |
| .80 | 1.2 | 0.9 | 4.6 | 21.2 | 31.0 | 27.0 | 14.0 |
| 1.00 | 1.2 | 0.9 | 4.5 | 21.0 | 30.9 | 27.2 | 14.3 |

and a female of IQ class j is given by an equation similar to the one used in table 4:

$$R_{ij} = 2.0 + k * |6 - i - j|,$$

where k is a constant determining how fast fertility increases as one moves away from the minor diagonal. Even when $k = 1$, that is, when couples who are either both in the lowest IQ class or both in the highest IQ class have fertility of 8, equilibria are unique. Apparently, the greater mobility between classes in the empirical H matrix compared to the simplified matrix examined above is sufficient to overcome the tendency for centrifugal fertility to produce multiple equilibria.

RANDOM MATING WITH A CHANGING INHERITANCE MATRIX

One objection that can be raised to the previous results is that the H matrix is assumed to be fixed through time. Undoubtedly, environmental factors influence outcomes, and environments can change. Even a purely genetic process would not be expected to lead to a perfectly fixed H matrix, since alleles, not genotypes, are transmitted to the next generation, and genotypes can be disrupted by Mendelian segregation and recombination (Hartl and Clark 1989).

In order to deal with this issue, we relax the requirement that the inheritance matrix be fixed from generation to generation. While leaving unchanged the mean IQ of children born to each IQ combination in the Reed-Reed H matrix, we allow the variance to change from generation to generation according to

$$s_{ij} = \underline{s}_{ij}(1 + x),$$

where x is a random number between $-\alpha$ and α . The variance in a particular generation (s_{ij}) is therefore equal to the empirically observed variance (\underline{s}_{ij}) multiplied by a random number between $1 + \alpha$ and $1 - \alpha$. The empirically observed variances (\underline{s}_{ij}) are in fact the same variances that were presented in table 3.

Changing the variances makes it impossible to use the same empirical H matrix that we used in earlier examples. Instead, using the means and variances for IQ of children in each parents' IQ class combination, we construct a new inheritance matrix every generation. For each parents' IQ class combination, the distribution of the children's IQ is assumed to be a normal distribution. The mean is taken from table 2. The variance is taken from table 3, but multiplied by the random factor mentioned above. With the means and the modified variances, we can fill in the cells of the H matrix.

Figure 4 presents mean IQ under random mating with uniform fertility and differential fertility in the cases where there is no random variation ($\alpha = 0$) and where there is random variation ($\alpha = 0.5$).⁹ According to figure 4, the introduction of random variation into the inheritance matrix can diminish the effects of differential fertility on mean IQ. When there is no random variation, the difference in mean IQ at equilibrium between the uniform and differential fertility regimes is .319. When $\alpha = 0.5$, there is no equilibrium, but the difference in mean IQ is narrowed to approximately 0.1.

When $\alpha = 0.5$, there is of course considerable variation in mean IQ from generation to generation. Variation occurs because each new inheritance matrix produced by a changed variance is associated with a particular equilibrium distribution. Every time a new H matrix is imposed, that is, every new generation, the distribution begins moving toward the equilibrium implied by the new H matrix, even though the target will be changed again in the next generation. But as in the case of a fixed H matrix, the IQ distribution with differential fertility becomes trendless within a short space of time, as does the distribution without differential fertility. Thus, the link between fertility differentials and trends in IQ is severed even when shocks are introduced into the H matrix.

What we have not allowed to change about the H matrix is the number of IQ classes. The extremes of the IQ distribution are assumed to be fixed. But introducing new IQ classes that are unoccupied in the initial generation and allowing some probability that these classes would be

⁹ In this case, fertility is additive and given by the same equation as was used for additive fertility in table 4. The top pair of lines contains random variation in H . Within each pair, the top line has uniform fertility. Means are plotted for the pair with random variation.

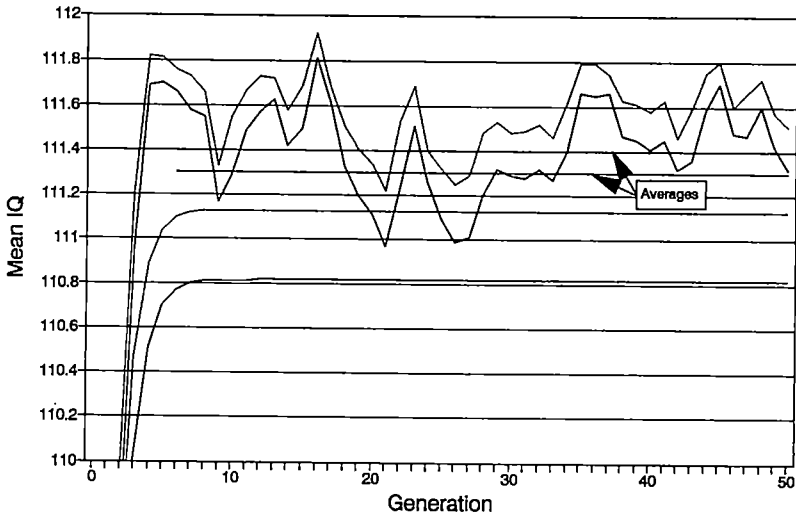


FIG. 4.—Effect on mean IQ of random variation in inheritance matrix (line 1—uniform fertility, $\alpha = 0$; line 2—differential fertility, $\alpha = 0$; line 3—uniform fertility, $\alpha = .5$, line 4—differential fertility, $\alpha = .5$).

occupied by a descendant of the initial generation clearly would not alter the equilibrium results. These new extremes could be added so far beyond the tails of any known IQ distribution that no one could doubt that they comprise the imaginable range of human intelligence. Expanding the range of classes in the H matrix may be desirable if we are dealing with millennial changes, but it seems unnecessary when dealing with the shorter time spans considered here.

DISCUSSION

These results suggest that negative reproductive differentials are perfectly compatible with a constant, improving, or deteriorating IQ distribution in the population. What matters for the trend in IQ is how those reproductive differentials relate to the pattern that has prevailed in the past. If the present IQ distribution is lower than the equilibrium distribution implied by current values of H , R , and M —even if R has negative fertility differentials by IQ—then the IQ distribution can be expected to improve over time when H , R , and M remain constant.

These results suggest that the “Cattell paradox” is not paradoxical. Cattell (1951) noted that, despite negative relations between IQ and fertility that had apparently prevailed for several generations in certain countries, trends in mean national IQ showed no decrease in those countries. But, as we have seen, there is no reason to expect that it should have

decreased. Other explanations of the Cattell paradox related to environmental influences on IQ, or to the differential incidence of nonmarriage by IQ class (e.g., Higgins et al. 1963), are necessarily of secondary interest. If there is no reason to expect A to cause B, then we do not need to explain why A does not cause B.

SUMMARY

Concern that IQ levels in Western countries will decline because groups with lower scores produce more offspring than groups with higher scores dates back more than a century. It has been empirically demonstrated that intelligence levels have risen rather than fallen (Flynn 1984) despite widespread lower fertility rates among high IQ groups (Van Court and Bean 1985). The expectation that fertility differentials would lead to a decline in intelligence scores appears largely to be based upon a fallacious analogy to closed subpopulations. We have shown that, once mobility between parent and child scores is admitted into a simple model of IQ inheritance, equilibria are established in the population's IQ distribution even with persistent fertility differentials.

As intuition suggests, the equilibrium distribution of IQ will be affected by differential fertility. We have shown that fertility differentials favoring low IQ groups will generally produce a lower equilibrium mean IQ than fertility differentials favoring high IQ. The differences are not large empirically and appear to be smaller when intermarriage between IQ classes is more frequent and when the variance introduced into the IQ inheritance matrix is greater. It should be noted that higher fertility among a group in a one-sex model does not always increase that group's representation in the equilibrium distribution because the offspring of that group may experience exceptionally high mobility (Chu 1987).

We have found one theoretical instance in which fertility differentials in one generation could have a much larger effect on future distributions: when fertility rates are highest at the extremes of IQ, multiple equilibria can result, so that one generation's behavior could push the system toward one of two very different outcomes. Such a fertility regime is empirically rare, and when we impose on it an empirically observed IQ inheritance matrix we are unable to demonstrate the existence of multiple equilibria.

Conventional wisdom breaks down altogether in the translation of negative fertility differentials by IQ into implications about trends. It is entirely possible for there to be an upward trend in mean IQ in spite of fertility differentials favoring low IQ, or a downward trend in spite of fertility differentials favoring high IQ. There can be no direct implications for trends until one compares the present distribution to the equilib-

rium distribution implied by M , R , and H . Microlevel studies reporting negative fertility differentials by IQ in one generation have no implications for IQ trends until they are placed within a population model in which relations across generations can be explored at the macro level.

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Comment on Preston and Campbell's "Differential Fertility and the Distribution of Traits"¹

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Samuel Preston and Cameron Campbell have made a strong contribution to the theory of population changes. What is especially exemplary in their paper is that they show some of the complexities in inferring macrolevel trends from microlevel processes. They model the combined processes of assortative mating, differential fertility, and heritability of IQ to show that a common belief—"that higher reproduction rates by those who score lower on a particular measurable trait will, *ceteris paribus*, lead to a reduction in the mean value of that trait from one generation to the next"—is "often not correct." A statement of this common belief is given by Reed and Reed (1965, p. 64), whose data they use: "There have been many fears and numerous predictions that our national intelligence level is declining because the larger the family size, the lower the average intelligence of the children in it." Reed and Reed point out that the predicted decline has not materialized and explain the apparent paradox by the evidence from their data showing that low IQ persons are less likely to marry and to have any children at all.

Preston and Campbell explain this same apparent paradox (the "Cattell paradox") by the results of their modeling, that is, that "negative reproductive differentials are perfectly compatible with a constant, improving, or deteriorating IQ distribution in the population."

Here, despite my admiration for their general approach, I want to show that their application of this fact to changes in the IQ distribution depends on two assumptions that seem unlikely to be true. If either assumption fails to be met, then their dismissal of the "common belief" is not well founded in the case of IQ.

¹ I am indebted to Kazuo Yamaguchi for comments. Correspondence may be directed to James S. Coleman, Department of Sociology, Social Science Research Building, 1126 E. 59th Street, Chicago, Illinois 60637.

THE GENERATIONAL TRANSMISSION MODEL

Preston and Campbell first demonstrate their major argument by use of a simplified model in which all marriages are within the same IQ class. This allows reducing the $1 \times n^2$ (where n is the number of IQ classes) marriage matrix to a $1 \times n$ matrix and permits writing the movement from the t generation to the $t + 1$ generation as

$$M^t RHN^{**} = M^{t+1}, \quad (1)$$

where M^t is a $1 \times n$ vector of the number of females in each class in the t generation or, equivalently, the number of couples in each class; R is an $n \times n$ diagonal reproduction matrix, showing the mean number of surviving offspring per couple in each class; H is an $n \times n$ heritability matrix with elements h_{ij} , showing the proportion of children of class i marriages that are in class j ; N^{**} is an $n \times n$ diagonal matrix in which each diagonal element is $1/2$. (My notation is the same as that of Preston and Campbell in their equation on p. 1005 except that the diagonal matrix N^{**} with elements $n_{ii} = 1/2$ replaces the product of their N^* , an identity matrix, and the scalar $1/2$.)

By assuming constant R and constant H over generations (which implies that a couple's occupancy in class i gives all the information necessary to know the probability distribution of their family size and the probability distribution of IQ destinations of their children) equation (1) becomes similar to a first-order Markov process modified only by an overall growth rate, which allows the sum of elements of M^{t+1} to differ from the sum of the elements of M^t . The matrix RHN^{**} is, so long as each IQ class is reachable from each other class, what is termed in demography a primitive matrix. As Preston and Campbell point out, a well-known result in demography is that, in the case of complete endogamy, the primitivity of RHN^{**} is a sufficient condition for there to be a unique equilibrium distribution of the population *independent of the starting distribution*, M^0 . They show an empirical example of this in their figures 1 and 2, which use two different reproduction matrices R and lead to two different equilibria. As the graphs show and as the authors point out, the reproduction matrix with a negative reproduction differential leads to an equilibrium distribution with a lower mean IQ than does the R matrix with a positive reproduction differential. But—and here is the central point of their article—this equilibrium distribution, though dependent on R , is not dependent on the starting IQ distribution M^0 ; and if that distribution is sufficiently skewed downward as in their examples, the average IQ will increase, even in the face of the negatively skewed reproduction matrix.

The remainder of their article elaborates on this, showing that the result holds for random mating combined with reasonable assumptions about R and H , including the empirical H matrix from Reed and Reed (1965) (though not with a very unlikely R matrix or a highly restrictive nonprimitive H matrix). In addition they show results for a time-varying H matrix, differing from the Reed and Reed matrix by giving a tighter or looser distribution of child's IQ around that of parents. But the result stated above, without elaborations, is the central result of Preston and Campbell's article, and this result is sufficient for my comments.²

For each of two reasons, I will argue, they are wrong to dismiss, for the case of IQ, the common belief "that higher reproduction rates by those who score lower on a particular measurable trait will, *ceteris paribus*, lead to a reduction in the mean value of that trait from one generation to the next" (p. 997). My difference with Preston and Campbell depends on two of their assumptions, either of which, if not correct, would invalidate their application of this to IQ.

THE IMPORTANCE OF HISTORY

The first assumption is specific to IQ, and concerns the relation between current fertility differentials by IQ and earlier fertility differentials by IQ. In their figures 1 and 2, in which they use a very negatively skewed initial distribution, M^0 , this initial IQ distribution is lower than the equilibrium distribution,³ both for a positively skewed reproduction matrix R and for one skewed equally in the negative direction. The latter illustrates the main point of their article, that is, that a negative relation between IQ and fertility is, *ceteris paribus*, compatible with a constant or rising IQ distribution. However, if this is to be true, it implies that the current distribution is the same as or lower than the equilibrium distribution compatible with the current R and H . But then how could the current distribution get at or below its equilibrium? This could occur only by an earlier RH matrix compatible with the current distribution or a lower equilibrium. This implies that the earlier R was equally or more negatively skewed than at present.

There is evidence on earlier fertility distributions, but it points in exactly the opposite direction. Becker (1991, p. 144) reviews this evidence,

² A more straightforward way of showing the dependence of the equilibrium distribution on fertility differentials would have been to begin not with a highly skewed distribution but with a distribution that is the equilibrium for the given heritability matrix and fertility unrelated to IQ.

³ When I write that a distribution is "higher" or "lower" than another, this should be taken as shorthand for "has a higher mean" or "has a lower mean."

ranging from 15th-century Tuscany to 19th-century Canada, United States, and Germany. He concludes, "Wealthier men also tended to have more children in monogamous societies prior to the nineteenth century. . . . Sometime during the nineteenth century, however, fertility and wealth became partially or wholly negatively related among urban families. . . . The evidence for advanced countries during the twentieth century has been rather mixed, although income and fertility have generally been negatively related at lower income levels and unrelated or positively related at upper levels" (see Becker [p. 144] for references to primary sources).

Income is, of course, not the same as IQ, but it is a reasonable proxy for IQ in historical data. As Becker indicates, the relation between income and fertility was positive until the 19th or 20th century. Assuming from this that the earlier relation between IQ and fertility was positive, it would mean that the shift to a negative relation has shifted the equilibrium IQ downward, to be compatible with this newly negative relation between IQ and fertility. This would place it *below* the existing IQ distribution (assuming the existing IQ distribution was at or near the equilibrium compatible with the earlier IQ-fertility relation), which is exactly the *opposite* of what is necessary for Preston and Campbell's main point to be true. The historical conditions that would be necessary for the current negative IQ-fertility relation to be compatible with a constant or rising IQ distribution are not present.

But there does remain the Cattell paradox for IQ, the simultaneous existence of a negative reproduction differential and slight intergenerational increases in M . If it is not dismissible in the manner proposed by Preston and Campbell, then what accounts for it? I will return to this question at the end of my comments.

DIFFERING HERITABILITY MATRICES

The second assumption of Preston and Campbell which seems unlikely to be true is not specific to IQ, but if not true invalidates more generally their dismissal of the common belief that negatively skewed reproduction should usually lead to lower means for the trait across generations. If the assumptions of the model proposed by Preston and Campbell are in fact met, this means not only that each couple is characterized by its state, which is IQ class ij , but also that each couple is characterized by the same probability distribution for number of children as a function of the parents' IQ class, that is, the same R , and that each couple is characterized by the same probability distribution of child's IQ as a function of parents' IQ class, that is, the same H . There is no unobserved character-

istic which leads persons in the same IQ class to have different R or H matrices.

What is most serious for their conclusions is the assumption that the heritability matrix is alike for all, independent of the couples' IQ state and independent of any unobserved characteristics. If this assumption were not met, the observed heritability matrix would be a mix of heritability matrices, which would not allow predicting the IQ distribution of future generations solely on the basis of the vector of parents' current states. If the heritability matrix were nevertheless used to do so, as Preston and Campbell do in the case of IQ, and if couples with higher IQs had heritability matrices that led to higher mean IQs than did those with lower IQs, then, in the presence of negatively skewed reproduction, their analysis would understate, perhaps seriously, the decline in average IQ. There would be a decline over generations *not only in the observed IQ distribution, but also in the equilibrium distribution that the current distribution approaches.*

In the application of Markov chain models to behavior of individuals, it has long been observed that, generally, individuals in the same current state do not have the same implicit transition matrix. The first models making use of the possibility that different persons had different transition probability matrices were occupational mobility models termed "mover-stayer" models. Instead of assuming that all had the same transition matrix, it was allowed that among those persons observed to be in the same state at times t and $t + 1$, there could be two kinds of persons, "movers" and "stayers." By observing the same persons again at time $t + 2$, it was possible to estimate the proportion of movers and stayers in each observed class (Blumen, Kogan, and McCarthy 1955). This work was extended by Goodman (1961), McFarland (1970), Morrison, Massy, and Silverman (1971), Spilerman (1972), and Bartholomew (1973). Spilerman's generalization of the mover-stayer model allowed not two classes but individual rates of movement distributed according to a gamma distribution. The mover-stayer model has also been applied to event-history models by Yamaguchi (1992) to deal with unobserved heterogeneity.

The assumption that persons have the same implicit transition matrix when they in fact have different ones does not lead simply to random errors of prediction, but to quite systematic ones. In particular, if a transition matrix X_1 , consisting of movement between unordered classes, is estimated using data from time t and $t + 1$ and then used to predict the transition from time t to $t + 2$ as X_1^2 , then there is, systematically, an underprediction of the main diagonal compared to the empirically observed matrix (X_2) over the two periods (see Blumen et al. [1955] and Coleman [1964, chap. 1] for discussion). For ordered classes like IQ classes, in which there is regression toward the mean, the same incorrect

assumption leads to underprediction of the number at the extremes over two periods. For example, the Preston and Campbell model would predict the same average IQ for children from parents with medium IQ and grandparents of high IQ as for children from parents with medium IQ and grandparents of low IQ. Unless IQ is different from the phenomena previously studied in this way, the prediction would be wrong and would underpredict the IQ of children from high IQ grandparents and overpredict that for children from low IQ grandparents. The reason, of course, is that the medium IQ parents whose own parents were low IQ are more likely to carry with them an implicit heritability matrix with a low mean toward which their children may be expected to regress. This has even more serious consequences when coupled with differential fertility by IQ. Any heterogeneity, coupled with differential fertility, shifts the equilibrium distribution toward which the next generation's IQs move, and shifts it in the direction of the high-fertility couples' heritability matrices. It is this which directly affects Preston and Campbell's substantive conclusions. All this gets complicated, so it is useful to express it mathematically and then to show how it works out in a simple hypothetical example.

The difference between the predictions made by assuming a common heritability matrix for all, when in fact there are different matrices, can be seen by comparing the first-generation predictions and the predictions s generations into the future. As in Preston and Campbell's equation (2) and my equation (1), complete endogamy is assumed.

Assume there are three types, a , b , and c , with different heritability matrices, H_a , H_b , H_c . Complete endogamy of heritability types is also assumed. Using a single heritability matrix, which combines the three by averaging,

$$H^* = (N_a^0 H_a + N_b^0 H_b + N_c^0 H_c) \left(\frac{1}{N_a^0 + N_b^0 + N_c^0} \right), \quad (2)$$

where H^* is the mixture of heritability matrices, N_i^0 is the number of persons of heritability type i at time 0, for $i = a, b$, or c , and $M^0 = M_a^0 + M_b^0 + M_c^0$, where M_i^0 is a vector of the number of persons of high, medium, and low IQ in heritability type i at time 0, for $i = a, b$, or c . Then

$$M^1 = \frac{M^0 R H^*}{2}, \quad (3)$$

and

$$M^{t+s} = M^t \left(\frac{R H^*}{2} \right)^s. \quad (4)$$

Using the three heritability matrices,

$$M^1 = M_a^1 + M_b^1 + M_c^1, \quad (5)$$

where

$$\begin{aligned} M_a^1 &= \frac{M_a^0 RH_a}{2}, \\ M_b^1 &= \frac{M_b^0 RH_b}{2}, \\ M_c^1 &= \frac{M_c^0 RH_c}{2}, \\ M^{t+s} &= M_a^{t+s} + M_b^{t+s} + M_c^{t+s}, \end{aligned} \quad (6)$$

where

$$M_i^{t+s} = M_i^t \left(\frac{RH_i}{2} \right)^s, \quad (7)$$

for $i = a, b, c$.

AN EXAMPLE

Suppose we have phenotypically three IQ levels: high, medium, and low. Suppose also that genotypically there are three types of persons: those with heritability matrices skewed toward a high mean, those with heritability matrices skewed toward a low mean, and those with nonskewed heritability matrices. Furthermore for simplicity assume, as do Preston and Campbell in their first analysis (their figs. 1 and 2), that mating is endogamous with respect to IQ and that, again for simplicity, mating is endogamous with respect to the three heritability types. These simplifications make it possible to characterize couples according to the three IQ classes (high, medium, and low) and according to these heritability matrices, type *a*, type *b*, and type *c*. Heritability matrices satisfying these conditions are given in tables 1A–1C.

Suppose that the initial state vector for each of the three types is of unit size and is the equilibrium vector for the case of constant fertility across IQ classes. This would give

$$\begin{aligned} M_a^0 &= .580 \quad .319 \quad .101 \\ M_b^0 &= .214 \quad .571 \quad .214 \\ M_c^0 &= .101 \quad .319 \quad .580 \end{aligned}$$

TABLE 1A

TRANSITION-PROBABILITY MATRIX FOR HERITABILITY TYPE A

| PARENTS' IQ | CHILD'S IQ | | |
|--------------|------------|--------|-----|
| | High | Medium | Low |
| High | .8 | .15 | .05 |
| Medium | .3 | .6 | .1 |
| Low | .2 | .4 | .4 |

TABLE 1B

TRANSITION-PROBABILITY MATRIX FOR HERITABILITY TYPE B

| PARENTS' IQ | CHILD'S IQ | | |
|--------------|------------|--------|-----|
| | High | Medium | Low |
| High | .5 | .4 | .1 |
| Medium | .15 | .7 | .15 |
| Low | .1 | .4 | .5 |

TABLE 1C

TRANSITION-PROBABILITY MATRIX FOR HERITABILITY TYPE C

| PARENTS' IQ | CHILD'S IQ | | |
|--------------|------------|--------|-----|
| | High | Medium | Low |
| High | .4 | .4 | .2 |
| Medium | .1 | .6 | .3 |
| Low | .05 | .15 | .8 |

The distribution into the IQ classes for the total population is simply the sum of these distributions. The proportion in each class at the starting point is

$$M^0 = .298 \quad .403 \quad .298$$

If the reproduction matrix R is equal to replacement and the same for all IQ classes (that is, 2 in each element of the main diagonal), then the proportion (and the numbers) in each class would remain the same, since each type is already at its equilibrium. Thus $M_i^c = M_i^0$ for $i = a, b$, and c , with the result that the proportion in each class would remain at .298, .403, .298. However, if instead the three types were treated as if they had

identical heritability matrices, as in Preston and Campbell's treatment, a different H^* would be found, by averaging H_1 , H_2 , and H_3 , as in equation (2). The value of H^* , from table 1 and equation (2) is

$$H^* = \begin{matrix} .567 & .317 & .117 \\ .183 & .633 & .183 \\ .117 & .317 & .567 \end{matrix}$$

This would result in a predicted equilibrium distribution $M^e = .268, .463, .268$. As in the earlier discussion, the assumption of a common heritability matrix for all underpredicts the extremes (.268 compared to .298) and overpredicts the number toward the middle of the distribution (.463 to .403).

What is of interest and relevant to Preston and Campbell's major point is the result that occurs when reproduction is negatively skewed. I assume a reproduction matrix similar to that assumed by Preston and Campbell in their empirical example of complete endogamy:⁴

$$R = \begin{matrix} 1.5 & 0 & 0 \\ 0 & 2.0 & 0 \\ 0 & 0 & 2.5 \end{matrix}$$

With this reproduction matrix and with use of equations (5), (6), and (7), the mean IQ (where high = 120, medium = 100, low = 80) follows the pattern over generations shown as the solid line in figure 1. Note the decline in mean IQ especially in the later generations as the fraction of type *c* persons in the population increases. On the chart also is a broken line showing the prediction that would be made from a single heritability matrix H^* , a prediction that would be grossly in error if the population consisted of the three endogamous types.

It is important to point out that the decline shown by the solid line in figure 1 is greater than would be the case if there were intermarriage between the heritability types.⁵ We know that in the case of differential fertility and no intermarriage between heritability types, the equilibrium

⁴ The reproduction diagonal matrix, with elements representing the number of surviving offspring per couple in IQ classes *i* and *j*, can be considered either to vary according to IQ class, as do Preston and Campbell, or to vary according to heritability type (*a*, *b*, or *c*). Since there is no reason to assume that family size is inherited, I will assume, with Preston and Campbell, that *R* is specific to IQ class. There is evidence, of course, that family size does depend on earlier family size; but Preston and Campbell neglect this, and I will as well.

⁵ However, as Kazuo Yamaguchi has pointed out to me, in the comparison of random mating and endogamy carried out by Preston and Campbell, the difference is less than that predicted in their fig. 3 insofar as fathers have stronger effects on children's IQs in some couples, while in others, mothers have stronger effects.

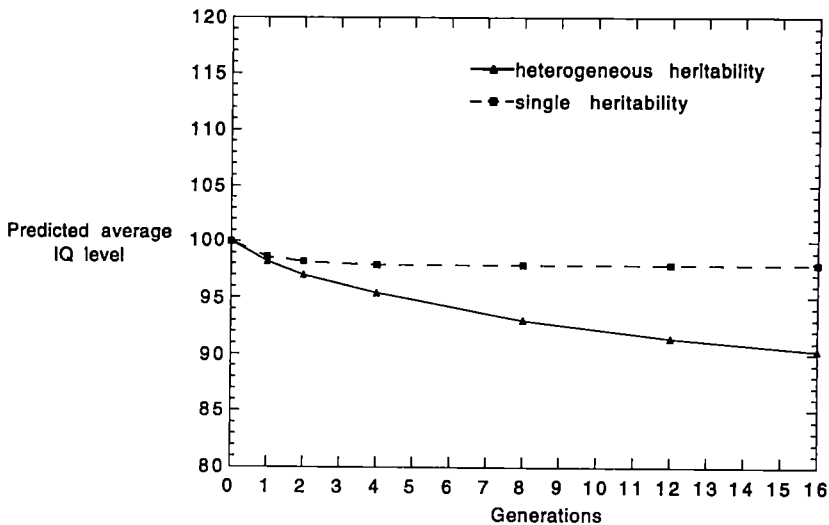


FIG. 1.—Predicted IQ levels with heterogeneous heritability and single homogeneous heritability.

which the population approaches is that consistent with R and H_c , that is, $M_c^e = .080, .269, .651$. This contrasts with that predicted by Preston and Campbell's model ($M^e = .218, .458, .324$). If there were intermarriage between heritability types, then, assuming that the offspring had heritability matrices that were mixtures of mother's and father's, the differences between single heritability and heterogeneous heritability would be reduced.⁶ The analysis would be more complicated, especially if there were also intermarriage among IQ classes, since parents could be of different heritability types, requiring an algorithm for transmission of heritability type, and of different IQs, requiring a reproduction matrix with dimension $n^2 \times n^2$, and heritability matrices with dimension $n^2 \times n$. But without going into that complexity, it is clear that intermarriage would reduce the differences. However, only with complete exogamy of heritability types a and c , each marrying the other in the first generation and each child having a heritability matrix that was the average of the parents', would anything like the homogeneous heritability assumed by

⁶ I have carried out calculations involving some intermarriage among heritability types, but the results are not reported here. A qualitative result is that unless the fertility differential by IQ is small relative to the intermarriage rate, the growth of the heritability type with a high proportion of high-fertility IQs will overwhelm the effect of intermarriage.

Preston and Campbell result. Such exogamy, of course, is unheard of in modern society.⁷

Another way of stating the overall result is that any heterogeneity in heritability matrices in the population, coupled with higher fertility among lower IQ parents, will lead to an over-generation degradation of the average heritability matrix and thus to a degradation in the equilibrium IQ distribution to which the actual distribution approaches. Therefore, use of an empirically estimated heritability matrix to infer the equilibrium IQ distribution is not warranted unless it can be shown (e.g., through use of three-generational data) that the assumption of homogeneity of matrices across the population is warranted. It seems extremely unlikely that this can be shown to be true.

A second point about the prediction that would result from using the Preston-Campbell method is not directly related to their argument, but shows the potential error of assuming a single heritability matrix when there is unobserved heterogeneity. The prediction of the population growth could be an extreme underestimate. The reason for this underestimate is that, with the assumption of a single heritability matrix, the growth of the low IQ segment of the population is greatly underestimated, as shown in figure 2, and this is the segment of the population that has highest fertility.

CONCLUSION

After all this, what is the import of this comment for "the common belief" that Preston and Campbell refer to, and what is its import for the conclusions of Preston and Campbell?

1. Preston and Campbell make one implicit assumption that invalidates their dismissal of the "common belief" that a negative relation between IQ and fertility will reduce further IQ levels. The implicit assumption is that the negative relation between IQ and fertility has been at least as great in the past as in the present. Evidence, however, points in the opposite direction, toward a positive relation before the 19th century.

⁷ One element of a two-sex population model that is omitted in the Preston and Campbell article, and in demography generally, is a model of an assortative mating process by which the distribution of children on some characteristic (IQ in their model) is transformed in a bivariate distribution of couples on this characteristic. It is possible to specify such a process in a matching model in which matches are determined by the interests on both sides in being matched with persons in particular classes. Interests are estimated on the basis of existing matches in one generation and then applied to subsequent generations, which may be differently distributed on the characteristic in question. See Coleman (1992).

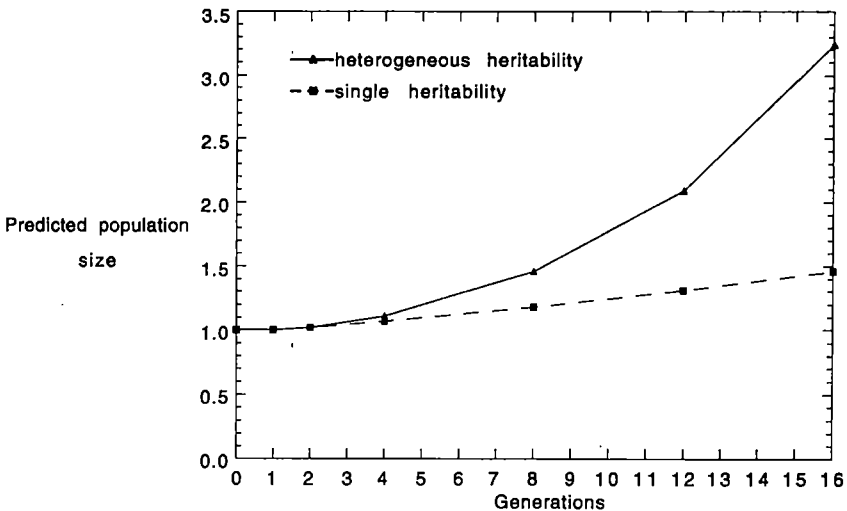


FIG. 2.—Predicted population size with heterogeneous heritability and single homogeneous heritability.

2. Preston and Campbell are certainly correct that the trait distribution will not move to that of the most prolific subgroup (where by subgroup I mean either heritability type or IQ class) insofar as there is intermarriage between subgroups. Their article shows the fallacy of this assumption. However, in doing so, they make an assumption that leads them to seriously underestimate the effect of a negative association between IQ and fertility.

3. Unless all persons are characterized by the same heritability matrix, which can hardly be the case, Preston and Campbell are incorrect in stating that the distribution will move to that consistent with the current fertility matrix and the current heritability matrix (e.g., the Reed and Reed matrix they use). It will instead move to a heritability matrix more like that of the subgroup with the highest fertility. If this subgroup has a heritability matrix that is consistent with a lower mean value of the trait than the current heritability matrix, then there will be a long-term decline in the value of the trait, below that predicted by the current mixture of heritability matrices.

4. The greater the endogamy within heritability types, the more the long-term distribution of the trait value will approximate that of the subgroup with highest fertility.

5. When there are panel data on heritability of a trait over three generations (rather than two, as in the Reed and Reed matrix), it will be possible to infer something about the distribution of heritability matrices in the population. Until then, and until we know the degree of endogamy

within heritability types, inferences about multigenerational trends in the trait should be avoided.

Coming back to the Cattell paradox, what is it then, in view of the two problems I have outlined with the Preston-Campbell explanation, that has prevented decline in average IQ and even led to slight increases? Reed and Reed point to the low fertility of very low IQ persons. I suspect, however, that the principal reason lies in the dependence of IQ on social environment, coupled with a social environment that has become more intellectually stimulating. (In the Reed and Reed data that Preston and Campbell use, the IQ of children is, on average, four points higher than that of their parents.) This conjecture, however, is not a substitute for research.

Altogether, Preston and Campbell have rendered a service by applying mathematical demography to questions of the changing distribution of traits in the population; but the points I raise here indicate that their rejection of the common belief about the effect of fertility differences on IQ is not warranted. What they have done is not to answer the questions involved, but to frame the problem in a most useful way.

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Comment on Preston and Campbell's "Differential Fertility and the Distribution of Traits"¹

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Preston and Campbell's article makes an important point that has not been well understood among social scientists interested in the effects of differential fertility in a wide variety of areas. This point is that the existence of higher fertility among individuals with a particular characteristic does not necessarily imply that the prevalence of that characteristic in the population will increase over time. As long as there is even a small amount of intergroup mobility across generations, then, for an important class of cases there will be a steady-state distribution of traits in the population, that is, a distribution of traits that, once reached, will remain constant over time. Once that steady state is reached, there will be no tendency for the population to become increasingly represented by traits associated with high fertility. This important principle of population dynamics has been applied to a variety of social and economic issues in previous literature. It remains relatively ignored, however, and Preston and Campbell provide an important service in pointing it out in this paper for the case of IQ.

It is useful to consider the basic intuition behind this result, beginning with a simple Markov process that does not include differential fertility but that does include intergenerational transmission of traits. Consider this simple example: the children of farmers become farmers 40% of the time. The children of parents who are not farmers become farmers 20% of the time. These probabilities remain constant across generations, an essential assumption in this example and in models such as Preston and Campbell's. First assume that each couple has two children, so that the population size remains constant. Since the children of both farm and nonfarm parents are more likely to grow up to be nonfarmers than to be farmers, should the proportion of farmers in the population not decrease steadily over time? The answer for this simple type of Markov process is that regardless of the initial proportions of farmers and nonfarmers there is a steady-state proportion of farmers that will eventually be

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reached and that will remain constant as long as the transition probabilities remain constant. In this example, the steady state (given by the leading eigenvector of the transition matrix), has 25% of the population as farmers in each generation. Given a population of 1,000 people, 250 farmers and 750 nonfarmers, there will be 100 farmers descended from farmers and 150 farmers descended from nonfarmers in the next generation, exactly preserving the previous generation's distribution. Once this steady state is reached, each generation will look exactly like the one preceding it. Introducing differential fertility of farmers and nonfarmers complicates the traditional Markov transition problem but does not change the fundamental principle of convergence to a steady state. Suppose farmers continue to have two children per couple but nonfarmers have 10 children per couple. Now there is even more reason to suspect that farmers will disappear from the population, since they have much lower fertility than nonfarmers and are more likely to have their children grow up to be nonfarmers than farmers, and since only 20% of nonfarm children grow up to be farmers. But it is easy to verify that this world also converges to a steady state in which the proportion of farmers stabilizes and remains constant forever, in this case at about 21% of the population. The reason is fairly transparent in this simple example. Even if we eliminated all the farmers in one generation, 20% of nonfarm children would become farmers in the next generation. This intergenerational mobility across types, as long as it is nonzero, is the mechanism that guarantees convergence to a steady state in these types of models, with differential fertility only changing the specific values of that steady state.²

Preston and Campbell's first model, in which endogamous mating occurs within IQ categories, is essentially a more complicated version of this simple example. No matter what fertility levels we assign to IQ categories, and no matter how IQ is inherited across generations, as long as all children do not have the same IQ as their parents, a steady-state distribution of IQ will be reached in the population.³

The critical assumptions that drive this result have nothing to do with the particular relationship between IQ and fertility or the values in the IQ inheritability matrix. As Preston and Campbell emphasize, the critical

² Even more surprising in these models is that the relationship between fertility of a given trait and the prevalence of that trait in the steady state is not necessarily monotonic. As demonstrated in Lam (1986), Chu (1987), and Dietzenbacher (1989), it is possible for the fertility level of a given group to increase but for the proportion of that group in the steady-state population to decrease.

³ A sufficient (but not necessary) condition is that children from any parental IQ category have some positive probability of ending up in any IQ category themselves. The general condition for convergence is that the transition matrix is primitive, the formal definition of which is provided in Preston and Campbell's discussion above.

assumption is the Markovian assumption that the inheritability matrix remains constant over time. Whether this assumption is a reasonable one is, of course, debatable. To the extent that IQ is genetic it is possible to construct a genetic model in which the probability of low IQ parents having low IQ children changes in each generation. The distribution of genotypes among those with a given phenotype may in general change from generation to generation. This evolution of genotypes can, for example, cause a particular trait associated with lower fertility (or higher mortality) to eventually disappear from the population.

Even if the assumption that the transition matrix is constant over time is unrealistic, however, it does not mean that the convergence result it implies is incorrect. That is, the intuition that traits associated with high fertility will become increasingly prevalent in the population over time cannot be rescued simply by arguing that the Markovian assumption of a constant transmission matrix is incorrect. Only particular types of changes in the transition probabilities over time would cause a trait to become increasingly common over time. Roughly, the Markovian tendency for convergence must be overcome by some countervailing trend, such as an increasing probability that parents in some classes will produce children of the type associated with high fertility. Although genetic models can be constructed to produce this result, it is not clear that we should expect such a pattern in general or for the specific case of IQ.

Preston and Campbell demonstrate that convergence to a steady state can occur in models with nonconstant transition matrices by assuming one particular type of change across generations in the structure of the matrix. Their example, in which there is a random variance of IQ for offspring of parents in a given IQ category, may be restrictive, since it does not capture the kind of drift in the mean that might be produced by genetic models. Nonetheless, the example is a useful demonstration that simply removing the assumption of a constant transition matrix need not in and of itself eliminate the tendency for convergence to a steady-state distribution of traits in the presence of differential fertility. They do not get convergence to a single distribution in their example, since the underlying transition matrix is constantly changing, but they do generate a sequence of distributions over time that has no systematic drift toward the trait associated with highest fertility.

Another important contribution of Preston and Campbell's article is the introduction of assortative mating by IQ. One-sex models such as the simple farmer/nonfarmer example above clearly miss a critical element of intergenerational transmission of traits by failing to address marital sorting. Preston and Campbell's two-sex model with complete endogamy is, as they point out, really no different than a one-sex model, and they attempt to go beyond this by simulating several different types of marital

sorting by IQ. While it would be nice to have general theoretical results, their simulations demonstrate the possibility that the steady-state equilibrium distribution may not be unique when random marital sorting is included in the model. As the simulations demonstrate, the standard convergence results from one-sex models do not carry over to all possible models of assortative mating.⁴ Their simulations also demonstrate, however, that introducing random sorting need not cause the traditional convergence to a unique equilibrium to break down. Models that produce multiple equilibria are the least plausible of their assumed models of fertility and marital sorting. For most reasonable models, Preston and Campbell continue to generate well-behaved convergence to a unique steady state, with no tendency for IQ to change over time once that steady state is reached.

It is important to recognize that Preston and Campbell's results can be applied to a wide variety of social and economic characteristics. In previous work with one-sex models, Preston (1974) analyzed the relationship between differential fertility across occupation classes and the steady-state distribution of occupations. I have used a similar approach to analyze the issue of whether higher fertility among lower income groups will lead to increasing level of poverty and greater income inequality in the population (see Lam 1986). As in Preston and Campbell's analysis of IQ, the point of these models is to demonstrate that if the intergenerational transition probabilities remain constant, higher fertility among some particular socioeconomic group will not necessarily lead to an increased prevalence of that group in the population.

It is worth considering when these models that imply convergence to a steady state are applicable and when they are not. Do they, for example, apply to the commonsense notion that if Hispanics have higher fertility than non-Hispanics then the proportion of Hispanics in the population will increase over time (abstracting from other complications such as mortality and international migration)? What about a variable with no biologically hereditary component such as religious affiliation? If Mormons have higher fertility than other groups, will the proportion of Mormons increase in the population indefinitely, or will there be convergence to a steady-state proportion Mormon as in Preston and Campbell's IQ model? What about characteristics that might be considered entirely genetic? If parents with blue eyes have higher fertility than parents with brown eyes, will brown eyes eventually disappear from the population? Finally, what about a characteristic like IQ, the focus of Preston and

⁴ Pollak (1987, 1990) has demonstrated several important general results about convergence to steady states in two-sex models.

Campbell's current discussion, a characteristic whose genetic component is poorly understood and which is affected by nongenetic variables in ways that are also poorly understood.

The intergenerational dynamics driving these and many other questions about the relation between differential fertility and population characteristics vary in important ways for different questions. Without attempting a comprehensive discussion of the many possibilities, a discussion for which existing research provides only a limited foundation, a few simple extreme cases shed some light on the possibilities. Beginning with the simple case of closed subpopulations, the dynamics are quite simple and, as Preston and Campbell point out, give rise to the intuition that traits associated with high fertility will become increasingly prevalent in the population over time. If there were no intermarriage between Hispanics and non-Hispanics, for example, implying an intergenerational transition matrix for ethnicity with no intergroup mobility, then it would be true that higher fertility among Hispanics, given some constant fertility rates across ethnic groups, will lead to an increasing proportion of Hispanics in the population over time. The results for religious groups, on the other hand, might be more complicated and might fit into the ergodic structure of Preston and Campbell's IQ model. If there were a constant intergenerational transition matrix with nonzero entries for transitions from Mormon into non-Mormon and from non-Mormon into Mormon, then the population would in fact converge to a steady-state percentage Mormon, just as in the farmer example above. This would occur even if Mormons have persistently higher fertility than non-Mormons and even if only a small percentage of Mormon children become non-Mormons.

The difference between the Hispanic example and the Mormon example results from the different assumptions about the existence of intergenerational mobility across groups. We can also change the intergenerational dynamics if we change the assumption about whether this intergenerational mobility is constant. Preston and Campbell's model would appear to be the wrong model, for example, to examine whether differential fertility by a genetic trait such as eye color will lead to steady increases in the proportion of the population with that trait. The proportion of brown-eyed parents that have blue-eyed children depends on the proportion of brown-eyed parents who are heterozygotic, that is, the proportion carrying the recessive blue-eyed allele. This proportion will, in general, change across generations given the evolution of genotypes. The fundamental Markovian assumption of a constant intergenerational transition matrix that drives Preston and Campbell's IQ model is thus violated in a systematic way. In purely genetic models, then, it is possible

to return to the naive intuition that traits associated with higher fertility will become increasingly prevalent in the population over time, without the unrealistic assumption of closed subpopulations.

What can be said, then, about a characteristic like intelligence that, unlike the Mormon example, has some plausible genetic component, but that, unlike the eye color example, is likely to have a large nongenetic component as well. Intelligence also differs from blue eyes in that any genetic component that does exist is likely to be a complicated function of many more fundamental characteristics, with many possible models of genetic evolution over time. The essential question is whether it is realistic to model the transmission of IQ across generations by a matrix of constant probabilities. There are obviously many difficult issues involving questions such as the meaning of IQ as a measure and the arbitrary division into discrete IQ categories. The more fundamental question, however, is whether there is some process, genetic or otherwise, that causes the transmission of intelligence from parents to children to change systematically from one generation to the next, or whether, as in Preston and Campbell's model, the transmission process is constant across generations. If their assumption of a constant transition matrix is correct, then their conclusion—that IQ will not steadily decrease over time when lower IQ parents have higher fertility—is correct. This is an important point that needs to be understood by anyone working on the relationship between differential fertility and any population characteristic. It is also important to note that if Preston and Campbell's assumption is too strong, it does not follow that their convergence result is wrong. As they demonstrate with one particular example, it is possible to have a constantly changing transition matrix without having a continual drift toward lower IQ. Given our limited understanding about transmission of intelligence across generations, we have very little basis for knowing whether Preston and Campbell's answer is in fact the right one. Their article provides an important lesson, however: the casual intuition that traits associated with higher fertility will become increasingly prevalent over time may be wrong. Any attempt to draw inferences about the evolution of population characteristics over time requires serious modeling of the underlying dynamics of intergenerational transmission. Preston and Campbell's article is an excellent demonstration of this type of modeling, and I hope it will be used as an example by future researchers interested in the effects of differential fertility.

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REPLY TO COLEMAN AND LAM

Fears that "inferior" groups are out-reproducing superior groups have surfaced throughout the 20th century. High fertility rates among certain groups—Catholics, Democrats, Southern Europeans, welfare mothers, and people with low IQ scores—have raised the specter of national submergence in a demographic wave of undesirables. The understanding (usually implicit) is that the offspring of such individuals are more likely than the offspring of others to appear as adults in the "inferior" category themselves. A recent illustration of such fears is Richard Herrnstein's 1989 article in the *Atlantic Monthly* about fertility differentials by IQ. His final words are "We ought to bear in mind that in not too many generations differential fertility could swamp the effects of anything else we may do about our economic standing in the world" (p. 79). We believe that such fears are more widespread, and a more salient factor in public policy deliberations, than a survey of the professional literature would indicate.

Most social scientists, we suspect, accept the basic premise of the previous paragraph—above-average fertility in such groups will increase their prevalence in the population—but are either unconcerned about the implications of the statement or avoid the subject because they consider it objectionable. We question the basic premise. Even if fertility is higher in such groups, and even if children born among those groups are more likely to become members of the group as adults, the proportion of the population represented by the group may rise, fall, or remain constant. Under the simple model that we present, the proportion will eventually become constant.

A key assumption of our model is that the probability distribution of children's outcomes (e.g., IQ score) given parents' characteristics—the

H matrix—is constant over generations. Undoubtedly, this assumption will not be strictly accurate for many processes. Does this mean that the model has nothing to teach us? There are several responses.

1. The assumption of our model is undoubtedly more accurate than the model with which most people appear to approach the question; in the latter, children inherit the exact same characteristics as the parent, with no chance of intergenerational mobility.
2. Introducing random shocks into the H matrix does not alter the basic result: the population distribution tends toward an equilibrium.
3. For most processes, it would be difficult or impossible to say how the H matrix should be construed as changing. In the case of IQ scores, the subject of our article, it is almost certainly the case that environments are improving. Such improvements are perfectly compatible with a constant H matrix. Only if *rates* of improvement are systematically rising or falling would the assumption that H is constant (or changing randomly) be misleading. We know of no genetic or environmental reasons to expect systematic, directed changes in H . In their absence, the assumption of constancy seems a reasonable place to begin.
4. Without simplifying models, we cannot understand the “buzzing, blooming confusion” that confronts us. Words, symbols, and even numbers are all models of some underlying reality that the analyst is attempting to comprehend and describe. An analogous model in demography, the stable population model, has had an enormous payoff by demonstrating the implications for age structure and growth of a constant set of age-specific mortality and fertility rates, despite the fact that such rates are never constant in a strict sense. It shows what would happen if nothing changed, answering the same What if? question as a speedometer.

David Lam supplies exceptionally lucid illustration of how the model works and useful examples of when it may or may not apply. He concludes that it is least satisfactory for a purely genetic process such as eye color. We would only add that, in the case of eye color as a prototype of a one-locus—two-allele model, the H matrix is still constant when expressed in terms of genotypes. Hadelor and Liberman (1975) offer a detailed examination of how differential fertility combines with random mating acts to produce genetic equilibria in such traits. Because there are many zeros in Hadelor and Liberman’s H matrix (e.g., two AA parents cannot produce an aa offspring), there are many possible equilibria, some of which are unstable. The translation from genotype to phenotype is straightforward in this example (but, as Lam suggests above, the reverse translation is not).

James Coleman makes two basic points about our article. One implies that our intent is to reconcile two apparently conflicting data: negative

fertility differentials by IQ and rising IQ scores across generations in the United States. Coleman is correct that the model can account for such a paradox only if either the R matrix (reproductive differentials by IQ) or the H matrix is or has recently been changing. He introduces a stylized history of R matrices that suggests that reproductive differentials have become more negative over time. If so, the equilibrium distribution would be below the currently observed distribution, so that IQ scores (with constant H) would trend downward across generations. However, this version of R histories is inconsistent with more detailed accounts (Van Court and Bean 1985; Vining 1986). These show negative and trendless fertility differentials throughout the 20th century, except for attenuated or even positive differentials among parents of the baby boom. In this connection, it is important to note that the intergenerational transmission process that we describe is dominated by recent history. For example, if we begin with a plausible equilibrium distribution of IQ in the population (unlike the extreme distribution chosen for illustrative purposes in our discussion) and impose a sharp change in the R matrix, equilibrium (to three decimal places) is generally reestablished within three generations. Thus R matrices in the 18th and 19th centuries are essentially immaterial; the population rapidly "forgets its past."

It is important to recognize that our article is not an attempt to account for the Cattell paradox in U.S. data but to explain why, in general, the coexistence of negative reproductive differentials and rising IQ scores should not be considered paradoxical. There is no necessary or likely connection between IQ trends and the magnitude of reproductive differentials by IQ. Insofar as IQ trends are affected by reproductive differentials, it is changes in those differentials that matter. That is the basic point of our modeling exercise. If we had set out to account for observed data, we would have been more attentive to establishing realistic initial conditions and to integrating the actual history of reproductive differentials. Based on simulations that we have done (not presented), it seems unlikely that changes in R can account for very much about recent trends in IQ, which suggests that these trends have been driven by changes in H .

Coleman's second point is that population heterogeneity will invalidate our model. This assertion is not correct. Suppose, as Coleman does, that the population consists of subgroups defined by some variable other than IQ, which we will call variable 2. Each of these subgroups on variable 2 has its own H and R matrices, with complete marital endogamy on both traits. In order to introduce this added complexity into the model, it is only necessary to redefine the H and R matrices for the entire population to account for the fact that there are more subgroups. Operationally, the simplest way to do this would be to treat the first element of the

distribution-of-trait vector (q^t) as q_{11}^t , the second as q_{12}^t , and so on, where q_{ab} is the proportion of persons in IQ class a and variable 2 class b . Each two-variable subgroup has a place in this vector and corresponding places in the R and H matrices. Nothing in the mathematics of our article is changed by this modification. Equilibrium would be established under the same circumstances that we describe. In this case, it would be an equilibrium with respect to IQ, to the other trait, and to their joint distribution. In all of the examples of this more complex world that Coleman describes, an equilibrium is reestablished. It is not accurate to say that the equilibrium IQ distribution in such a population is itself declining; the equilibrium is entirely contained in the (expanded) H and R matrices and visible in the first generation to which they apply.

Coleman is certainly correct that the equilibrium distribution of IQ in this two-variable world would not necessarily, or in general, be the same as that in our one-variable world. In the examples he develops, the two-variable equilibrium distribution of IQ is lower than the one-variable distribution. But there is no reason to expect that this outcome would be generally observed. His examples produce this result because the subgroup on variable 2 with the highest overall fertility also has the lowest subgroup-specific equilibrium distribution of IQ. Such a pattern of correlations may be empirically observed in some application, but there is no reason to assume that it will be.¹ Likewise, there is no reason to suppose that the intermediate IQ cells will be relatively evacuated in a two-variable world compared to a one-variable world. For example, the subgroup on variable 2 with the highest equilibrium distribution in these cells may also have the highest fertility, in which case the equilibrium proportion in these cells would be larger in a two-variable world than in a one-variable world.

In short, neither Coleman's first nor his second point invalidates the model. Both, however, are useful in indicating the need to incorporate as much information as possible if the model is to be used as a device for interpreting or predicting trends in the characteristics of a particular

¹ Coleman seems to concede this point in item 3 of his conclusion, but it is inconsistent with his earlier statement that "heterogeneity in heritability matrices in the population, coupled with higher fertility among lower-IQ parents, will lead to an over-generation degradation of the average heritability matrix and thus to a degradation in the equilibrium IQ distribution to which the actual distribution approaches" (p. 1030 above). This statement would only be approximately accurate if the phrase "higher fertility among those variable-2 couples that have the lowest equilibrium IQ distribution" were substituted for "higher fertility among lower IQ parents." Even with this substitution, the sentence would contain the misleading implication that the equilibrium distribution is itself changing (see our previous paragraph).

real population. We are grateful to both commentators for their valuable elaborations.

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Work in Progress toward a New Paradigm for the Sociological Study of Religion in the United States¹

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This article reviews recent literature on U.S. religious institutions and argues that a new paradigm is emerging in that field, the crux of which is that organized religion thrives in the United States in an open market system, an observation anomalous to the older paradigm's monopoly concept. The article has six sections: first, a brief survey of the paradigm crisis; second, a development of the concept of an open market in the historiography and sociology of U.S. religion; third, fourth, and fifth, arguments that U.S. religious institutions are constitutively pluralistic, structurally adaptable, and empowering; sixth, a consideration of recent religious individualism in the light of the new paradigm. A conclusion sketches some research implications.

In every scientific venture, the thing that comes first is Vision. That is to say, before embarking upon analytic work of any kind, we must first single out the set of phenomena we wish to investigate, and acquire "intuitively" a preliminary notion of how they hang together or, in other words, of what appear from our standpoint to be their fundamental properties. This should be obvious. If it is not, this is only owing to the fact that in practice we mostly do not start from a vision of our own but from the work of our predecessors or from ideas that float in the public mind. [JOSEPH A. SCHUMPETER, *History of Economic Analysis*]

INTRODUCTION

The sociology of American religion is undergoing a period of ferment, interpreted herein as a paradigm shift in process. This article is at once a partial review of a vast, rapidly growing literature and an attempt at

¹ Research for this article began when I was a visiting member of the Institute for Advanced Study, Princeton, New Jersey. Earlier formulations were presented to audiences at the institute in October 1988, Princeton Theological Seminary in February

theoretical integration that draws tendentiously on certain strains within that literature. Thus the article is part of the very process it heralds.

The older paradigm—identified here with the early work of Peter Berger (1969, 1970)—is still cited by a great many researchers in the field and remains useful for understanding aspects of the phenomenology of religious life. However, those who use the older paradigm to interpret American religious organization—congregations, denominations, special purpose groups, and more—face increasing interpretive difficulties and decreasing rhetorical confidence. The newer paradigm—consciously under development by only a handful of independent investigators—stands a better chance of providing intellectual coherence to the field.

The newer paradigm stems not from the old one (Tschannen 1991), which was developed to account for the European experience, but from an entirely independent vision inspired by American history. Thus, rather than fully documenting the alleged deficiencies of the older paradigm (see Hadden 1987), this article will only briefly recount some recent developments in American religion that are anomalous from its perspective before turning to an exposition of the emerging new paradigm. Section I sketches the crisis in the old paradigm, and Section II presents the presuppositional key to the new paradigm, the idea that religious institutions in the United States operate within an open market. The balance of the article is a series of corollaries to this idea. Section III argues that institutional religion in the United States is constitutively pluralistic, Section IV that American religious institutions are structurally flexible, Section V that they can serve as vehicles of empowerment for minorities and otherwise subjugated people, and Section VI that recent individualistic tendencies in American religion are consistent with its history. A conclusion considers benefits to be gained from future research oriented to the new paradigm.

1989, a joint session of the American Sociological Association and the Association for the Sociology of Religion in San Francisco in August 1989, the Humanities Research Forum and the Office of Social Science Research of the University of Illinois at Chicago in January 1990, the Department of Sociology at Northwestern University in October 1991, and Swarthmore College in November 1991. I am indebted to members of these audiences for their reactions, to numerous colleagues for advice and commentary, and to the Institute for Advanced Study, the Rockefeller Foundation, the National Endowment for the Humanities, the University of Illinois at Chicago, and Northwestern University for support. Rather than implicate by name any of the many individuals who have commented on previous drafts and assisted with this one, I wish to express my deep appreciation to all of them, as well as to four referees for the *American Journal of Sociology*. Correspondence may be directed to R. Stephen Warner, Department of Sociology (M/C 312), University of Illinois at Chicago, Box 4348, Chicago, Illinois 60607-7140.

The focus throughout is sociological, on religion as an institutional sector (Friedland and Alford 1991) rather than a primarily cultural or psychological phenomenon, and comparative in conception, focusing on the distinctive parameters of religion in American society, rather than on the evolution of "religion" as a generic phenomenon. Unless otherwise indicated, "America" refers to the United States and, for stylistic convenience, "American" to things pertaining to the society, government, or people of the United States.

I. ANOMALIES AND CRISIS

In *Protestant, Catholic, Jew*, a product of the 1950s, Will Herberg (1960) influentially portrayed a society suffused by religion. At that time, when cultural elites took liberal Protestant hegemony for granted, Reinhold Niebuhr and Paul Tillich appeared on the cover of *Time*, and the claim of the National Council of Churches to represent nearly 40 million Americans still seemed credible (Herberg 1960, p. 134), it was not particularly striking that the majority of leaders of the civil rights movement were clergymen. But a generation later, when many educated Americans had come to believe that religion was inconsequential, the fact that the movement that had produced the most sweeping progressive social change in modern U.S. history was led by Protestant preachers (Morris 1984) struck many intellectuals as anomalous (Wills 1990).

The surprise that news of American religion has occasioned in the past 15 years—the incomprehension that met Jimmy Carter's confession as a born-again Christian; the embarrassment occasioned by Jesse Jackson's public prayers; the near panic that greeted the emergence of the New Religious Right; the incredulity met by regular reports that more than 90% of Americans believe in God and 70% in an afterlife, that nearly 90% report they pray and that the majority of those pray daily, that 70% claim church membership and 40% attend weekly (Gallup 1990; Davis and Smith 1991); the derision earned by Oral Roberts's reports of conversations with God; the patronizing response given to the National Conference of Catholic Bishops' pronouncements on peace and justice—testifies not so much to the state of American religion as to its misunderstanding by those who have been too deeply schooled in the conventional wisdom of social science. "The learned have their superstitions, prominent among them a belief that superstition is evaporating" (Wills 1990, p. 15).

This conventional social science wisdom is rooted in a paradigm that conceived religion, like politics, to be a property of the whole society, such that the institutionalized separation of state and church in modern society offered religion only two alternatives: either religious values would become increasingly generalized so that they could remain the

property of the whole, increasingly pluralistic, society, or, if they remained resolutely particularistic, they would devolve to an inconsequential private sphere. The former alternative was theorized by Talcott Parsons (1960, 1967, 1969); the latter by Peter Berger (1969). We shall see below that religion in the United States has typically expressed not the culture of the society as a whole but the subcultures of its many constituents; therefore, that it should not be thought of as either the Parsonian conscience of the whole or the Bergerian refuge of the periphery, but as the vital expression of groups.

For the older paradigm, insofar as religion had a place in the lives of conventional Americans, it merely supported, or only decorated, the status quo; insofar as religion was obstreperous, it was likely found only on the margins of society. Thus, writing on the basis of his observations at midcentury, Talcott Parsons (1960, 1967, 1969) proposed that religion in modern society, to the extent that it was viable, was likely to be ascriptive in recruitment, generalized in content, and consensual in appeal. Yet, by the 1980s, previously descriptive designations like "Christian" and "humanist" became divisive labels, and arguments over the particular statuses of Jesus and the Bible took on renewed urgency. On the one hand, nearly one out of 10 Americans no longer professed a religion at all (Gallup 1988, p. 47). But on the other hand proselytizers were busy: one out of three persons reported that, sometime during the year ending in March 1988, they had been invited to join someone else's church (Gallup 1990, p. 29).

Conversely, Peter Berger said in 1970, defiant assertions of supernaturalism were "likely to be restricted to smaller groups, typically those whose social location (in 'backward' regions, say, or in the lower classes) gives them little interest or stake in the world of modernity" (Berger 1970, p. 21), but less than a decade later Mary Jo Neitz (1990, p. 91) "met lawyers and business executives . . . speaking in tongues and practicing faith healing" at a huge Roman Catholic charismatic prayer group meeting every Monday night in an affluent Chicago suburb (see also Neitz 1987). Pentecostalism was no longer peculiar to the down-and-out: Gallup (1988, p. 56) estimated that 9% of all Americans, including 8% of all U.S. college graduates, took part in a charismatic group in 1986-88. A startling phenomenon to those who expect religion to be innocuous and conventional is an energetic new Christian denomination of 22,000 members, the Universal Fellowship of Metropolitan Community Churches (UFMCC), which ministers to gays and lesbians on the basis of an orthodox trinitarian theology with echoes of pentecostalism (Perry and Swicegood 1990, esp. chap. 4; Warner 1989; Jacquet and Jones 1991, p. 262). Alongside the UFMCC are gay and lesbian congregations standing on their own (Thumma 1991) and operating within conventional

Protestant denominations as well as in Catholic and Jewish communities (Gorman 1980). The sheer variety within American religion is staggering (Melton 1989).

Many scholars studying these developments—assertive particularism, resurgent traditionalism, creative innovation, and all-round vitality in American religion—have attempted to frame their reports using the “sacred canopy” perspective (Berger 1969), which is, after all, their disciplinary cultural capital. Yet inconclusive results have become chronic in the field (Ammerman 1987, esp. pp. 1–3; Christiano 1987, chap. 7; Davidman 1991, pp. 28–29, 203–4; Poloma 1989, p. 93; Prell 1989, pp. 161–65, 270). Others (e.g., Bender 1991; Finney 1991; Gilkes 1985; Gorman 1980; Kaufman 1991; Preston 1988; Rose 1987; Stacey 1990), especially feminist scholars, drawn to the field by interests more topical, moral, political, or personal than the theoretical interests defined by the sociology of religion, have produced reports unframed by either paradigm and thus subject to appropriation by both. That the reigning theory does not seem to work has become an open secret. Indeed, a sociological observer from abroad reports “the impression that, at least in the United States, it is the antiseccularization thesis that has become the accepted wisdom” (Sharot 1991, p. 271). “Antiseccularization,” however, is a mere negation; it is not yet a paradigm.

Advocates of the older paradigm have by no means retired from the scene and, indeed, have counterattacked (Lechner 1991; Tschannen 1991). Using the apparatus of secularization, they have attempted to account for apparent anomalies such as the resurgence of fundamentalism (Lechner 1985) and the persistence of evangelical (Hunter 1983) and liberal (Roof 1978) Protestantism. They have reformulated “secularization” to make it conform better to the American experience (Chaves 1991c). Yet much of secularization theory’s best evidence and most forceful advocacy comes from Europe, where secularization is arguably a historical fact as well as a theory (see Hadden 1987, pp. 589–91, 599).

The debate is unsatisfactory and will likely remain so until the opponents of the secularization paradigm develop their own paradigm. In the last few years, they have begun to do so.

II. THE CRUX OF THE MATTER: DISESTABLISHMENT AND RELIGIOUS MOBILIZATION IN THE UNITED STATES

The emerging paradigm begins with theoretical reflection on a fact of U.S. religious history highly inconvenient to secularization theory: the proportion of the population enrolled in churches grew hugely throughout the 19th century and the first half of the 20th century, which, by any

measure, were times of rapid modernization. Whereas about 10% of the population were church members at the time of the American Revolution, about 60% are so today, with particularly rapid growth registered in the 50 years preceding the Civil War and the Great Depression (Herberg 1960, pp. 47–50; Caplow, Bahr, and Chadwick 1983, pp. 28–29; Finke and Stark 1986; Stark and Finke 1988). One naive glance at the numbers is bound to give the impression that, in the experience of the United States, societal modernization went hand in hand with religious mobilization.² The end result is that, with the exception of “a few agrarian states such as Ireland and Poland,” “the United States has been the most God-believing and religion-adhering, fundamentalist, and religiously traditional country in Christendom” as well as “the most religiously fecund country” where “more new religions have been born . . . than in any other society” (Lipset 1991, p. 187).

In default of census data on individual religious affiliation, which the government may not inquire into, sociologists of religion employ what can be called “poll” data (sample surveys done by Gallup, NORC, etc.) and “roll” data (reports of internal counts by religious bodies themselves). Measured in terms of poll data, the current rate of adult church membership is between 69% (Gallup 1990, p. 43) and 61% (Davis and Smith 1991, p. 399); in roll data terms, the figure is 59% (Jacquet and Jones 1991, pp. 303, 265).³ While mainline Protestant denominations have lost members since peaking in the mid-1960s and individually reported church membership has declined four or five percentage points from the 73% registered in polls at that time, it stretches these points mightily to see this slight and uneven decline over three decades as evidence for secularization theory, in view of about 10 previous decades of strong and positive zero-order correlations between church membership

² Peter Berger has recently acknowledged (1986, pp. 226–27) that his early work erred in supposing that modernity in the United States must lead to an erosion of communal, including religious, life. But it is his earlier work that is still influential in the field.

³ It is noteworthy that reports of individual respondents (poll data) yield higher rates of church membership than ecclesiastical reports (roll data) for the high-status liberal Protestant denominations and lower rates for Roman Catholicism. Thus, many (and in the case of Presbyterians, most) of those who claim in sample surveys to be Protestant church members evidently do not comply with the denominations’ membership requirements (e.g., regular attendance and contributions) but instead evidently feel a residual or anticipatory identification with the church community as a reference group. Many of those who claim to be church members are thus said to be effectively “unchurched” (Gallup 1988), thereby explaining the 10% discrepancy between poll data church membership and the corresponding roll data figure. The implications of these discrepancies represent a research frontier in the field (see Marcum 1990; Roof and McKinney 1987, pp. 177–79; Hadaway and Marler 1991b).

and industrialization, urbanization, immigration, and most of the other processes that are thought to cause secularization.⁴

The research of historians helps us understand what is distinctive about American religious institutions (Hackett 1988). Despite the impression on the part of today's conservative Christians that the United States was founded as a Christian nation, the early decades of American independence were times of eclectic spiritual ferment but thinly distributed church membership. Jon Butler (1990), in *Awash in a Sea of Faith*, emphasizes widespread religious indifference in colonial New England and documents flourishing non-Christian strains of magic, astrology, and spiritualism in the antebellum years. In *The Democratization of American Christianity*, Nathan Hatch (1989) chronicles the strenuous efforts of early 19th-century revivalists to bring into religious fellowship the masses of common people who had been ignored by colonial religious establishments.

The great antebellum revivals, or the Second Great Awakening (ca. 1790–1830), have accordingly been interpreted by historians not as exercises in nostalgia but as strenuous, and largely successful, efforts at social organization (Mathews 1969; Smith 1968). “Worried about colonial spiritual lethargy since the seventeenth century, concerned about the rise of deism and skepticism among the political and social elite, propelled by a republican ideology to secure Christian foundations of American political virtue, and seeing in independence new opportunities to win adherents, religious leaders rushed to proselytize citizens in a growing nation” (Butler 1990, p. 274). The enormous growth of the Catholic church in the United States later in the 19th century was similarly not merely a matter of the importation of pre-existing religious commitments but the result of strenuous effort by revivalists worried about apostasy of spiritual kinsfolk in a dominantly Protestant culture (Dolan 1978; Finke 1988).

The analytic key to the new paradigm is the disestablishment of the churches and the rise of an open market for religion, the process that intervened between colonial lethargy and antebellum fervor. Establishment was not uniform across the colonies and disestablishment did not

⁴ In order to account for recent downturns in religion, some analysts seem to posit that modernity arrived in the United States only in the 1960s, which, in effect, abandons the secularization *paradigm* for a *finding* of “secularization” over a two-decade period (cf. Chaves 1991c, p. 502). Much more promising from the point of view of the new paradigm is to focus on such specific factors as governmental activity (Wuthnow and Nass 1988) and family formation (Chaves 1991c) as variables affecting church membership and attendance. Meanwhile, overall church attendance has been remarkably stable for the last two decades (Hout and Greeley 1987; Greeley 1989; Chaves 1989, 1991c; Firebaugh and Harley 1991) and specifically Protestant church attendance for the last half-century.

occur overnight in 1789, for the First Amendment prohibited only Congress (not the states) from establishing religion. Disestablishment was the fruit of an ironic alliance between deistic political elites and insurgent evangelical firebrands (Littel 1962; Mead 1963; Finke 1990), and it had two profound implications for the institutional order of religion: first, protection for the free exercise of religion in general, second, no protection for any religious organization in particular. For the people, there was freedom of worship. For the churches, it was sink or swim, and the market share of Congregationalists started quickly to sink, whereas the Baptists and Methodists swam expertly (Herberg 1960, pp. 103–7; Finke and Stark 1989a). The long-term result of disestablishment was a far higher level of religious mobilization than had existed before.

Thus, rather than viewing American religion as a mere exception to or negation of the pattern of European establishment, new paradigm sociologists have learned from historians to view U.S. religion as institutionally distinct and distinctively competitive. Table 1 dramatizes the difference between the new and old paradigms.

Economic imagery is widespread among students of American religion. The historian Butler writes of Baptists developing “national spiritual markets” and observes that denominational leaders on the postrevolutionary frontier “read maps with an intensity that challenged land speculators” (1990, pp. 275–76). The historian Terry Bilhartz (1986, p. 139) focused on “the marketing skill of . . . competing venders” in his monograph on religion in post-Revolutionary Baltimore, and he casually introduced “supply-side” imagery to the discussion. This imagery was later developed by Hatch into a theory of “competition in the religious marketplace” among spiritual “entrepreneurs” in a “divine economy” (1989, pp. 15, 67, 101). Anthropologists Irving Zaretsky and Mark Leone (1974, p. xxxvi) write that American religion “is the last voice for decentralization and the free enterprise system.” Sociologist Richard Lee (1992, p. 6) claims that “both religious and economic behavior are shaped by a common independent variable, reward,” and Andrew Greeley (1989, p. 122) agrees that “a ‘rational choice’ theory does much to explain the persistence of religion in the United States.” Rodney Stark and William Bainbridge (1985, 1987) developed an elaborate theory of “the religious economy,” and Finke and Stark (1992) have used the theory to account for 200 years of organizational success and failure of U.S. religious bodies. The economist Laurence Iannaccone found an early statement of the market theory of religion in Adam Smith’s *Wealth of Nations* and tested it with cross-national data. He found that, “among Protestants, at least, church attendance and religious belief both are greater in countries with numerous competing churches than in countries dominated by a single church” (Iannaccone 1991, p. 157).

TABLE 1
SCHEMATIC COMPARISON OF NEW AND OLD PARADIGMS

| | New | Old |
|------------------------------|--|---|
| Paradigmatic situation | Competition | Monopoly |
| Best historical fit | Second Great Awakening | Medieval Catholicism |
| Place and time | United States, early 19th century | Europe, 500–1500 C.E. |
| Master narrative | Revival and routinization | Linear secularization |
| Master process | Mobilization | Differentiation |
| Secularity threatens | Irksome demands | Implausible beliefs |
| Elite prototype | Entrepreneur | Prebendary |
| View of pluralism | Constitutive | Degenerative |
| Social base | Social groups | Whole society |
| Typical organization | Denomination, congregation | Universal church, parish |
| Function of religion | Solidarity, morale | Explanation, meaning |
| Identity | Contested | Taken-for-granted |
| Recruitment | Emergent, achieved | Primordial, ascribed |
| Today's figures | Stark, Finke, Greeley | Berger, Lechner, Hunter |
| Classic texts | "Protestant Sects" and <i>Elementary Forms</i> | <i>Protestant Ethic and Division of Labor</i> |

The new paradigm is not *defined* by economic imagery, however, but by the idea that disestablishment is the norm. For instance, Berger's statement that in contemporary America "religious institutions become marketing agencies and the religious traditions become consumer commodities" (Berger 1969, p. 138) is an often-cited observation (see also Hammond 1986). Still, Berger represents the older paradigm, dominated by the scenario of European secularization, not the emerging one, which is based on and specifically describes and explains the American experience. For Berger, the *modern* American market situation is a degenerate one of "loss," "rupture," "deprivation," "fragility," "tenuousness," and "crisis" (1969, chap. 6), whereas the paradigmatic, durable, *traditional* situation is one in which religions are "authoritatively imposed" as "monopolies" by the "coercive support" of the state (1969, pp. 138, 135, 131). According to Berger, the market situation of religion in modern society deprives religious institutions of the support of coerced monopoly; above all they lack the support of being "taken for granted" that arises in the absence of ideological competition.

Accordingly, when Bergerians are confronted with some old-fashioned religion thriving in modern America, they feel constrained to account for it as a kind of deviant case. Thus the analyst focuses on the surrogate supports, called "plausibility structures," that the group provides itself. Such framing of the research usually requires that the ethnographer concentrate on the sociological commonplace that members of a group surround themselves with like-minded others. The reader learns indeed how the group cognitively defends itself but, unless other theoretical sources are drawn upon, little of how it organizationally expresses itself (Wuthnow 1986).

Contrary to the sacred canopy concept, ethnographers (e.g., Snow and Machalek 1982) have argued that maintaining supernatural religious beliefs in U.S. society is not particularly difficult, and opinion pollers have found high levels of self-reported belief in God and devotional and attitudinal religiosity (praying daily, reading the Bible, etc.) among the "unchurched" (those who do not regularly participate in a religious body; see Gallup 1988; Davis and Smith 1991). This suggests that other factors than "implausibility" (e.g., anger at church pronouncements on birth control) are at work in whatever religious disaffection does exist in the United States (Greeley 1989).

Thus one result of the older paradigm was that relatively too much attention was paid to the question of maintaining deviant religious cognitions and too little to what the religion in question did for its adherents and they for it in the real world. The work of James Davison Hunter is a case in point. Hunter devoted his first two monographs (Hunter 1983, 1987) to the quandaries and compromises modern America poses for

conservative Christians, leaving for a third (Hunter 1991) the question of the cultural challenge that conservative Christians pose for modern America. Since Hunter is an intelligent, energetic, resourceful, and empirically responsible scholar, his work can be appropriated for the new paradigm despite his Bergerian presuppositions.

Another result of taking the monopolistic situation as the norm is an illusory focus on the shock posed to religious organizations by their adjustment to the circumstances of pluralism. It is said that churches can "*no longer take for granted*" the loyalty of clients. Their ideas, "*which previously could be authoritatively imposed,*" now must be marketed. In the pluralistic situation, religious activity "*comes to be dominated by the logic of market economics*" (Berger 1969, p. 138; emphasis added). Thus does Peter Berger account for the secularizing, homogenizing, psychologizing tendencies of religion in the contemporary United States, as if a few gigantic, previously privileged suppliers had been suddenly confronted a few decades ago by hordes of price-conscious consumers (see also Luckmann 1991, pp. 176–79). Yet it has been nearly two centuries since religion in the United States could be coercively imposed. Very few of the hundreds of religious organizations flourishing in the United States today—arguably only the Episcopal church (Swatos 1979)—have had to adjust to a pluralistic situation. Most of them were born into it.

The adjustment-to-pluralism model is not groundless. It matches the personal and familial experience of some groups from which religion scholars derive. Eastern European Jews came to the United States from encapsulated, religiously monolithic communities, and their offspring are aware of the contrast of traditional and modern worlds (Davidman 1991, pp. 34–37; Furman 1987, pp. 130–31). Bergerian theory narrates the psychological experience of intellectuals who emerge from religiously conservative families to the religiously indifferent world of the academy, where they learn that religion is socially constructed and that theirs is only one of many systems of meaning (Wacker 1984). People with such biographies have undergone psychologically the perforation of a sacred canopy that the old paradigm attributes sociologically to Western society as a whole. Bergerian theory can thus succeed as a phenomenology of religious lives where it fails as a theory of American religious organizations.

Contributors to the new paradigm have not reached consensus on all matters. Historians (Marsden 1990; Michaelson 1990) might well be surprised to hear the work of Jon Butler (1990) and Nathan Hatch (1989) attributed to the same "new paradigm," for Butler's long-standing focus on religious authoritarianism is known to contrast with Hatch's equally persistent stress on consumer sovereignty. Yet not only does each endorse

the other's book,⁵ both agree on the fundamental point that, however it is to be evaluated, the Christianization of the United States was neither a residue of Puritan hegemony nor a transplantation of a European sacred canopy but an accomplishment of 19th-century activists. This fundamental point is itself hardly new to historians and was central to those upon whom Herberg relied (e.g., Mead 1963). Indeed, more recent in religious historiography are challenges from European-oriented Marxian and Foucauldian perspectives. But Butler and Hatch join forces with the new paradigm sociologists in stressing the distinctive properties of U.S. religious institutions against such challenges.⁶

Those who use economic imagery do not agree on the full logic of market analysis as applied to religion or on all its empirical corollaries. For example, the rational consumer of religious commodities is as often imagined to be fickle as to be brand loyal. Analysts as different as Finke and Stark, Bellah and associates, and Berger look for signs of individual religious mobility as evidence of religious rationality, whereas Iannaccone, Greeley, and Lee model steadiness of religious identity as a means of reaping rewards of investments in religious cultural capital.⁷ On the organizational side, the free marketplace of religion is expected by Stark and Bainbridge (1985) to exhibit inexhaustible variety and by Iannaccone (1986) to offer sectarian as well as churchly alternatives, whereas Berger (1969, p. 148) expects to find "standardization and marginal differentiation." A paradigm is not yet a theory but a set of ideas that make some questions more obvious and urgent than others. Much remains to be specified.

In particular, debate has arisen in the literature over the effects of pluralism on religion. Finke and Stark, the most outspoken exponents of the new paradigm, assert categorically that "the more pluralism, the

⁵ Butler's "book is path-breaking and simply has no competitor in treating the full scope of religious history in early America" (Hatch on the dust jacket of Butler's book); Hatch's "deeply researched, superbly written book goes to the very heart of the American religious and cultural development" (Butler on the dust jacket of Hatch's book).

⁶ Butler (1990, pp. 297, 345) cites the work of Stark and Finke (1988; Finke and Stark 1986), and Hatch (1989, p. 298) cites that of Caplow (1985).

⁷ Nor do all theorists assume religious wants to be exogenous in the manner of pure microeconomics (cf. Friedland and Alford 1991, pp. 232–35). On the contrary, it seems to be a widespread notion among new paradigm exponents that Americans' high level of religious interest is due in part to the historic vigor of religious organizations in this country (Iannaccone 1991, pp. 161–62). A path-breaking article by Gary Becker and Kevin Murphy (1988, p. 675) begins with an epigram from Shakespeare, "Use doth breed a habit," and argues that addictions, which are the dependent variable, "require *interaction* between a person and a good" (p. 694; emphasis added).

greater the religious mobilization of the population" (1988, p. 43). The exchange began when Finke and Stark (1988) argued that they had successfully tested that claim using turn-of-the-century census data. These data had previously been analyzed by Kevin Christiano (1987), who had expected consistency with the older paradigm but was surprised by mixed results. Kevin Breault (1989*a*, 1989*b*) then defended the older paradigm's expectations with both a critique of Finke and Stark's methods and an analysis of another, contemporary, data set. More recently, Judith Blau, Kenneth Land, and their associates have entered the fray, testing elaborate models on data from 1910 to 1930 (Land, Deane, and Blau 1991) and from 1850 to 1930 (Blau, Redding, and Land 1991). They reject Finke and Stark's pluralism theory with the conclusion, "It may be true that America has exceptionally much religious diversity and also exceptionally high rates of religious membership, but the two are not causally related—at least not positively" (Blau et al. 1991, p. 36).

The debate on pluralism is unresolved and the issues are complex, having to do with measures of religious diversity, proper units of analysis, the adequacy of various data sets, the possibility that causal forces differ for Protestant and Catholic contexts and that causal relationships may have changed over time, and other matters. But resolution seems close on two issues. The older expectation (see Christiano 1987) that cities are necessarily inimical to religion has been refuted; indeed, U.S. cities and their modern economy evidently provide resources and conditions conducive to religious mobilization (Finke and Stark 1988; Blau et al. 1991; Olson 1993). Score one for the new paradigm.

On the other hand, Finke and Stark (1988, 1989*b*) have conceded what Christiano (1987) earlier found, that religious concentration, in the sense of numerical predominance of one denomination in a geographical area, does not necessarily militate against religious vitality, particularly if that denomination is Catholic. Iannaccone (1991, p. 171) reports that this theoretical complication was also recognized by Adam Smith for Catholic countries in the 18th century. If, as in the case of Ireland and Poland, the church is allied with a sense of submerged nationalism (Lipset 1991), or—as in the American instances of Utah Mormons, turn-of-the-century urban Catholics, and postbellum Southern Baptists—the geographically concentrated group perceives itself to be a minority surrounded by a hostile culture (Shipps 1985; Finke and Stark 1988, pp. 44–46; 1989*b*, pp. 1054–56; Wacker 1991), the regionally dominant church is less likely to lapse into the complacency that a protected position invites. Conversely, if, as in France, a monopolistic church is allied with the widely despised, losing side in a nationwide struggle, it will languish.

Finke and Stark, Iannaccone, and Blau and Land and their associates all employ a measure of monopoly/competition that is based on the rela-

tive homogeneity/diversity of religious affiliation within a given unit, but Iannaccone (1991) argues that neither models of pure monopoly (literally, a single supplier of religion) nor pure competition (countless small suppliers) is realistic in the discussion of religion. Instead, he suggests (Iannaccone 1991, pp. 160–63) that religious disestablishment should be measured by the relative prevalence of government subsidies to or interference in religion. Chaves and Cann (1992) take up his suggestion; measuring state regulation directly, they provide more evidence for the proposition that an open market is conducive to religious vitality.

Such is the contribution of Hatch's supply-side imagery: what is important about religious markets from this perspective is not so much the diversity of alternatives available to consumers as the incentive for suppliers to meet consumers' needs, which is maximized when the religious economy is wide open to energetic entrants, none of whom has a guaranteed income. Consider the phenomenally influential Oral Roberts, whose career is a key to Pentecostalism breaking out of its class-, race-, and region-based boundaries in the second half of this century. Roberts was ordained at age 18 as a preacher in the Pentecostal Holiness denomination, which, "like most new sects, had a vast oversupply of ministers" (Harrell 1985, p. 20). The ambitious Roberts soon outgrew his denomination and at age 30 invested \$60,000 in his own infrastructure: a truck-and-trailer rig, portable organ, piano, sound system, folding chairs, and a tent with room for 3,000. Four years later, he bought a tent big enough for 12,500 and soon began broadcasting (these biographical details come from Harrell [1985, esp. pp. 20–21, 51]). For Roberts and entrepreneurs like him, ordination was not a sinecure, but a license with a built-in incentive to reach out to new audiences through innovative means.

Accordingly, the concept of a competitive religious market entails neither that religious organizations pander to a lowest common denominator of spiritual commitment nor that religious consumers constantly compare competing suppliers' responses to their fixed demands (cf. Scherer 1988, p. 481; Wuthnow 1991, pp. 6–7). For example, evangelical Protestants are currently worried about the presumed mass defection of inner-city African-Americans to Islam (e.g., Guthrie 1991), and Catholic bishops are concerned about the reported defection of Hispanics to Pentecostalism (e.g., National Conference of Catholic Bishops 1984). No matter that, because the U.S. Census Bureau may not inquire into individuals' religion, hard data on such trends are lacking: widely repeated anecdotes and case studies help mobilize countermeasures that initially take the form of paying more attention to the client at risk, where the supplier changes the distribution of its effort, not its basic teaching (e.g., Fitzpatrick 1990). Thus, research indicates that recent denominational growth and decline patterns are largely explained by patterns of new

church plantings (foundings of new congregations, parishes, and missions; see Hadaway 1990; Marler and Hadaway 1992). Religious organizations cannot succeed in the market unless they bring their services to consumers, but the new paradigm does not claim that religious entrepreneurs are insincere about their product. Quite the contrary.

If the paradigmatic situation for Bergerians is the sacred canopy, the religious monopoly inaugurated in Europe by Constantine in the 4th century, then for market theorists it is the furious competition to evangelize North America in the 19th, the revivalism of the Second Great Awakening and later (Warner 1991). The competitive patterns that emerged two centuries ago in the United States constitute "an institutionally specific cultural system" (Friedland and Alford 1991, p. 234), which minimizes state interference in religion and permits adaptation to an always changing society.

III. THE MASTER FUNCTION OF RELIGION IN THE UNITED STATES: SOCIAL SPACE FOR CULTURAL PLURALISM

Unity is a normative ideal for Christian ethicists and an analytic presupposition for their old-paradigm cousins. But, from the beginning, religion in the United States has been associated with societal differentiation, and pluralism has tended in this society to take on a religious expression. During colonial times, New England was the stronghold of Congregationalism; New York, New Jersey, Pennsylvania, and Delaware of the Presbyterian and Reformed churches; and Virginia of Anglicanism. Religion and region are associated to this day (Stump 1984; Hill 1985). But already by the time of the American Revolution, the Anglicans were relatively stronger on the coast and the Baptists in the hinterland, and another axis of variation—urban-rural—came into play, with a distinct social class component soon overlaid upon it as the Methodists and Baptists swept across the frontiers of upstate New York, the Ohio River Valley, and the Appalachian Piedmont. At the present time, Episcopalians, the successors to the Anglicans, still outrank Baptists and Methodists not only in income and education but also in the likelihood of urban residence.

These three demographic factors—region, social class, and urbanism—at first served to differentiate from each other a dozen or so denominations of mostly white, Anglo-Saxon Protestants, but by the middle of the 19th century, religion in the United States became much more multicultural, with race, ethnicity, and national origin added to the demographic differentiators of religious denominations. The immigration of masses of Catholics and Jews (as well as more Lutherans) from Germany and Catholics from Ireland increased the sociological salience of

religious identity itself, the Civil War intensified religious sectionalism, and the rapid rise of African-American churches after the war added a color line between the churches (instead of simply within them). At the turn of the 19th century, immigration from Scandinavia gave language and national identity renewed religious significance for Lutherans and Baptists, and that from eastern and southern Europe strengthened minority religious groups—Catholics, Jews, and Eastern Orthodox—whose claims on constituents' loyalties were at least as strong as those of the older Protestant bodies.

These social factors in religious differentiation—class, race, ethnicity, language, urbanism, region, and the like—are not simply templates on which religious association is modeled, nor are they merely identities people carry as individuals from one locale to another, identities destined to fade as the carriers die. Religion itself is recognized in American society, if not always by social scientists, as a fundamental category of identity and association, and it is thereby capable of grounding both solidarities and identities (Herberg 1960).

First of all, religion is constitutive for some American subcultures. From early colonial days to the present many groups came to this country to practice their religion unmolested: English Puritans and French Huguenots, German Mennonites and Russian Jews, Tibetan Buddhists and Iranian Baha'is. Others, like Mormons, Seventh Day Adventists, and Jehovah's Witnesses, began in America, withdrew from the wider culture into their own geographical and associational enclaves, and then brought their new ideas back to the world. Originally a protest movement of American whites, Jehovah's Witnesses have recruited substantial numbers of African-Americans to their cause (Cooper 1974) and expanded the social space available for other religious protest movements (Fields 1982); Seventh Day Adventism created a worldwide health-oriented religious culture (Bull and Lockhart 1989); and Mormonism became "a separate and distinct religious tradition in its own right" (Shippo 1985, p. xi), the first world religion created in the United States.

Second, religion in America has historically promoted the formation of associations among mobile people. Many frontier settlements were consolidated when the pioneers set up churches. The principle of voluntary, congregational church membership made "a concrete social contribution [that] was to provide a means for hitherto complete strangers, migrants on the frontier, to establish close personal relations quickly" (Miyakawa 1964, p. 214). The same was true of transatlantic migrants. "Immigrant congregations . . . were not transplants of traditional institutions but communities of commitment and, therefore, arenas of change. Often founded by lay persons and always dependent on voluntary support, their structures, leadership, and liturgy had to be shaped to meet

pressing human needs" (Smith 1978, p. 1178). In a system where religious institutions comprehend not the whole society but subcultures, modernity, migration, and mobility make it possible for people to found religious associations that are at once self-selected and adapted to present circumstances (Olson 1992).

Third, religion in America serves as a refuge of free association and autonomous identity, a "free social space" (Evans and Boyte 1986). Throughout its history, the United States has been a dynamic, rapidly changing society, particularly in its economic aspect, with a political constitution that protects minority religious rights at the same time that it stifles minority political representation (Wuthnow 1991, p. 295). In such a setting, religion is a refuge for cultural particularity. Such was the heart of Will Herberg's theory of American religion.

Of the immigrant who came to this country it was expected that, sooner or later, either in his own person or through his children, he would give up virtually everything he had brought with him from the "old country"—his language, his nationality, his manner of life—and would adopt the ways of his new home. Within broad limits, however, his becoming an American did not involve his abandoning the old religion in favor of some native American substitute. Quite the contrary, not only was he expected to retain his old religion, as he was not expected to retain his old language or nationality, but such was the shape of America that it was largely in and through his religion that he, or rather his children and grandchildren, found an identifiable place in American life. [Herberg 1960, pp. 27–28]

Later I shall cite Herberg's own qualification of the notion that "old country" religion is simply "retained." Yet his argument helps us to understand why religion remains the preeminent voluntary associational form in our society (Curtis, Grabb, and Baer 1992).

Yet such constitutive pluralism is foreign to the older paradigm. Religious prophets scorn the social functions of particularistic religious participation in the United States and they long for signs of religious unity. The theologian H. Richard Niebuhr (1929) was scandalized by the denominational particularism he did so much to analyze. Will Herberg, as much a theologian as a sociologist, celebrated the "triple melting pot" that transmuted multitudinous nationalities into three nationwide religious communities, and he openly cheered the victory of the (Americanizing and mostly Irish) Roman Catholic hierarchy over the (particularly German) movement for ethnic dioceses within the American church (Herberg 1960, pp. 144–45). Both Niebuhr and Herberg appreciated the role that genuine religion played for communal identity in a rapidly modernizing, pluralistic society, but the fact that mass migration, one of the major and arguably least illegitimate sources of American religious

pluralism, was all but shut down in the years they wrote made the persistence of pluralism seem all the more perverse.⁸ Their moralistic attitude influenced others to interpret pluralism—beyond the legitimate Protestant-Catholic-Jew trinity—as evidence of weak religion (Berger 1961; 1969, pp. 108, 200) rather than the paradigmatic situation of religion in America.

Recent immigration has given a new boost to religious pluralism, which is just starting to receive the attention of students of religion (e.g., Christiano 1991; Denny 1987; Hurh and Kim 1990; Kivisto 1992; Numrich 1992; Warner 1990; Waugh 1992; Williams 1988). The religious experiences of immigrants from the 1840s through the 1920s—Protestants, Catholics, Eastern Orthodox, and Jews, overwhelmingly European—have been analyzed by historians central to the new paradigm (e.g., Dolan 1975, 1985, 1988; Smith 1971, 1978), but since 1965 there has been a whole new stream of immigration. Nearly as many people entered the country in the past quarter century (1966–90) as did between 1890 and 1914 (14 million compared to 17 million; see Keeley 1991). Moreover, the “new” immigrants are racially, ethnically, linguistically, and religiously more heterogeneous than those of a century ago. One result is that the purely religious boundaries of American religious pluralism have expanded with the recent addition of about as many Muslims and Buddhists to the U.S. population as there are adherents of the Eastern Orthodox churches, and a significant number of Hindus have arrived as well (Kosmin and Lachman 1991). Yet despite the novelty of these beliefs, many of the processes of immigration and religious settlement today are similar to those of the past.

Because of restrictions on the U.S. Census Bureau, firm data are hard to come by on the religious profile of the new immigrants and other expanding minorities. We do not know for certain the religious affiliations of the 22.4 million Hispanics, 1.6 million Chinese, 1.4 million Filipinos, 897,000 Indo-Pakistanis, 848,000 Japanese, and 799,000 Koreans recorded by the 1990 census (*Asian American Handbook* 1991, p. 9.61), nor the 919,000 refugees admitted from Southeast Asia between 1975 and 1989 (Rutledge 1992, p. 37). However, locally based studies have recently been conducted of some new immigrant religious identities and associations: Asian Indians in Atlanta (Fenton 1988), Chicago, Houston, and

⁸ Keeley (1991) has pointed out that large-scale de facto migration of Mexican laborers continued from 1942 to 1964 through the *bracero* program. Moreover, until 1946, the Philippines were a U.S. possession, and substantial numbers of Filipinos came as U.S. nationals in the 1920s (Kitano and Daniels 1988, pp. 78–83). The religious institutions of both groups—Mexican-Americans and Filipino-Americans—have received astonishingly little scholarly attention.

elsewhere (Williams 1988); Sri Lankans in New York City, Washington, D.C., and Los Angeles (Blackburn 1987; Numrich 1992); Muslims in Toledo (Denny 1987), rural Alberta (Waugh 1992), and three unnamed locations in the northeastern United States (Haddad and Lummis 1987); Iranians in Los Angeles (Bozorgmehr et al. 1990); Thai Buddhists in Chicago (Numrich 1992); Vietnamese in Oklahoma City (Rutledge 1982); and Koreans in Chicago and Los Angeles (Hurh and Kim 1990; Warner 1990; Yu 1988)—and patterns seem to be consistent with the experiences of earlier immigrants.

First, today as in the past, ethnic and religious mobilization and minority consciousness often begin in the home country, “amidst complex economic and cultural rivalries” (Smith 1978, p. 1165). Vietnam is culturally divided along Buddhist, Catholic, and other lines, and so are immigrant Vietnamese (Rutledge 1982, 1992). “Asian Indians” are united by a U.S. census category, but elsewhere they are divided by religion into Hindus, Muslims, Sikhs, Jains, Christians, and other groups, and by language into Hindi, Punjabi, Malayalee, Gujarati, and others. Urdu-speaking Muslims may be Pakistani or Indian in national origin (Williams 1988). Neither in the homeland nor in the United States are these identities “taken for granted” in the manner of the older paradigm.

Second, immigrant identities are not fixed after migration to the United States (Smith 1978; Sollors 1988). National, regional, linguistic, religious, and other country-of-origin affiliations vary in their contribution to the emigration process and become more or less salient in the United States (Williams 1988). A critical mass of the respective grouping may or may not be present in the U.S. locality; when it is absent, solidary groups may be broader in recruitment but thinner in commitment. The host culture itself contains “proximal hosts” (Mittelberg and Waters 1992), preexisting ethnic and racial groupings into which the host society places the immigrants, who, in turn, may reject their ascription. Group consciousness is also affected by the vagaries of interaction with agents of the host society: federal immigration authorities and census takers stress national origin, local school districts and potential employers are concerned with language, a few underfunded networks focus on gender, and some resettlement agencies care about religion (Gold 1987).

Third, because religion in the United States “is an accepted mode both of establishing distinct identity and of intercommunal negotiation” (Williams 1988, p. 3) and because migration itself is “often a theologizing experience” (Smith 1978, p. 1175), religious association may be more salient for both individuals and the group after immigration than it had been before immigration. This effect seems to pertain particularly for contemporary Indian, Pakistani, and Korean immigrants (Williams 1988; Haddad and Lummis 1987; Hurh and Kim 1990; Lee 1991). Perhaps the

most distinctively religious new immigrant group are Korean-Americans, half of whom, as sampled by Hurh and Kim (1990), report premigration membership in Christian churches but another quarter of whom affiliate with Christian churches after arriving in this country. By 1988, Korean immigrants had established some 2,000 congregations in the United States (Lee 1991). Religion does not always take on increased salience. In Los Angeles, for example, ethnic Muslim refugees from the Iranian Islamic revolution have shied away from mosques during their relatively brief residence in the United States (Bozorgmehr et al. 1990). But religion is widely available to new immigrants as a legitimate institutional form (Williams 1988).

Fourth, the institutions established by immigrants are affected by generational succession. Early in the experience of the first generation, the immigrant congregation approximates a *gemeinschaft* within the *gesellschaft*, a remembrance of Zion in the midst of Babylon, and for that among other reasons Muslim women are more visible in American mosques than in Middle Eastern ones (Waugh 1992; Haddad and Lummis 1987) and competition increases among men for positions of clerical and lay leadership in Korean-American congregations (Hurh and Kim 1990; Shin and Park 1988). For the first generation, religion is in part a refuge from America. But the arrival of a second generation, now as in the past (Niebuhr 1929), suggests to many participants that some old country ways, in particular, language, must be sacrificed in order to maintain the attention of the children (Numrich 1992). Conducting worship in the English language is one of the classic paths by which America transmutes ethnicity into religion, where what gives the group its identity is no longer Urdu, for example, but Islam (Hathout, Osman, and Hathout 1989; Williams 1988, pp. 282–83), not Japanese but Buddhism (Mullins 1988; Kashima 1990), not Yiddish but Judaism (Herberg 1960, p. 31).

In addition to international migration as a factor in religious pluralism, there is internal migration, particularly that impelled by culture, of which the migration of gay men to major cities since World War II is prototypical. Frances FitzGerald (1986, p. 27) cites a late 1970s report that more than three-fourths of the population of the Castro, San Francisco's gay district, had moved to that city within that decade. With such massive population shuffling in mind and a view of the Bay Area in front of her as she stood on one of the city's hills, FitzGerald conceived a new metaphor for the social mechanism at work in contemporary American cultural life, "*not a melting pot but a centrifuge* that spun [people] around and distributed them out again across the landscape according to new principles," including income and life-style (1986, p. 16; emphasis added). On the basis of such social sorting into a gathered community

(and his own energy and imagination), the religious entrepreneur Troy Perry founded the Metropolitan Community Church in Los Angeles in 1968 (Dart 1969, 1991; Perry 1972; Warner 1989). Thus, the grounds on which Americans gather and find one or another religious message compelling, grounds that have historically included geography, social class, race, national origin, generation, ethnicity, and language, now also include gender, sexual orientation, "life-style," and moral culture.

The work of Daniel Olson (1989, 1993) has shed light on the anomaly (to the older paradigm) that religious institutions flourish in this most mobile of societies. On the one hand, it is a well-established generalization that geographic mobility is inversely correlated in the short run with religious participation (Wuthnow and Christiano 1979; Finke 1989). It seems that mobility disrupts the social networks that support regular church attendance. On the other hand, as we have seen, transatlantic migrants invigorated American religious life in the 19th century and at least some of the post-1965 immigrants are doing so today. Moreover, churches located in growing communities—particularly suburbs—have a better chance of growing in membership than those in stable or declining areas (see, e.g., Roof et al. 1979). Furthermore, denominations grow when they "plant" new churches and decline when they do not (Marler and Hadaway 1992). Assuming that one of the motivations for religious participation in the United States is the desire for friendly and culturally supportive associations, Olson argues that members of old and stable churches tend to have all the friends they want, but that new churches are likely to have many members whose demand for church-based friendships are not yet satiated. They therefore make room in their lives for newcomers (Olson 1989). Claude Fischer's (1982) work suggests that larger communities facilitate cobelievers' spending time with each other, so that urbanism can promote religious communalism rather than homogenization. Locations with high rates of in-migration thus offer attractive markets for aggressive religious organizations.

IV. STRUCTURAL ADAPTABILITY

Religious forms are typically sacralized. For example, "apostolic succession" in the leadership structure of Catholic and Episcopal churches is a hallowed doctrine traced to Jesus's laying his hands on Simon Peter, and the "divine liturgy" in the Orthodox church is claimed to represent an unbroken patriarchal tradition well over a thousand years old. The older paradigm theoretically privileges religious establishments and is inclined to take their word for what is truly "religious" and what is "worldly." But because of disestablishment, U.S. religious forms have historically been malleable. Thus in America today there are Roman

Catholic parishes pastored, *de facto*, by women (Wallace 1992), and Orthodox churches where scriptures are read (with an American accent) also by women (Warner 1992). Religious forms change in the United States, and Herberg (1960, p. 83) knew better than to say that they are only "retained" from historical patterns: "religion in America has tended toward a marked disparagement of 'forms,' whether theological or liturgical." But the new paradigm does not regard religious change as presumptive evidence of "worldliness."

From the perspective of Europe, where the universal "church" was the social and theoretical norm against which the radical "sect" perennially protested, the American "denomination"—making little claim to inclusiveness yet also working within the world—was a structural innovation (Niebuhr 1929; Smith 1968; Swatos 1979). The voluntarism of the U.S. religious system has also facilitated the development, since early in the 19th century, of parachurch "special purpose groups" (Wuthnow 1988, chap. 6), both bureaucracies (e.g., the American Bible Society) and collegia (e.g., the Full Gospel Business Men's Fellowship International). Another pervasive American pattern is the congregational model of local church organization, whether or not sanctioned by the hierarchy (Silver 1990; Warner 1993).

Long-term centralization and bureaucratization characterize much of organizational life in modern societies (Powell and DiMaggio 1991), but the institutional history of U.S. religion is better seen as an alternation of centripetal and centrifugal tendencies.

The centripetal tendency comprises bureaucratization and professionalization of denominational and ecumenical staffs, where material and especially human resources flow upward toward headquarters and greater attention is paid to edicts of the center, whether or not such a flow is legitimated by the respective religious doctrines. In the history of mainline Protestantism from the Civil War to the Vietnam War, centripetal processes produced denominational bureaucracies and ecumenical agencies (notably the National Council of Churches) and tended to substitute professional expertise and political ideology for religious orthodoxy as a source of organizational power (Hadden 1969; Pratt 1972; Carroll, Hargrove, and Lummis 1983, chap. 2; Wuthnow 1988; Ammerman 1990; Chaves 1991b; Olson and Carroll 1992). At its height in the first half of this century, centripetalism in mainline Protestantism took the form of what felt like, to both its own fiduciaries and excluded minorities, a protected establishment (Hutchison 1989); the recent erosion of this "establishment" centripetalism is interpreted by some participants as the long-delayed process of secularization predicted by the older paradigm. One result of this temporary establishment was that, for a generation, mainline Protestant leaders were in general more liberal than their laity

(Hadden 1969; Hoge 1976; Takayama 1980; Wood 1981; Warner 1988). More recently, the centripetal flow in mainline Protestantism has ebbed, indeed reversed, as denominational and ecumenical budgets have been slashed and symbolically significant mainline headquarters have been moved from New York to places like Cleveland and Louisville (McKinney and Roof 1990). Undoubtedly, the most influential attempt to delineate recent centrifugal processes is found in Robert Wuthnow's (1988) chapter on "the declining significance of denominationalism," which analyzes both the blurring of individual denominational identities and the weakening of denominational organizations. Wuthnow's evidence suggests that since World War II denominations have become less distinct from each other but more diverse within themselves in member attitudes and demographics. For instance, the old nominal-level variable "Catholic-Protestant" has ceased to have predictive power for most sociological studies of individual beliefs and behavior and is instead replaced by frequency of church attendance. Wuthnow documents declining interdenominational antagonism and increasing cross-denominational intermarriage and membership switching. Perhaps because of the success of ecumenism, denominational loyalties have declined and church shopping has increased. In important mainline Protestant bodies, fewer clergy confine their training to their denominational seminaries, and local churches send a declining share of their resources to organizational headquarters.

Wuthnow intends none of this to say that religious boundaries have disappeared or that religion has no further power as a sociological variable. The social centrifuge, as FitzGerald (1986) would have it, not only spins things out from the center but also reshuffles them into new combinations. Wuthnow's (1988) "restructuring" spotlights an organizational shift along a new cultural fault line of American religion, comparable to that which took place early in the 19th century (see Wuthnow 1988, p. xiv). Wuthnow sees denominations fading relative to nationwide special purpose groups on each side of the divide between religious "left" and "right" (see also Liebman and Wuthnow 1983; Roof and McKinney 1987; Diamond 1989; Jorstad 1990; Hunter 1991). On the left are such organizations as People for the American Way, Witness for Peace, and Clergy and Laity Concerned; on the right are the Moral Majority, Focus on the Family, and Religious Roundtable.

Another centrifugal conceptualization is "de facto congregationalism" (Warner 1992), that is, labeling an institutionalized bias of American religious life toward affectively significant associations under local and lay control, beginning with observations of differences between congregations within the same denomination (Warner 1983, 1988; see also Carroll and Roozen 1990). De facto congregationalism implies that the local religious community is in fact constituted by those who assemble together

(which is the etymological root of "congregation") rather than by the geographic units into which higher church authorities divide their constituents, which is what "parishes" historically are. Since Vatican II, the Catholic church seems to be quietly relaxing the geographic parish concept of local church affiliation in the direction of the more cultural gathered-congregation concept, accommodating itself to members' cultural values. Music, architecture, preaching, liturgy, and sexual orientation are thus joining language and national origin as principles of intra-Catholic differentiation (Warner 1992; Christiano 1991). The historian Jay Dolan indicates that Catholic *de facto* congregationalism has deep roots in America. "The post-Vatican II era has rightly been called the age of the emerging laity, but history reminds us quite clearly that, even in the brief past of Catholic America, lay people at one time had a major responsibility for the growth and development of the local church" (Dolan 1985, p. 192; see also Smith 1971).

The normative congregationalism of Judaism has long facilitated adaptability, and at the present time it is women who are causing the greatest changes in that ancient tradition by claiming professional opportunities as cantors and rabbis and demanding that they ought to be counted as members of the minyan for prayer. The resulting adaptations include ordination for women in Reform and Conservative branches, the synagogue reform movement known as Egalitarian Minyan, and the house-based groups known as Havurot (Wertheimer 1989, pp. 96, 104, 129–34, 136, 141, 154–57; Prell 1989). Beginning in the middle of the 19th century, an earlier congregationally based innovation was mixed gender, or "family," seating (Sarna 1987).

Congregational patterns seem to be emerging among non-Christian religious groups. Among immigrant Muslims, the mosque, established in Islamic countries as a place for prayer, has become an educational and service center to meet the needs of the Muslim community, a congregation, in other words, with adult classes, potlucks, and coffee hours. The imam, who, according to Sunni Islamic practice, is simply the prayer leader, has become in America a religious professional who celebrates marriages, counsels families, visits the sick, conducts funerals, and represents his people among the local clergy, modeling himself in the process on pastors, priests, and rabbis (for details, see Chazanov 1991; Denny 1987; Fenton 1988, pp. 187–97; Haddad and Lummis 1987; Waugh 1992; see also Kashima [1977] on Japanese-origin Buddhists and Fenton [1988] on Hindus).

The adaptability of religion in the United States does not mimic liberal democracy or necessarily bring "progress." American religious institutions respond to both consumer demands and supplier initiatives. Today, many lay people make claims in the name of democratic and, increas-

ingly, feminist values that are discovered by insiders to reside in the religious tradition itself; one result is such innovations as Egalitarian Minyan. But other democratic tendencies tend to be theologically conservative: the Christian charismatic movement, in which individuals feel themselves to be directly in touch with the deity, and evangelicalism, whose tradition of Biblical literalism and literacy mean that the ultimate charter of the group is at the disposal of any member (McGuire 1982; Neitz 1987; Warner 1988; Bender 1991). Market incentives induce religious elites to maximize the appeal of their organizations to potential constituencies, and one result is entrepreneurial but authoritarian religious institutions—for example, fundamentalist missions to Hispanics (e.g., Montoya 1987) and Buddhist centers catering to European-American converts (Fields 1991, 1992; Preston 1988).

While in medieval Europe there was only one “the” Church, religion in America has taken many forms, denominational and congregational among them. Since the new paradigm recognizes the historic popularity of American religion, it is more generous than the old paradigm in crediting such forms as genuinely religious.

V. INDIVIDUAL AND GROUP EMPOWERMENT

The older paradigm expects religion to be increasingly privatized and invisible when it is not generalized (see Sec. I above). Moreover, those most interested in promoting social change, scholars affiliated with the left, have been long disinclined to think that religion could play a positive role from the point of view of their values (Hannigan 1991, pp. 317–18; Fields 1982). Thus, the role of religion in social change has been widely overlooked.

Yet religious involvement in the United States has historically been one way that groups have improved their lot. The 1960s would not have been the same without American churches, and Aldon Morris (1984, p. 4) reminds us that “the black church functioned as the institutional center of the modern civil rights movement.” Nor would the outspoken moral conservatism of the 1980s have been the same without churches. The Moral Majority was mobilized by entrepreneurial pastors of local churches, largely independent Baptists, who represented not an establishment, but an insurgency (Liebman 1983, p. 72; see also Guth 1983; Ammerman 1990). But moral conservatives are not the only recent players, since Sanctuary, the movement challenging U.S. immigration policies toward central American refugees, is also “firmly rooted in religious groups” (Wiltfang and McAdam 1991, p. 1003; see also Matters 1992).

Remarkably, the gay liberation movement is itself a practitioner of the art of church-based mobilization. Troy Perry, some of his allies in the

gay rights movement, and his severest conservative critics all credit the Metropolitan Community Church with being the organizational center of the attempt to legitimate gay culture in the United States (Perry and Swicegood 1990, chaps. 9–10; Humphreys 1972, pp. 149–53; Rueda 1982, pp. 270–96). Dennis Altman, a radical theoretician of the gay movement, observes that “in many places the church is the only form of the gay movement that exists,” and he characterizes Perry as “perhaps the most charismatic leader yet produced by the American gay movement” (Altman 1982, pp. 123, 27).

That churches can play this empowering role is due in part to the pluralism that Christian ethicists deplore and the old paradigm misconstrues, which is embodied in the widely cited observation that eleven o'clock on Sunday morning is the most segregated hour of American life. The obverse of this unpleasant truth is that any group can have its own church. Churches, synagogues, mosques, and the like, as human institutions dedicated to spiritual matters, also inherently have access to the worldly; they combine the symbolic and the material, the cultural and the structural, group morale and social networks. Insofar as a subordinated group requires for its emancipation access to financial and social resources, churches in the United States are a convenient and legitimate means of organization, and in some cases—the classic example may be found among African-Americans—they may be the only such means available. “Churches provided the [civil rights] movement with an organized mass base; a leadership of clergymen largely economically independent of the larger white society and skilled in the art of managing people and resources; an institutionalized financial base through which protest was financed; and meeting places where the masses planned tactics and strategies and collectively committed themselves to the struggle” (Morris 1984, p. 4). Jesse Jackson recently testified that “church was like my laboratory, my first actual public stage, where I began to develop and practice my speaking powers” (as quoted in Frady 1992, p. 59). Empowerment is partly a function of pluralistic social organization.

But the special potency of religious institutions comes from answers they give to a group's need for faith in the justice of their cause and the inevitability of triumph. Such faith depends on the conviction, misleadingly called “other-worldliness,” of the existence of a religious reality. If one assumes a sacred/secular dichotomy, supernatural beliefs can seem at best irrelevant to this-worldly action, antagonistic at worst (cf. Fields 1982; Hannigan 1991). In this view, shared by many social scientists and some liberation theologians, the most progressive religions must be the most demythologized; thus, for their own good, oppressed groups must slough off their superstitions. But on the model of the African-American experience, where sacred and secular are inextricable, the new paradigm

expects otherwise. To insist that rebels be iconoclasts is to deprive them of one source of their courage.

Eric Lincoln and Lawrence Mamiya state, in their recent comprehensive study of the black church, that "other-worldly religious transcendence can be related dialectically to the motivation, discipline, and courage needed for this-worldly political action" (Lincoln and Mamiya 1990, p. 234). Based on a textual analysis of spoken sermons, Bruce Rosenberg writes that African-American preachers know that there are "sacred texts that cannot be altered. . . . David must always slay Goliath, Christ is always the Son of God, and always in the beginning God makes the heaven and the earth" (Rosenberg 1988, p. 145). Pastor James Henry Harris adds, "Black folk expect the preacher to reassure them of God's power, not to question or doubt it. They expect the pastor to help them cope with joblessness, poverty and discrimination by transforming their despair into hope" (Harris 1990, p. 599). As Garry Wills puts it, "Hope welling up from the darkest places remains the miracle of African-American Christianity" (Wills 1990, p. 204). Thus numerous empirical studies attest that the black church is historically *both* theologically conservative *and* a resource for social change; it contributes *both* to group solidarity *and* personal well-being (Ellison 1991). African-Americans who leave the church in disgust in effect distance themselves from the black community and its struggle as much as from religion (Sherkat and Ellison 1991).

Other communities have manifested the power of combining autonomous organization with theologically conservative beliefs and practices. Dwight Billings's theory of "religion as opposition" argues that among Appalachian coal miners in Holiness and Baptist congregations independent of company control, "the repetition of collective symbols and their ritualized expression in sermons, prayers, and group singing helped to sustain miners' commitment to the sacred cause of unionism and solidarity" (Billings 1990, p. 20). By segregating themselves into a denomination, the members of the Metropolitan Community Church are able both to organize their own ministries and to celebrate Troy Perry's pentecostally inspired conviction that he was created gay by a beneficent personal God (Perry 1972, p. 111; Perry and Swicegood 1990, p. 30; Warner 1989). Although the analyst must in each case elucidate the mechanisms by which supernatural religion facilitates social strength, experience teaches us to look for such a link.

Thus it is to be expected that the empowerment functions of religion are latent. At an individual level, those who seek well-being in religion tend not to find it; those who gain well-being from religion are not those who seek it (Althauser 1990). At a communal level, the New Religious Right of the early 1980s harnessed individual religious convictions to

political ends, but the process did not work in the other direction, that is, individual political ends did not take religious forms. Those who watched the politicized TV preachers were more interested in their religious than political messages (Shupe and Stacey 1982). Something similar is true of African-American religion: Wilcox and Gomez (1990) argue on the basis of data from the 1979–80 National Survey of Black Americans that the significant effect of religion on political participation and political attitudes is indirect and is mediated through the contribution of religion to group identification. In other words, political empowerment appears to be a by-product of religion, not its manifest goal (see also Sherkat and Ellison 1991).

Therefore, although we can notice stirrings of collective public participation stemming from the religious institutions of new immigrants in the United States, it would be grossly premature to test the political empowerment hypothesis on their current level of outspokenness. As Lincoln and Mamiya observe, in reference to the black church and political action, “Both forms of protest and electoral politics are only made possible by the *prior* foundation of community building activity” (1990, p. 199; emphasis added). Studies of conservative Protestants, who seemed to have emerged out of nowhere in the 1970s (cf. Herberg 1960, p. 123), reveal that their vociferousness was preceded by generations of institution building away from the attention of the broader public (Brereton 1991; Carpenter 1980; Marsden 1987).

The role of religious organizations for the empowerment of women presents a rich test case for the new paradigm. Given the role of religion in the construction and maintenance of patriarchy, it is no surprise that, with few exceptions, the more an American woman identifies with feminism, the less she identifies with organized religion (Wuthnow 1988, pp. 226–30). Yet it is also true that, by nearly any measure, American women are more involved in religion than their male counterparts—more likely than men to attend church, to read the Bible, to pray, to say that religion is salient to their lives, and less likely to profess nonbelief (Wuthnow 1988; Wuthnow and Lehrman 1990). Some theoretical approaches see women’s religiosity as so much false consciousness, an extension of patriarchal control. Those oriented to the new paradigm, however, are inclined to agree with feminist theories that women’s cultures—whether or not feminist (Cott 1978, 1989)—represent women’s attempts to make the best of their historical circumstances (Kandiyoti 1988), and they have learned to look to U.S. social history for evidence.

The mid-19th-century social movement for “moral reform,” arguably the first public social space for American women as a whole and, in turn, the organizational matrix out of which first-wave feminism emerged, was a direct outgrowth of the evangelical Second Great Awakening. During

that era, roughly the first third of the 19th century in the United States, the modern gender order of "separate spheres" was developed,⁹ which in turn ruled relations between the sexes until only a generation ago. As social organization in the years between the Revolution and the Civil War moved away from home-centered production toward the differentiation of industrial production and family consumption, the vocational education of young men no longer took place in apprenticeships at their fathers' side, and American Protestant women joined with their pastors to construct an essentialist doctrine of womanhood as custodian of culture and protector of morality. Business and politics were men's business, but culture and morality were spheres belonging to women. Seeing that their interests and values were distinct from those of men, women soon came together in local, regional, and national associations for the promotion of "moral reform." Insisting that home was woman's place, they did not limit woman's place to the home but also opened a public arena of women's action (Cott 1977; Ryan 1979, 1981; Epstein 1981; Smith-Rosenberg 1985).

The lesson for the new paradigm is pointed. The very pluralism of American religion that gives it power to promote group solidarity also makes it the more likely that the voices of those subordinated within the group are silenced. If the religious community simply mirrors the local patriarchy (or the local gerontocracy), women (or young people) will have reason to escape it (Gilkes 1985; Burdick 1990; Billings 1990). Yet insofar as the relationship between domestic and religious institutions is orthogonal—that is to say as long as there is a structural church-family differentiation—"institutional contradiction" (Friedland and Alford 1981) can allow women to play one patriarchal institution against another. Women's associations provide such differentiation. "Using religion to develop extra-domestic roles, [women] created powerful local and nationwide single-sex organizations expressive of women's particular angers, anxieties, and demands" (Smith-Rosenberg 1985, p. 142).

Successors to moral reform organizations provided avenues of women's influence in mainline Protestantism until a generation ago, when, in a program of gender desegregation and ecumenism, they were absorbed into denominational agencies (Brereton 1989) at about the same time that openings accelerated for women in seminary education and mainline Protestant and liberal Jewish ordination (Carroll, Hargrove, and Lummis 1983; Wertheimer 1989, pp. 104, 130–34, 141). Thus, although ordained women's career opportunities are not equal to men's, it is probably no

⁹ The nuclear family with a division of labor into men's and women's spheres is historically "modern," not, despite the rhetoric of both reactionaries and feminists, "traditional" (see Stacey 1990, chap. 1).

accident that among Presbyterians, Episcopalians, and Jews, there is no inverse correlation between feminist consciousness and women's religious participation (Wuthnow 1988, pp. 229–30).

Yet with only the minimal organizational leverage of being extrafamilial, religion has long provided moral leverage to American women when other power resources were lacking. Among Catholics in the years between the Depression and the 1960s, devotion to Saint Jude led women to feel “empowered in new ways. They broke off relationships with ‘mean’ boyfriends [and] rejected unwanted medical treatments” (Orsi 1991, p. 159; see also McGuire 1982, pp. 179–82; Neitz 1990, pp. 101–4). Among Pentecostals in rural Missouri, “religious life provided the *only* opportunity for young girls to leave home and travel. . . . Young girls who could go virtually nowhere else could hardly wait to don their best dresses and hurry down to the tent” (Lawless 1988, pp. 73, 75; see also Lawless 1983). In today's neo-evangelical and charismatic movements, women often convert before their husbands do; in this they are like their sisters of the Second Great Awakening and today's Latin American evangelicals, who gain the moral leverage that comes with entering the religious community of their own volition (Warner 1988, pp. 118–27, 143–45, 292–93; Ryan 1981; Brusco 1986). Precisely because of the power of its God, Pentecostalism gives some women opportunities as religious entrepreneurs (Lawless 1988; Kwilecki 1987).

Women committed to marriage and motherhood find moral support and interpersonal leverage in many evangelical and charismatic fellowships, in which relational values and androgynous images of God are increasingly endorsed (Hunter and Stehlin 1987; Neitz 1987, pp. 149–50). In a modern Orthodox synagogue, Lynn Davidman observed that “one of the aspects of family life in this Orthodox community that was highlighted by several women was that the men seemed to be involved in child care, both within the synagogue and outside of it” (Davidman 1991, p. 117). On the basis of field research with “postfeminist” converts to evangelicalism, Stacey and Gerard (1990, pp. 111–12) write that such women's “turn to evangelicalism represents . . . a strategy that refuses to forfeit, and even builds upon, the feminist critique of men and the ‘traditional’ family,” and provides them with “effective strategies for reshaping husbands in their own image.”

Proponents of both the newer paradigm (Iannaccone and Miles 1990) and the older one (Hunter and Stehlin 1987) have recognized that much of contemporary conservative religion in the United States, particularly evangelicalism and neo-Pentecostalism, has been affected by feminism, which indeed is a fact that the public needs to know (see Stacey 1990). But this fact can be interpreted as representing the capitulation of religion to secular currents—as ultraconservatives, radicals, and the older para-

digm are inclined to see it—or as a mobilization of latent feminist currents in religion itself—a view consistent with much recent historical research. For example, the stringent patriarchy of contemporary Protestant fundamentalism, which the new evangelicals are relaxing, the dictum that no woman may hold authority over any man, far from being “traditional,” was itself a turn-of-the-century reaction against feminist currents in 19th-century evangelicalism (Caldwell 1991; Barfoot and Sheppard 1980; Zikmund 1979).

Ideological leverage inheres in religion (Fields 1982). Jewish and Christian feminists have good reason to see adumbrations of their ideals lying near the heart of the traditions they wish to alter, for example in the Gospel testimonies that the Resurrection was first revealed to Jesus’ female disciples. The affinity of Korean-Americans for Christianity, and more particularly for Presbyterianism, is partly due to the heroic role played by Presbyterian missionaries and converts in Korea’s turn-of-the-century struggle against Japanese colonialism (Kitano and Daniels 1988, pp. 113–14). Troy Perry’s moment of inspiration to establish the Metropolitan Community Church came when he realized that the God whom he had not ceased to love and fear wanted Perry to take his gospel of innate homosexuality into the gay community (Perry 1972, p. 8; Perry and Swicegood 1990, p. 30).

VI. THE “NEW” VOLUNTARISM

The preceding four sections have developed four distinctive and perennial aspects of American religion which, under different rubrics, have received the attention of contemporary researchers and which, considered theoretically, pose an alternative to the older paradigm. With appropriate complications and qualifications, religion in the United States is and has long been (a) disestablished, (b) culturally pluralistic, (c) structurally adaptable, and (d) empowering. My final topic is the recent (i.e., post-1960s) complex of individualized religious identification—including conversion to new religious identities and the assertive embrace of old ones, as well as apostasy on a wide scale—that I will follow Roof and McKinney (1987) in referring to as “the new voluntarism.”¹⁰ The contemporary

¹⁰ The concept of “voluntarism,” as used here, stands in the tradition of American religious studies rather than sociological theory. Sometimes called “voluntaryism” (Ahlstrom 1972, pp. 382–83), the religious studies concept refers to the *concrete* institutional facts of separation of church and state and religious freedom in the postrevolutionary United States and the consequent need for churches to rely on persuasion rather than coercion for their support (Littel 1962; Mead 1963). Such “voluntarism,” strange and remarkable to Europeans, “became a matter of course to Americans” (Rowe 1924, p. 53), and over time it evolved into the religious system portrayed here.

scene seems sufficiently discontinuous with the patterns described more than a generation ago by Herberg (1960) to raise the question whether the American institutional complex portrayed in this article persists or whether, as the older paradigm would have it, we are witnessing the latest stage of "secularization."

Consider these figures: between one-third and one-half of those responding to polls have changed denominations in their lives, some of them only to switch back to the affiliation of their youth, more to an adjoining denomination, but many to religious disaffiliation (Hadaway and Marler 1991a; Roof and McKinney 1987, p. 165). One-fifth of those raised Catholic no longer identify with that faith, and they include an estimated one million Hispanics who have gone over to Protestantism within the past 15 years (Greeley 1990, p. 120). The proportion of Americans claiming no religious preference (the people sociologists of religion call "nones") has jumped from 2%–3% a generation ago to 7%–9% today. Moreover, with the exception of African-Americans, nones do not as a whole occupy alienated or marginal status in U.S. society (Glenn 1987; Roof and McKinney 1987, p. 99; Kosmin, Keysar, and Lerer 1991). Probably as radical as switching—and certainly as unsettling to loved ones—are such surprises as the Pentecostal spirit baptism of lifelong devoted Catholics (Neitz 1987), born-again evangelicalism among mainline Protestants (Warner 1988), and the "return" of nominal Jews to an orthodoxy they had never before embraced (Davidman 1991; Kaufman 1991).

The voluntarism is attitudinal as well. Gallup (1988, p. 3) reports that 80% of Americans agree that the individual "should arrive at his or her religious beliefs independent of any church or synagogue" (see also Roof and McKinney 1987, p. 57). Roof (1993) and his associates have tracked accounts of spiritual trajectories of "baby boomers" away from and back toward conventional religion and many syncretic alternatives. Phillip Hammond (1988, p. 5) speaks of a growing shift from "collective-expressive" church membership in the past to "individual-expressive" religious involvement—voluntary and independent of other social ties—

The "voluntarism" of sociological theory, particularly associated with the early work of Talcott Parsons, concerns the *analytic* question of the categories needed for the analysis of individual action (Alexander 1982). Insofar as Parsons, himself raised a liberal Protestant, conceived modern social order to minimize coercion, the two concepts were no doubt related in his mind. Moreover, since the "new voluntarism" complex conceptualizes an additional, perhaps temporary, movement toward individualism in religion (referred to below as a disjunction of culture and social structure), the phenomena it delimits are closer still (albeit not identical) to the theorists' concerns. The new voluntarism has also been called the "third disestablishment" (Roof and McKinney 1987; Hammond 1992).

today (see also Hammond 1992). Samuel Heilman (1990, p. 195) writes that, "for the contemporary Jew, corporate identity diminishes and ascription gives way to achievement and autonomy as the most powerful determinants of identity." Elsewhere I have written that evangelical Protestantism upholds an ethic of achieved rather than ascribed recruitment (Warner 1988, pp. 52–53, 72, 292–93). It is true by definition that membership in a new denomination such as the Metropolitan Community Church is an achieved status; this is true as well as for the Vineyard, one of several conservative Christian protodenominations emerging out of the late 1960s Jesus movement and appealing primarily to baby boomers (Perrin and Mauss 1991). These are inherently churches of converts.

In other words, both religious disaffiliation and religious conservatism benefit from "achieved" religiosity; the United States has seen both religious revival and apostasy (Chaves 1989; Roozen, McKinney, and Thompson 1990). Taken-for-granted, traditional religion is passé. Born-again, return-to-the-fold neotraditional religion is all the rage.

The authors of *Habits of the Heart* (Bellah et al. 1985) have most eloquently lamented these individualistic trends. Although they recognize that Americans, no matter how individualistic, seek out like-minded others, they fear that the resulting associations are only "lifestyle enclaves," a term they intend to connote shallowness and mutual narcissism. "When we hear such phrases as 'the gay community' or 'the Japanese-American community,' we need to know a great deal before we can decide the degree to which they are genuine communities and the degree to which they are lifestyle enclaves" (Bellah et al. 1985, pp. 74–75). These authors worry about a culture that encourages Americans "to choose the groups with which [they] want to identify" (p. 154), and they propose instead, in the spirit of the older paradigm, that "there is a givenness about the community and the tradition. They are not normally a matter of individual choice" (p. 227).

I do not wish to dismiss the concerns of Bellah and his colleagues, but there is considerable evidence that religious switchers are morally serious. Kristin Luker reports that nearly 20% of the pro-life activists in her study of the abortion controversy were "converts to Catholicism, people who have actively chosen to follow a given religious faith, in striking contrast to the pro-choice people, who have actively chosen not to follow any" (Luker 1984, p. 196). Evidence that FitzGerald's social centrifuge contributes to moral coherence is presented in Roof and McKinney's analysis of 1972–84 General Social Survey data, which indicates that Protestants' inveterate switching of denominations is increasingly motivated by moral culture instead of socioeconomic status. Those shifting their allegiance to the liberal Protestant denominations like the Episcopal and the Presbyterian churches are more liberal on matters of women's rights and racial

justice than those raised in these communities, while those gravitating to conservative bodies like the Southern Baptists and the Nazarenes are accentuating those bodies' conservatism on sexual morality (Roof and McKinney 1987, pp. 218, 220, 222).¹¹ Switching is decreasingly likely to mirror upward social mobility and to represent instead genuine religious change; "switchers are, in a very real sense, converts" (Hadaway and Marler 1991a, p. 22). Protestant switching is not entropic (Sullins 1992).

Switching includes the disaffiliation of dropouts, to be sure, but for those who shift from one faith community to another it also means greater religious involvement—contributing money to the church, frequent prayer and Bible reading, being "born again," being in agreement with the moral culture of their newfound reference group, searching for more meaning in religious participation (Roof and McKinney 1987; Hadaway and Marler 1991a; Mauss and Perrin 1991). Conservative churches that expect high levels of involvement are organizational beneficiaries of such switching patterns (Mauss and Perrin 1991; Roof and McKinney 1987, pp. 177–79), but liberal churches that take strong stands and make strong demands can attract newcomers as well (Matters 1992; Wiltfang and McAdam 1991).

We also know that irreligion in the United States replicates itself across generations less effectively than active religious preference. Though the proportion of "nones" has roughly tripled since the 1950s, nones tend to generate additional nones less efficiently than Protestants, Catholics, and Jews do their own kind (Roof and McKinney 1987, p. 169). This may be in part because, in a religious society such as ours, nones are surrounded more by religion than by irreligion. Men are more likely to lack religion than women, but religiously indifferent fathers are particularly poor in passing on their indifference to their offspring. Moreover, the *rate* of disaffiliation is declining. Disaffiliation was common in the young adulthood of baby boomers but is less so more recently (Hadaway and Marler 1991a). Religion is still a prime idiom by which Americans identify themselves.

Yet religion need not represent something in which people are primordially rooted. Religious affiliation in the United States is not tribal. The freedom of Americans to choose with whom they will congregate in service of their most basic values is a freedom not to "pass" as biological

¹¹ Roof and McKinney (1987) measure mobility of identifiers, not members, across "families" of denominations, not denominations per se. Thus, for their research purposes, Presbyterians are the same as Episcopalians since both are "liberal Protestants," Lutherans and Methodists are alike "moderate Protestants," and Southern Baptists and Pentecostals are "conservative Protestants." The cultural sorting they map, therefore, is ambiguous with respect to the hypothesis of the declining significance of denominational identities, strictly speaking.

kin but to partake as full legatees of cultural traditions that add depth and richness to the association. Literate converts to a religion of the book have immediate access to its communal memory. The religious groups that seem to work best in cosmopolitan America are those that recognize the mobility of their members and bring them into contact with great cultural traditions by incessantly and elaborately recounting the founding narrative (Warner 1988, chap. 9).

It is helpful in this regard to think of religion in the United States as being subject to the decoupling of culture and social structure (Bell 1976). Religiously relevant statuses are increasingly random with respect to the standard categories of social structure (Caplow 1985; Hammond 1992; Hadaway and Marler 1991a). Thus the new paradigm is not surprised by news of people with modern intellectual resources who reject secularity in favor of "Bible-based" Christianity and "observant" Judaism. Susan Rose (1987, p. 255) tells of women in an upstate New York religious commune who "knowingly and willingly stepped down and relinquished their authority and power" because they valued relationships with men. Nancy Ammerman (1987, pp. 26–31, 72–102) studied a fundamentalist church in New England whose members were demographically and educationally clustered slightly on the more privileged side of the surrounding middle-class suburb but who withdrew morally from what they felt to be an alien world. Lynn Davidman (1991, chap. 5) spent time with converts to orthodox Judaism in Manhattan—young, educated, well-employed women who found in traditional religion a legitimation for the families they hoped to create. In each of these studies, religious commitments helped people set and maintain priorities in a time of perceived bewildering choice.

Has American religion become rootless, evanescent, or arbitrary? No. The breakdown of ascription may be welcomed when, like members of the Metropolitan Community Church, its beneficiaries are convinced that they have been freed to acknowledge their true nature. What the new religious voluntarism amounts to is a centrifugal process, sorting elemental qualities on the basis of which identities are constructed. The evangelical Presbyterians I met in Mendocino, California, had in common not their denominational or educational backgrounds but their histories as migrants to an idealized small town. Their pastor's preaching united their ideological neoparochialism with the theology he had learned in seminary so as to give their common narrative deep resonance (Warner 1988, pp. 86–87, 205–8). In this way, the breakdown of ascriptive ties to religion can enhance, rather than reduce, the elemental nature that believers attribute to their experiences. From this point of view, social ascription that denies one's true being is seen as arbitrary, while a new-found religion is self-affirming.

I do not wish to overestimate either the extent or the appeal of religious mobility, nor ignore the pain that often accompanies it. Once having chosen a religious home, one is supposed to be and likely to be loyal, and it is probably true that someone dissatisfied with her or his church is as likely to turn away from churches altogether as to seek a church more conformable to personal needs, at least in the short run. There is a norm to the effect that shopping for religion is wrong, and talk of a "religious market" is highly offensive to many people, particularly when it suggests an instrumental attitude toward religion, or ecclesiastical social climbing. Such a norm is no doubt functional for the stability of religious organizations.

What facilitates religious mobility despite such a norm and despite investments in religious capital (Iannaccone 1990) are several social facts, including aggressive proselytization; the emphasis on loyalty to God over institutions that is part of the evangelical—and hence mainline Protestant—tradition; members' intermittent involvement, such that some who are formally church members may not feel committed and therefore not disloyal when they leave; life-cycle events such as marriage, particularly religious intermarriage; children, for whom one may want to choose an appropriate Sunday school; and geographic mobility. Geographic mobility requires people to choose a church. Since denominations are not homogeneous, the church of one's former denomination in the new location may not "feel right." Denominations themselves change, and the switcher may well perceive that it is not she or he who left the fold.

More research is needed on the question whether rates of religious mobility have recently increased over those prevalent in the 1950s, as has been argued by Roof and McKinney (1987) among others. Yet it should be borne in mind that religious individualism and denomination switching characterized earlier periods of U.S. history, particularly the "awakenings" that took place around 1800–30 and 1890–1920.¹² It was in the former period that the numerical dominance of what later came to be called the liberal Protestant denominations was eclipsed by the surge of the evangelical Methodists and Baptists. The latter period saw the rise of the Holiness, Pentecostal, and fundamentalist movements, the recent visibility of which has so greatly altered the profile of late 20th-century U.S. religion.

Like the present, these were times of massive geographical and social mobility, when individuals could not effectively follow in the footsteps of their parents but had to "start over" for themselves (FitzGerald 1986, pp. 383–414; see Ryan [1981] for the earlier period and Thomas [1989] for the later). Large numbers heeded the messages of religious innovators,

¹² The dates are from McLoughlin (1978).

and from these times of intense, revivalistic competition new institutions were born, institutions with the potential to solidify into powerful organizations and even rigid bureaucracies (Pritchard 1984; Barfoot and Shepard 1980; Poloma 1989). Before entertaining the hypothesis that a new religious order prevails in the United States, it is worthwhile to mine the analogies between the present and the American past.

Such an analogy occurred to the anthropologist Riv-Ellen Prell as she looked back on her study of the early 1970s Egalitarian Minyan in Los Angeles. Prell (1989, p. 27) "came to understand the strong parallels between Minyan members and their parents' generation's constructions of Judaism." "I was struck," she writes, "by what these parallels revealed about American religion, namely that religion had been voluntaristic in America ever since immigrants arrived. What appeared, for example, as a counterculture rebellion had its roots deep in immigrants' attempts to maintain their Judaism within American society."

CONCLUSION: THE NEW PARADIGM AND THE AGENDA OF THE FIELD

It is conventional to conclude a paper with a call for more research, but this article—both a research review and a proposal—is such a call. I have highlighted recent work of many scholars, interpreting their findings as only loosely bound (if at all) to the older paradigm. My proposal claims that recent work is more compellingly framed in terms of the newer paradigm. The nascent paradigm itself is the self-conscious project of only a few scholars (without anyone's permission having been asked, it is reasonable in this connection to name Theodore Caplow, Roger Finke, Andrew Greeley, Nathan Hatch, Laurence Iannaccone, Mary Jo Neitz, Daniel Olson, and Rodney Stark), and they do not form a solidary group but a loose school of thought with a common focus on the distinctive institutional parameters of the U.S. religious system—particularly the combination of disestablishment and institutional vitality—as the analytic norm for the study of religion.

Some scholars who are aware of the theoretical ambitions of the new paradigm have spoken out in opposition to it (e.g., Lechner 1991; Breault 1989a), but most scholars in the field are uninvolved in the debate. Yet my claim here is that, because so much recent research focuses in fact on U.S. religious institutions, there is an immanent direction to the research programs even of those not involved in debates over paradigms. Progress in that direction could be facilitated if, in the work of such persons, the presuppositions of the new paradigm were substituted for the old. If that substitution were widespread, several consequences would ensue:

Students of religious communities and subcultures would focus more on the building of religious institutions and the role of religion in social mobilization and relatively less on the erection and maintenance of plausibility structures.

Students of religious organization would focus as much on the rise of new religious organizations (e.g., Perrin and Mauss 1991) as the decline of old ones (e.g., Hoge and Roozen 1979), even though data would be intrinsically more difficult to find; those who focus on individuals and organizations would analyze entrepreneurial (Kwilecki 1987; Harrell 1985; Stout 1991) as well as bureaucratic (Chaves 1991a) and professional (Charlton 1978; Carroll et al. 1983) religious careers.

Students of social change would investigate the ways in which religion alternatively facilitates and inhibits collective action but would extend their time horizon for these processes to the span of a generation and complicate their models to include indirect effects of group solidarity.

Students of the intersection of biography and social history would assume that individual religious affiliation is not an ascriptive identity set for life but something that can be affirmed and later denied, or vice versa.

The paradigm debate does not crucially impinge on all areas of research in the field; in particular, studies of religious cultures and religious social psychology are less centrally implicated in a paradigm shift whose level of analysis is organizational. Nonetheless it is a radical decision to choose to focus on the European experience of religious monopoly or on the American case of religious cacophony as the analytic norm, or paradigmatic situation, of religion.

Researchers in the field agree that sociology of religion should not be sealed off from the rest of sociology. This article is based on the assumption that the field contributes most when it recognizes that its empirical field constitutes a central institutional sphere of U.S. society. The nonexclusive strategy taken here has been to codify a paradigm adequate to the best-documented case—the United States—both so that researchers on that case can better understand their findings and so that the parameters of the case itself can be identified. Thus there has just begun one critical line of research that attempts to specify the determinants of the American religious system. Is the key to American religious vitality given in Tocqueville's analysis of the historically apolitical stance of most U.S. religious groups, the notion that American religion has largely stayed out of politics? (See, e.g., Caplow 1985.) Or is religious pluralism the key, as Finke and Stark (1988) would have it, the sheer variety of religious choices? Or is it deregulation, the lack of either subsidy or state oversight of religious organizations? (See Greeley 1989, pp. 126–27; Iannaccone 1991; Chaves and Cann 1992.) Comparative institutional research, unburdened of the secularization expectations of the older paradigm, will serve to demystify the concept of American exceptionalism (Tiryakian

1991). Until that has been accomplished, the exception may well be taken as the rule.

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Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology¹

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Recent developments in historical sociology emphasize the centrality of temporality to analysis and explanation. Narrative uses temporal order to organize information about events and to foster their understanding but is insufficiently systematic to substitute for sociological explanation. This article illustrates a new interpretative heuristic for the computer-assisted analysis of qualitative narrative sequences, "event-structure analysis," that infuses narrative with greater rigor and explicitness. Through the analysis of a lynching that took place in Mississippi in 1930, this article shows how event-structure analysis can be used to build replicable and generalizable causal interpretations of events.

INTRODUCTION

The Narrative

On April 23, 1930, an African-American named David Harris was lynched by a group of whites in Bolivar County, Mississippi. Hours before the lynching, Harris had killed a white tenant farmer, Clayton Funderberg, in an argument. Accounts of the actions that preceded Funderberg's death diverge. According to the lynching's chronicler, soci-

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ologist and social reformer Arthur Raper (1933, pp. 94–106), the African-American version is that Funderberg and two white friends went to Harris's home, from which he sold moonshine, and "demanded" liquor. Funderberg allegedly had not paid off previous liquor debts to Harris, so Harris "refused" Funderberg's demand. Funderberg then "threatened" Harris, and Harris "retaliated" by shooting and killing Funderberg (Raper 1933, p. 94; quoted words or phrases, both above and in the next paragraph, are taken directly from Raper). The "white" version of Funderberg's death holds that the three whites went to the home of Harris to confront him with the theft of Funderberg's groceries; the African-American "denied the theft" and killed Funderberg in an ensuing argument.

There is no discrepancy about what subsequently happened. After the incident was reported by Funderberg's friends, a "search party" was organized among the area's white tenant farmers, and two law enforcement officials were notified of the killing. One of the police officers, a deputy sheriff located in Rosedale (one of Bolivar County's two county seats), went to the site of the killing and, according to Raper (1933, p. 94), was "assured" by whites there that a search party was already trailing Harris. The deputy "expressed satisfaction" with how the event was being handled and returned to his office. Thereafter, he did nothing that overtly facilitated or hindered what subsequently happened. The other officer, the sheriff of Bolivar County, was "engaged in court" in Cleveland (the other county seat) and "did not respond" to the call. He, too, did nothing. The mob, however, did act. Using bloodhounds, several hundred members of the "search party" tracked the African-American, Harris, throughout the night and finally captured him the next morning when his hiding place was "revealed" by another African-American. Harris was then taken a few miles to the Mississippi River levee, tied to a tree, and shot to death. The African-American who disclosed where Harris was hiding was reported to have been "associated" with other bootlegging interests and was allegedly later killed by "the Harris crowd" in a vengeance killing (Raper 1933, p. 95).

The Historical Context

Bolivar County is located in the Mississippi Delta, an extremely fertile section of land long devoted to the growing of cotton, first by slaves and then, by the time of the lynching, by impoverished black and white tenant farmers increasingly in competition for work. More than 93% of African-American farmers and three-fourths of white farmers in the county were tenants or sharecroppers. Sixty years ago the county was overwhelmingly rural, African-American (75% of the population was

black) and, for the vast bulk of the population, both black and white, poor (Raper 1933, pp. 98–103). White supremacy and King Cotton reigned effectively unchallenged, and lethal violence against African-Americans was an accepted way of life: from Reconstruction to 1945, Bolivar County witnessed a total of 13 lynchings of blacks (McMillen 1989, p. 231).

In 1930, then, Bolivar County typified widely held images of the “savage South.” It was a South accustomed to the lynching of African-Americans: 534 black men, women, and children were lynched between the years 1882 and 1951 in Mississippi *alone* (Whitfield 1989, p. 5). In just five Deep South states (Louisiana, Mississippi, Alabama, Georgia, and South Carolina), almost 3,000 African-Americans were lynched during the period from Reconstruction to World War II (Beck and Tolnay 1990). Most of these lynchings shared common features and a general, stagelike patterning: an allegation of an African-American crime or some other form of deviance from white supremacist norms, the formation of a lynch mob, a search or a jail break-in, capture, and then sadistic rituals, either before or after the actual lynching (Williamson 1986, pp. 120–26; Shapiro 1988, pp. 30–31; see also Raper 1933, pp. 26–39). David Harris’s death at the hands of a white lynch mob thus was one among literally thousands of hideously frequent and broadly similar incidents characterizing the Jim Crow South. But it also contains some features—the racial betrayal and vengeance killing, for example—that appear to be quite rare and several other contingencies that powerfully govern what actually happened.

Attempts to understand the lynching of David Harris as both a historically singular event and as an instance of a class of historically repeated events evokes fundamental methodological issues in analysis and explanation.²

1. How, for example, can the processual quality of the lynching—that is, its narrative order and temporal sequences of actions—be analytically harnessed for explanatory purposes? How can inferences grounded in the event’s sequentiality be rendered replicable?
2. How can comparative knowledge about other lynchings be employed in the analysis of this particular lynching?
3. How can the historical and structural *contexts* of the lynching be used to help account for the *actions* in the lynching? How can these actions inform understanding of change or continuity in these contexts?

² An event may be defined as “a distinguishable happening, one with some pattern or theme that sets it off from others, and one that involves changes taking place within a delimited amount of time” (Conkin and Stromberg 1989, p. 173). Sociological discussions of events are contained in Abrams (1982), Abbott (1990), Griffin (1992), and Sewell (in press).

4. How can this particular lynching be generalized without sacrificing its historical specificity and context?

I will address these issues by discussing, in the first section of this article, what narrative is and how it uses temporal sequence to advance understanding. I argue that narrative is both essential to the sociological analysis of historical events and successful in providing certain kinds of explanations, but unsatisfying as an explicit and replicable causal framework. In the second section, I discuss how narrative may be merged with other forms of causal reasoning so that its strengths are analytically exploited and its weaknesses moderated. Here I lean especially hard on Weber's ([1905] 1949) formulation of "causal interpretation" and his brief for the use of historical counterfactuals and E. P. Thompson's (1978) insistence on "historical interrogation" as the basis of sound historical thinking. In the third section, I briefly identify several methods of formal analysis of narrative sequences, paying particular attention to the strategy known as "event-structure analysis" (Heise 1988, 1989). The power of any methodology to clarify thinking and sharpen analytic logic is most profoundly observed when that methodology must grapple with pressing substantive questions. Therefore, I next put the entire framework to the test through a detailed and substantively grounded analysis of the lynching of David Harris. Finally, the article concludes with an assessment of event-structure analysis and a discussion of several of its implications.

NARRATIVE, TEMPORALITY, AND EVENTS

Lawrence Stone (1979, p. 3) offers a succinct definition of narrative: "[It is] the organization of material in a chronologically sequential order and the focusing of the content into a single coherent story, albeit with subplots." Narratives are analytic constructs (or "colligations") that unify a number of past or contemporaneous actions and happenings, which might otherwise have been viewed as discrete or disparate, into a coherent relational whole that gives meaning to and explains each of its elements and is, at the same time, constituted by them (McCullagh 1978; Abbott 1990; Griffin 1992). Narratives are made up of the raw materials of sequences of social action but are, from beginning to end, defined and orchestrated by the narrator to include a particular series of actions in a particular temporal order for a particular purpose.

The intelligibility of narrative explanation.—Narrative explanations have a characteristic, "inherent" logic (Abbott 1992, p. 445) based on the sequential connectedness and unfolding of action. Narratives have a beginning, then a series of intervening actions, and then an end arrived at nonarbitrarily, as a result of preceding actions that emanated, ulti-

mately, from the beginning of the story or from various contingencies logically integrated into the story. To locate an action in the sequence of a narrative and to link that action to the narrative's previous actions, for example, is one way to understand what "caused" the action and thus to "explain" its occurrence (Danto 1965; Dray 1985; Abell 1987). Furthermore, when an action is linked to prior and subsequent actions in the narrative, one can comprehend its character and function in the entire temporal sequence; that is, how the action displays and furthers the unfolding of the event. Narrative, in sum, captures and reflects the methodological and historical significance of an action's temporal order: "Any historical moment is both a result of prior process and an index towards the direction of its future flow" (Thompson 1978, p. 47).

When all such actions constituting the event are so linked, the event itself is "explained" or "understood" because the event—or more precisely the narrative construction of the event—is but a configuration of "elements in a single and concrete complex of relationships" (Mink 1970, p. 551). Through the cumulative succession, connectedness, and holistic configuration of the event's actions, moreover, the narrative's coherence and unity are achieved and its central theme defined, refined, and exhibited. This allows the reader to follow the reasoning and story emplotted in the narrative (Gallie 1964) and, more generally, imbues narrative with a unique form of intelligibility (Mink 1970).³

Narrative and sociological explanation.—Andrew Abbott (1991, 1992; Abbott and Hrycak 1990) persuasively argues that the processual nature of narrative is essential to much of sociological inquiry. In particular, narrative, in focusing on temporality and social action, promises deep theoretical knowledge about the mutually constitutive interplay of agency and social structure, a dynamic continuously occurring *in* time and *through* time (Giddens 1979; Abrams 1982; Sewell 1992). This reciprocal process, labeled "structuring" by Philip Abrams (1982), is seen through the prism of unfolding historical events. Events, then, are our points of access to structuring (Abrams 1982, p. 191), and narratives are how we describe, reconstitute, and comprehend events.

Sociological explanation of how and why an event unfolds as it does requires a type of causal logic that is grounded in "time" and in distinc-

³ The merits of narrative are hotly debated among historians and philosophers of history, and the literature is vast and contradictory (see, in addition to those cited, e.g., Ricoeur 1979; Elton 1983; White 1984; McCullagh 1987). Three issues seem to be at stake. The first is the degree to which narrative is an acceptable form of explanation. The second pertains to the truth content of ostensibly factual historical narratives. The third has to do with narrative as a form of rhetoric (e.g., Reed 1989; Richardson 1990). These are important issues for sociologists as well (see, e.g., Abrams 1982, pp. 300–35), and I address some of them in the text and in the Appendix.

tively temporal processes (Abrams 1982, p. 302; Aminzade 1992). Most sociological explanations are comparative and generalizing, not temporal, in their logic (Lieberson 1985; Abell 1987). They rely on logical comparisons of a few cases, analysis of statistical regularities of many cases, or logical subsumption of particular cases under broader historical generalizations and theoretically general laws (Abbott 1991; Griffin 1992). Narratives, on the other hand, are intrinsically temporal in both construction and explanatory logic.

Narrative explanations take the form of an unfolding, open-ended story fraught with conjunctures and contingency, where what happens, an action, in fact happens because of its order and position in the story. Narrative therefore permits a form of sequential causation that allows for twisting, varied, and heterogeneous time paths to a particular outcome. In narratives, we can see how the cumulative consequences of past actions increasingly constrain and limit future action. This notion of "path dependency" (Aminzade 1992) can be used to examine the determinants of key actions at any given historical moment, counterfactually explore actions and choices not taken, and help explain why sequential paths are sustained through time. We also see the "emergence of novelty" in narrative (Porter 1981, p. 34), those contingent, unpredictable acts, often with big consequences (Gould 1989, p. 284), that are nonetheless explicable in light of temporal ordering and connectedness. Thus through the way it organizes information and fosters understanding of sequentially unfolding action, narrative encourages, even coerces, far more explicit deployment of the sort of temporal causation envisioned by Abrams, Aminzade, and others.

The limits of narrative explanation.—Narrative accounts of historical events are not satisfactory sociological explanations of them. Narratives as stories often appear "merely" descriptive while really presenting, as noted above, an artful blend of explanation and interpretation. Even when narratives are avowedly causal in purpose (e.g., Fredrickson 1981), the criteria used to determine selection, causality, and significance remain tacit. Narratives too often lull readers into accepting the narrator's account as simply a "happening" (Abrams 1982, p. 307). Following, not verifying, the story is essential to successful narrative.

Despite the undoubted intelligibility of what the philosopher William Dray (1985, p. 185) calls "running" narrative explanations, they are, moreover, poor causal explanations of social process. By permitting temporal flow and sequence to carry the explanatory burden, narrative implicitly portrays all actions occurring before time t as direct or indirect causal antecedents of an action at time t . But, as has often been noted (Leff 1971, p. xiii; Marini and Singer 1988), chronological order does not necessarily suggest historical or causal significance. Some early incidents

are surely of no significant consequence in any given event, while others may have no causal impact on some subsequent actions and yet be required for the later occurrence of still other actions. Simply put, the distinction between a temporal antecedent and a causal one is too often obscured in narrative. Narrative sequence, therefore, is but the "primitive" raw material of explanation and interpretation and must "itself undergo radical transformation" in the course of analysis (Thompson 1978, p. 29). Sociological explanation requires that events and their contexts be openly theorized, factual material abstracted and generalized, and the causal connections among narrative sequences established in a way that can be explicitly replicated and criticized.⁴ This requires information and insight not given solely by narrative.

HISTORICAL INTERROGATION AND CAUSAL INTERPRETATION

Narratives must be "unpacked" (Abrams 1982, p. 200) and analytically reconstituted to build a replicable causal interpretation of a historical event. Knowledge of an event's temporality, although too often inadequate for causal purposes, is invaluable because it allows the analyst to pose the basic historical question asked of any set of narrative sequences constituting an event: What is the causal influence of a temporal antecedent on what happened later in an event?

I suggest, as have others, that answers to this question are best aduced through a synthesis of different kinds of reasoning and knowledge. These range from the theoretically deductive and historically general to the historically contextual and particular, from the temporal to the culturally interpretative (Weber 1949; Thompson 1978, pp. 25-50; Porter 1981). The most powerful synthesis of forms of knowledge comes through posing and responding to historically counterfactual questions and situations. I will discuss counterfactuals below and indicate how wrestling with them merges general and particular ways of thinking about historical events.

⁴ This assertion hinges on two premises. First, more systematic thinking about a problem typically yields better inferences. By "systematic" I do not mean quantification, statistical analysis, theoretical deduction, or many of the other characteristics often associated with the science side of social science. I mean those aspects of analysis listed in or implied by the sentence in the text. Second, most sociologists wish to understand how others explain the real world, however one defines "explanation," and are dissatisfied when theories are covert, crucial assumptions are not made explicit, and inferences are based predominantly on the idiosyncratic insight or intuition of individual scholars. This is not a plea for formalism as an end in itself; there may be very good reasons to avoid presenting the more formal dimension of sociological inquiry to readers.

Facts and counterfactuals.—Weber (1949) argues that counterfactuals are an essential tool in the analysis of events because the “concrete event” (p. 165) is too complex to subsume under causal generalizations or theoretical laws. When counterfactual “what if” questions are posed, the researcher conceptually isolates and abstracts facts from their historical concatenations and asks whether their absence or modification would have altered the course of the event as it was recorded (i.e., narrated). If the answer is yes, the fact is judged both essential to the historical configuration as it “actually” happened and a significant historical cause of what followed (Weber 1949, pp. 166, 171, 180).

The attribution of historical significance is a vital step toward explaining what happened because explanations of the factual connote explanations of what did not happen (Moore 1978, pp. 376–97). What did not happen in the case of David Harris was that the lynching was prevented. It was not prevented because of the way particular actors acted and refrained from acting. To understand the actions that kept the lynching from being averted is to explain the lynching itself. By aiding analysts to understand what could have happened and why it did not happen, counterfactuals help them understand what factually happened and why it happened as it did (see Moore [1978] and Zeitlin [1984] for examples of excellent research premised on historical counterfactuals).

Counterfactual questions and their answers are sometimes thought to be mere historical “figments” that reduce history proper to “quasi-history” (Redlich 1965, pp. 485–87).⁵ This objection undeniably exerts restraining force. For example, if, in my attempts to understand why Harris killed Funderberg, I had counterfactually assumed that the Mississippi of 1930 was not white supremacist to its core, I would have had to envision a Mississippi that did not exist and had never existed up until that point in history. That Mississippi is a fantastic implausibility, and any answers adduced from this counterfactual world would indeed have been historical figments.

But some critics of counterfactual reasoning, such as Jon Elster (1978, pp. 175–223), concede that counterfactual questions are logically “assessable” and “legitimate” (p. 191) under certain limiting circumstances. The most crucial condition is that a contrary-to-fact historical alternative,

⁵ A general philosophical defense of counterfactuals rests on the premise that all explanations inevitably imply, and thus must inferentially support, their own counterfactuals. The utility of a particular explanation, therefore, depends on the plausibility of its implied counterfactual (Moore 1978, p. 377; Lieberman 1985, pp. 45–48; Hawthorn 1991, p. 14). If the counterfactual world necessarily posited by an explanation is not plausible, the explanation should not have been advanced.

a "possible world," is conceptually and empirically quite close to the "real past" (see Appendix). Geoffrey Hawthorn (1991, p. 158) imposes similar conditions on the plausibility of counterfactual reasoning: possible worlds should (a) start from the real world as it otherwise was known before asserting a counterfactual, (b) not require us to "unwind the past," and (c) not unduly "disturb" what we otherwise understand about the actors or their contexts. To return to the counterfactual Mississippi envisioned above: a racially tolerant Mississippi in 1930 disturbs virtually everything we know about Mississippi's past, and it would require us to unwind two centuries of racial apartheid and national economic history. It is therefore not an analytically assertable counterfactual condition. Counterfactuals are plausible, in short, only if the posited historical alternatives are "objective possibilities" (Weber 1949, p. 164) in the particular historical context—the real past—housing the action or event subject to counterfactual interrogation.

Objective possibility.—How can objective possibility be determined? Within wide limits, it is based on general theoretical and historical knowledge (Hawthorn 1991, p. 78). In the Deep South of the 1930s, for example, African-Americans generally could not organize and act collectively to stop a threatened lynching. They were denied sufficient social space and political opportunity for antiracist mobilization, and they were denied adequate resources to resist costly attacks against such mobilization, even if it were otherwise possible. Moreover, without the right to vote, they generally had no electoral clout with which to sanction politically those public officials who, overtly or otherwise, permitted lynching. These descriptions are historical generalizations suggesting some of the limits of objective political possibilities available to blacks in that region at that time.

Counterfactual possibilities, however, must be "concrete alternatives and specific to concrete situations" (Moore 1978, p. 377). To establish more precisely the possibility for and limits to action in a specific event, generalizations must be challenged by and augmented with information on the event's particularities. These, in turn, are apprehended through knowledge of the event's actors, its immediate context, and how both unforeseen contingencies and unfolding path dependencies facilitate and hinder the possibilities for future action. Once David Harris killed Funderberg, for example, certain future actions (such as Harris's acting routinely) were permanently precluded. Given what had happened previously to these particular actors in this particular historical setting, these actions were not objective possibilities. Finally, empathetic insight into particular actors' understandings and the "imaginative re-enactment" (Beer 1963) of their action helps the analyst determine how actors viewed

the happening and its context and the possibilities for their own agency, given those understandings (Weber 1949, p. 164–88; see also Elster 1978; Leff 1971; Thompson 1978; Hawthorn 1991). Thus what we know of the real past and hence of objective possibility is comprehended through integrating what we know of the general and the particular.

Answering counterfactual questions.—Answers to counterfactual questions also merge particular and general knowledge and reasoning. When a plausible counterfactual question is posed, a series of other questions is also implied or asked. Some of these provide possible answers to the counterfactual question. For example, What is *theoretically* expected to follow from this (counterfactual) action or condition? What has *generally* been the consequence of this action at the time and place it occurred? How can *comparable* and *analogous* events aid our understanding of this particular counterfactual condition? How do these kinds of actors *typically* respond to these actions? What has been the consistent *pattern* of action of this actor? and How does that help one judge what her or his reaction to the counterfactual situation might have been? Other questions spurred by the counterfactual challenge the answers adduced from the first set of questions. How well, for instance, do the theoretical, historical, and interpretative generalizations elicited above mesh with what is known of the particular event and its context, sequencing, and actors? In answering counterfactual questions, then, general theoretical explanations (Kiser and Hechter 1991), explanations in principle (Watkins 1952), and empirical generalizations are often evoked. But they are culturally and historically contextualized and tested against the particularities and sequences of the event (Thompson 1978, p. 46).

A crucial example from the analysis that will follow shows this kind of self-interrogation in practice. As the lynching of David Harris unfolded, neither Bolivar County law enforcement officer acted to prevent what was happening. Their nonaction in this regard can be a historically significant and morally responsible cause of the lynching only if their (contrary to fact) intervention was an objective possibility that was allowed, deliberately or otherwise, to lapse unrealized. Was intervention possible?

In response to this question, I would first turn to what is known generally of the actions of other southern police officers confronted with comparable events. A large percentage of these officers, including many in Mississippi, did in fact stop lynchings (see below). Thus the *general pattern* of their interventions leads me to conclude tentatively that active intercession by the Bolivar County officers was a distinct possibility. Although clearly grounded in a valid historical generalization, my initial judgment may nonetheless be incorrect. I must also ask myself if there were circumstances *peculiar* to this particular lynching, such as the offi-

cers' ignorance of what was happening, that precluded the possibility of their action. There is no evidence that such was the case in this lynching; most important, we know from prior actions in the event and the connectedness of those actions to subsequent actions that both officers had requisite knowledge of an interracial killing. Thus the generalization that provided a provisional positive assessment of objective possibility is not only not contravened by the particulars of the event, it is strongly supported by the lynching's precise unfolding and sequencing.

Posing the counterfactual, therefore, first forced me to synthesize general and particular kinds of reasoning and then allowed me to infer that a contrary-to-fact action, legal intervention, was an objective possibility in the lynching of David Harris. This is a plausible counterfactual because the possible world it posits—a world in which the Bolivar County officers acted to stop the lynching—was quite close indeed to Mississippi's real past and neither disturbs nor requires us to unwind much of that past.

Now to the second important counterfactual question. Would the intervention of the law have significantly altered what subsequently happened? Probably, though, as I will discuss in greater detail below, inferences about this are difficult because the requisite generalization is murky. We do know, however, that actions of police officers prevented 13 lynchings in Mississippi during the years 1930–32 (Raper 1933, p. 473–79) and literally hundreds more throughout the Jim Crow South. Here I use comparable events as “historical counterexamples” (Martin 1979) to answer, again tentatively, the counterfactual question. Does this particular event exhibit any evidence, such as the use of subtle chicanery or massive force by the mob, suggesting that the lesson learned from the counterexamples is misapplied here? No, there is no evidence of any such particularity of the event or its context. Again, then, the positive answer provided by historical counterexamples was tested against the event's specificity before being finally accepted.

Through positing possible actions and consequences of varying likelihood, counterfactuals therefore transmute one aspect of what is known, observed temporal sequence, into historical questions and then synthesize general and particular knowledge into answers to those questions. Causal interpretations, I suggest, are built, brick by brick, by answering factual and counterfactual questions about historical sequences. What is required, then, is some procedure that facilitates the “interrogation” (Thompson 1978, pp. 25–50) and “cross-examination” (Bloch 1953, p. 64) of narrative sequences and thereby aids the analyst in the extraction and marshaling of evidence of causal significance from them. This is precisely what event-structure analysis promises.

MODELING NARRATIVE SEQUENCES WITH EVENT-STRUCTURE ANALYSIS

Several approaches to the formal qualitative analysis of narrative sequences and actions have been developed (see Abbott's [1992] review). Peter Abell's (1987) "comparative narrative analysis" traces the sequences in individual narratives with an algebra of intentional action and intended and unintended consequence. The structure of the logic in the narrative is then generalized and systematically compared with the logical structures of other narratives. Andrew Abbott's (1992) procedures categorize narratives through the use of unidimensional and multidimensional scaling. Abbott's concern in analyzing repeated events is also with the "generic" or typical narrative (1991, 1992).

These two approaches have much to commend them, and the utility of Abbott's has already been demonstrated (Abbott and Hrycak 1990). Both, however, are directed toward objectives that differ from mine. Neither Abell nor Abbott appears much interested in constructing Weberian-type causal interpretations. Abell (1987), for example, places great, perhaps even indiscriminating, weight on sequence per se, while Abbott (1991, p. 228) finds "abstracting 'causes' out of their narrative environments" seemingly impossible in principle. Given narrative's configurational, conjunctural character, Abbott's demurral has force. But not analyzing narrative from a causal perspective sanctions by default the defects, discussed above, of accepting temporal flow as the basis of explanation and the narrator's construction of the event as *the* happening. This is unacceptable *if* the purpose of the analysis is to construct a causal interpretation.

A third possibility, which is used here, is event-structure analysis (ESA), recently developed by David Heise (1988, 1989). Heise explicitly advances ESA and its associated computer program, ETHNO, for both causal and interpretative purposes (Heise 1988; Corsaro and Heise 1990). It was developed to study cultural routines and the subjective representations of reality (Heise 1989, p. 139) and was influenced by developments in cognitive anthropology as well as by rational choice theory. Event-structure analysis and ETHNO have features that render them especially appropriate for developing causal interpretations of historical events.

First, ESA forces the analyst to replace temporal order with her or his "expert judgment or knowledge" about causal connections. It does this by quite literally transforming a chronology of actions into a series of "yes/no" questions where the analyst/expert is asked if a temporal antecedent ("or a similar event") is required for the occurrence of a subsequent action. Questions of this sort are essential to circumvent one of the main weaknesses of "running" narrative explanation. To again take

the example of the Bolivar County lynching, I was asked by ETHNO if Funderberg's threat to Harris required Harris's prior refusal of Funderberg's demand for liquor. My response, "no," rested on my understanding of the practice and context of white supremacy in the Deep South and generalizations about class differences in the expression of racism by whites there. The answer clearly reflected a causal judgment, not a temporal relationship. With this response, therefore, I have begun to unpack the event and causally reconstitute it, and understanding the event has begun to surpass the cognitive act of simply following the flow of the narrative.

Second, ESA's elicitations maintain fidelity with the interrogatory spirit undergirding much historical reasoning. This in turn naturally leads the analyst to "question" the sequences about their connectedness and causal significance and to posit counterfactual questions and their answers.

Third, its logical foundations in the theory of production systems (Fararo and Skvoretz 1984) parallel the understanding of historical events as configurational, contingent happenings characterized by "the emergence of novelty" (Porter 1981, p. 34). This is most clearly seen in the view in ESA of how action is governed in a production system: "If a certain configuration of conditions arises, *then* a certain production occurs. . . . Productions have natural consequences—they cause changes in conditions, and these results also can be phrased as if-then rules: *if* a given production occurs, *then* condition A changes from state x to y. . . . In general, the production system approach permits representation of knowledge about verbally-defined events in models that can generate new and meaningful sequences of events" (Heise 1989, p. 141; emphases in original).

Fourth, the inferences reached through ESA, though often interpretative, are strictly replicable. Critics have precise knowledge of the causal imputations and the reasons for them and can directly challenge any aspect of the analysis, from the selection of actions to be analyzed to their imputed significance and causal connectedness.

How ETHNO works.—The analyst first prepares a chronology of actions which, in the analyst's mind, define the event. While ETHNO offers no direct assistance at this point, preliminary event-structure analysis often helps the analyst to detect weaknesses in the chronology and to refine it in various ways. The chronology is then entered as input into ETHNO, where it is then reformulated as a series of questions about the causal connections among actions constituting the chronology. The responses to ETHNO's questions are displayed as a "directed" or causal diagram of the logical structure of action underlying the event's narrative or chronol-

ogy.⁶ The diagram, finally, is the event structure and represents the analyst's interpretation of the causal connections among sequences constituting the chronology.⁷

The causal logic reflected by the diagram is then tested for consistency with a set of logical constraints or rules about how action can proceed that is built into ETHNO. The most telling constraints (a) limit when an action is allowed to occur (i.e., it must be "primed" by the occurrence of a temporal prerequisite); (b) limit when an action can reoccur (i.e., its antecedents must be repeated, and its causal efficacy must be used up or "depleted" by a consequence); and (c) limit the causal efficacy of an action to a single consequence. These logical rules can be deliberately relaxed by the analyst.

If the analyst opts to circumvent the constraints (as I repeatedly did in the analyses that follow) rather than exercise some other solution to logical inconsistencies (e.g., reconfigure the diagram, and thereby alter one's understanding of logic of action), the theoretical constraints embedded in ETHNO are effectively mooted. One need not, therefore, accept the theories of production systems or rational action to structure narrative sequences with ESA. Indeed, event-structure analysis could be used to illustrate or test virtually any processual theory.

Event-structure analysis and ETHNO also allow explicit generalization of the initial (or "concrete") configuration. Here the analyst abstracts from the concrete event structure in two ways. First, actions which, in the expert's judgment, are embellishments on or otherwise incidental to the main paths of action or without imputed cultural or historical significance can be dropped from the "abstracted" chronology. Second, actions that are retained for further analysis are conceptualized as instances of theoretically general sequential actions. The computer program, ETHNO, then interrogates the expert about the causal relations between these

⁶ The software views the second word in each sequence as the verb connoting action and assigns it a three-letter abbreviation that appears in the diagram of the event structure. Should the same action verb appear more than once, ETHNO abbreviates the second appearance by using a number, beginning with "1" and moving to "2," etc., in lieu of the original second letter (see "Sho" for "shot" in action 5 and "S1o" for "shot" in action 22 in table 1). The abbreviations can be changed at any point during or after the analysis to those more to the analyst's liking.

⁷ The ETHNO program elicits only direct causal connection. If action A is judged to be a prerequisite of action B, and action B is judged to be a prerequisite of action C, ETHNO will not ask about (and thus will not automatically diagram) the relationship between A and C. The program knows, by logical implication, that A, through its causal influence on B, is required for C. The analyst, however, can directly relate A and C, so that C is portrayed as having two causal prerequisites, A and B. The program permits many other modifications of the event structure as well.

generalized actions just as it did about the concrete actions. As the analyst answers these questions, ETHNO, again as before, constructs a diagram of action, this time structuring the theoretically abstracted actions at a higher level of generality and parallel to the concrete event structure. The general event structure is subsequently assessed for logical inconsistencies, both in relation to ETHNO's internal logic *and* to the causal imputations embedded in concrete event structure. Logical contradictions between the general and the concrete event structures may necessitate altering either structure or both of them or relaxing ETHNO's rules about action.

What ETHNO does not do is answer its own elicitations. Causality is not "discovered" through its use. The analyst, not the software, possesses the knowledge needed to structure and interpret the event. What ETHNO does—and, at root, all that it really does—is relentlessly probe the analyst's construction and comprehension of the event. By forcing the user to be precise and meticulous about the construction of historical narratives, to reason causally about their sequences, and to be clear about the bases of causal judgments, ESA and ETHNO lay bare the investigator's understanding so lucidly—indeed starkly, as a diagram of the logic of action—that insights into causal significance are intensely sharpened, and problems of causal interpretation are prominently displayed.

THE LYNCHING OF DAVID HARRIS

The exact chronology of the lynching used in the concrete event-structure analysis is presented in table 1. (I use the African-American account initially.) This is the input for ETHNO. While I adhere very closely to Raper's (1933) original narrative, including the use of proper names, I refrain from using some of his linking words and phrases, such as "because" and "due to," in order to avoid prejudging assessments of causality. Two actions (15 and 19) actually occurred prior to their position in Raper's account, and their order in the chronology is changed to reflect this. I have added one action (6) to the chronology because it is an implicit action that is instrumentally necessary for the occurrence of later actions. The last incident in the chronology (23) is an unverified allegation and is labeled as such. (The program does not care if the actions were implied or alleged but the analyst should.)

Concrete event structure and interpretation.—I was asked a total of 112 questions in this particular analysis.⁸ The diagram of the concrete

⁸ The number of questions ETHNO asks is a function of both the number of sequences in the chronology and the imputed connections among them. More time-linear causal sequences generate fewer questions. The entire set of questions, answers, and reasons for the answers are available on request.

Narrative, Event-Structure Analysis

TABLE 1

CHRONOLOGY OF BOLIVAR COUNTY LYNCHING, APRIL 23, 1930

| ETHNO Abbreviation for Action | Order of Action in Chronology* | Description of Action |
|----------------------------------|--------------------------------------|---|
| Eng | 15† | Sheriff (W) engaged in court. |
| Wen | 1 | Funderberg/others (W) went to home of Harris (B). |
| Dem | 2 | Funderberg/others demanded liquor. |
| Ref | 3 | Harris refused demand. |
| Thr | 4 | Funderberg threatened Harris. |
| Sho | 5 | Harris shot Funderberg. |
| Fle | 6‡ | Harris fled the scene. |
| Rep | 7 | Others reported the killing. |
| For | 8 | Search party formed. |
| Rec | 9 | Deputy (W) received news of killing. |
| Vis | 10 | Deputy visited site of the death. |
| Rea | 11 | Townspople assured deputy that search party was already trailing Harris. |
| Sat | 12 | Deputy was satisfied with handling of affair. |
| Ret | 13 | Deputy returned to his office. |
| Cal | 14 | Sheriff called about incident. |
| Not | 16 | Sheriff did not respond to call. |
| TRA | 17 | Search party tracked Harris. |
| REV | 19† | Black revealed hiding place of Harris. |
| CAP | 18 | Search party captured Harris. |
| Too | 20 | Mob took Harris to levee. |
| Tie | 21 | Mob tied Harris to tree. |
| Slo | 22 | Mob shot Harris to death. |
| Kil | 23§ | Harris's friends (B) killed black who revealed Har- ris's hiding place. |

NOTE.—Derivation of ETHNO abbreviations given in text at n. 6; (W) = white; (B) = African-American.

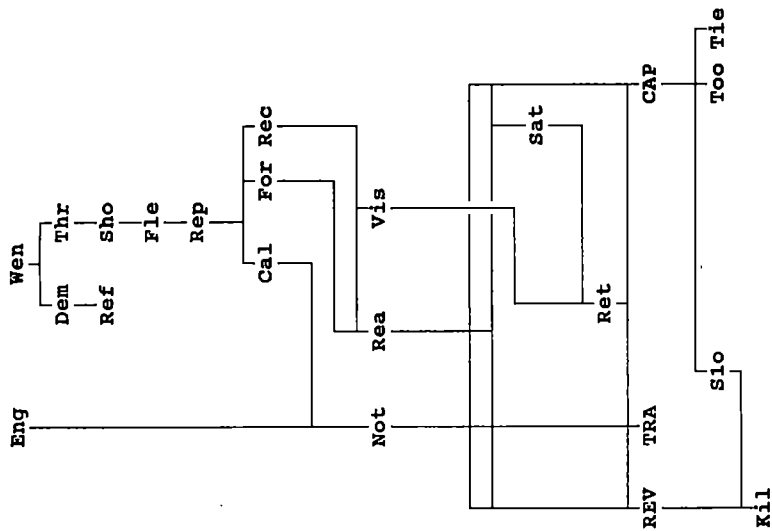
* Chronology constructed from account in Raper (1933, pp. 94–95).

† Action occurred here in event, but elsewhere in Raper narrative.

‡ Implicit action.

§ Alleged action.

event structure of the lynching is presented in figure 1. Using numbers from the chronological account in table 1, I begin the discussion of the interpretation at the point of mob formation (For 8). After the Rosedale deputy learned of Funderberg's death (Rec 9), he went to the site of the killing (Vis 10) and was told by white townspeople there that the search party was trailing Harris (Rea 11). The ETHNO program asked of this sequence: "Does 'deputy satisfied with handling of affair' require 'townspeople assured deputy search party already trailing Harris'?" I answered yes, and ETHNO drew a causal link between "Rea" and "Sat" (see fig. 1). If Raper is correct in his description of these actions (there is no



ABBREVIATIONS:

Cal: Sheriff called about incident

CAP: Search party captured Harris

Dem: Funderberg/others demanded liquor

Eng: Sheriff (W) engaged in court

Fle: Harris fled the scene

For: Search party formed

Kil: Harris's friends (B) killed black who revealed Harris's hiding place

Not: Sheriff did not respond to call

Rea: Townspeople (W) assured deputy that search party already trailing Harris

Rec: Deputy (W) received news of killing

Ref: Harris refused demand

Rep: Others reported killing

Ret: Deputy returned to his office

REV: Black revealed hiding Harris's hiding place

S1o: Mob shot Harris to death

Sat: Deputy satisfied with handling of affair

Sho: Harris shot Funderberg

Tie: Mob tied Harris to a tree

Thr: Funderberg threatened Harris

Too: Mob took Harris to levee

TRA: Search party tracked Harris

Vis: Deputy visited site of the death

Wen: Funderberg/others (W) went to home of Harris (B).

FIG. 1.—Bolivar County, Mississippi, lynching concrete event structure

independent evidence one way or the other), the deputy's "satisfaction" resulted directly from the "assurance" he received from whites at the scene of the killing. Both "assurance" and "satisfaction" symbolize the racist aspirations of the deputy and those whites who told him of the mob's activity, and both disclose the ideological affinity between the officer and the nonmob whites, on the one hand, and members of the lynch mob, on the other.

The next action is "deputy returned to his office" (Ret 13). The program asked if this action required his prior "satisfaction" with what he had heard from the white townspeople (Sat 12). I again answered yes, and again ETHNO causally connected the actions. Had the deputy not been satisfied with the fact that the mob was trailing Harris, he likely would not have returned to his office and done nothing. Instead, he would have acted differently, possibly by attempting to stop the mob, or by calling for assistance, or by searching for Harris himself. So given what the Rosedale deputy learned from the other whites about the mob's existence and activities, his inaction signifies that he consciously chose not to try to prevent a probable lynching.

If, contrary to what actually happened, the deputy had attempted to stop the mob, might the lynching have been averted? Historical generalizations are shaky on this crucial point. On the one hand, lynch mobs often prevailed against even determined antilynching actions by authorities (including the use of armed force; see Raper [1933], McGovern [1982], McMillen [1989], Wright [1990]). On the other, a large number of lynchings were in fact averted by the actions of the city, county, and state police. While no firm count is ever likely to surface, estimates of the number of averted lynchings suggest that they were at least as frequent as completed lynchings, possibly even double the number.⁹ During the years 1930–32, for example, there were 34 lynchings in the South. In this same time period, 127 attempted lynchings were prevented, 92 (or 72%) of them in the South and involving African-Americans as intended victims (raw data are from Raper [1933, pp. 473–79]). In each of these 92 instances the lynching of an African-American was prevented by white law enforcement officials acting to obstruct the mob or otherwise avert the lynching. Thirteen of these averted lynchings occurred in Mississippi, several in Delta counties virtually indistinguishable from Bolivar. Seven lynchings were completed in the state during the same time period. Given

⁹ Using data gathered by the Tuskegee Institute (now Tuskegee University), Jesse Daniel Ames (1942, p. 11) reported that from 1915 to 1942, 762 lynchings were completed while 1,476 were prevented. Raper's (1933, p. 484) count, which is based on the same information, indicates there were 648 completed lynchings and 688 prevented lynchings from 1915 to 1933.

what happened in comparable cases and the Bolivar County deputy's clear knowledge of the existence of the mob and of its early activities, his forceful intervention to prevent the lynching thus appears an objective possibility.

It is likely that the whites in the mob knew of the noninterventions by both the deputy and the sheriff, and they probably understood the deputy's absence, at least, to mean what he apparently intended, a deliberate "hands-off" stance in a potential racial lynching. The deputy's "return to his office," then, is interpreted as a direct encouragement of the lynch mob as it tracked and subsequently captured Harris. (See the causal tie between "Ret" and both "TRA" and "CAP" in fig. 1; *ETHNO* capitalizes the abbreviations of all actions, such as "tracked" and "captured," that are caused by *either* one *or* another logical prerequisite; see n. 11). The sheriff was apparently "engaged" in other business (Eng 15) and, because of this, did not respond (Not 16) to the call he received about the lynching (Cal 14; see the connection between "Eng" and "Not"). No direct evidence exists about the sheriff's intention in not responding. Whatever his motivation, or the mob's understanding of his nonintervention, the sheriff's absence nonetheless permitted the political opportunity needed for the lynchers to proceed with their actions (see Tilly 1978). I therefore judged that his "inattention" to the search party's actions (Not 16) was also causally (if unintentionally) responsible for subsequent mob activities up to and including Harris's capture (see the connection between "Not" and both "TRA" and "CAP").¹⁰

White civilians, largely tenant farmers, are also implicated in the mob's activities. By giving assurance to the deputy (Rea), they, along with him, are causally implicated in his actions and their consequences. Pervasive and avowedly violent antiblack sentiment from the white community, however, was not entirely channeled, in my judgment, through its consequences for the actions of legal authorities. If the white population had more extensively defied the mob *and* "indifferent" or racist law enforcement officers, as sometimes happened in Southern lynchings and near-lynchings (Raper 1933; Ames 1942), the mob's activities would have been much more difficult, if not impossible altogether.

Because the assurance (Rea 11) of white racists was logically implied in the deputy's satisfaction (Sat 12) and subsequent return to his office

¹⁰ Raper (1933, pp. 443–67) presents narrative accounts of five averted lynchings in the South. I used ESA to analyze these narratives, each of which is a "historical counterexample" (Martin 1979) to the Bolivar County lynching. In every case, the causal interpretation pointed to some form of legal intervention (e.g., removing the prisoner to a safer jail, calling in the National Guard to protect the prisoner, the showing of armed force against the mob) as the crucial contingency determining the prevention of the lynching.

(Ret 13), and the “return” was judged essential to the mob’s activities, ETHNO did not inquire about a causal connection between the white community’s assurance and the subsequent actions of the mob. I therefore modified the diagram to allow for a direct causal link between assurance (Rea) and the mob’s subsequent activities of tracking (TRA 17) and capturing (CAP 18) Harris. Moreover, any one of the three causal prerequisites of tracking and capturing (that is, assurance from the whites and the nonintervention of the two officials) is, in my judgment, a sufficient cause for their occurrence. *Either* systematically racist legal practices *or* systematically racist civil practices, therefore, adequately licensed racist mob violence in this lynching.¹¹

Racist assurance of white townspeople (Rea) and legal inaction (Ret, Not) are also imputed to be direct causal prerequisites of the act of the African-American who revealed Harris’s hiding place (REV 19; see fig. 1). This is neither obvious nor incontestable. Disclosing the hiding place of a suspect accused of a killing might be sensible in another culture as an indicator of “good citizenship.” That does not seem likely here, where African-Americans were in no real sense citizens of this community and where, on the whole, they felt great apprehension about white supremacist law (Davis, Gardner, and Gardner 1941; Williamson 1986; Shapiro 1988; McMillen 1989). Another possible motivation for this action is pecuniary. While there is no evidence that a reward was ever offered for information leading to Harris’s capture, the economics of the illicit liquor industry may have played a role. Harris’s betrayer allegedly was linked to other bootleggers; by informing on Harris, the informer removed a competitor. Venality, then, may be all that lies behind the racial betrayal.

Although this interpretation cannot be dismissed, it is woefully incomplete because it ignores the reality of black life and white law in Jim Crow Mississippi. That the law generally was not color-blind and was in fact “white law” in both its personnel and its “functioning” is incontrovertible (Zangrando 1980; McMillen 1989; Wright 1990). Law was seldom oriented or implemented to ensure in practice the U.S. constitutional rights of southern African-Americans as citizens of this country. The statistics on averted lynchings notwithstanding, the law all too infrequently even perfunctorily performed its duties in the strictly legalistic

¹¹ The ETHNO program calls this type of causal relationship, where an action is induced by either one or another antecedent, “disjunctive” causation. Its query as to whether action A is “required” for the occurrence of action C suggests that event-structure analysis elicits assessment of necessary cause (Heise 1989, p. 162). But there is an escape clause: what ETHNO actually asks is if action A “*or a similar event*” is required (emphasis added). This invites the imputation of causal sufficiency. *Either* action A *or* its functional alternative, action B, in other words, may be *sufficient* for the occurrence for action C, but the presence of one or the other actions is *necessary*.

manner dictated by white supremacy. African-Americans did not often enough "enjoy" the perverse "opportunity" of a legal lynching as opposed to a mob lynching (Carter 1979; McMillen 1989; Wright 1990).

Many southern blacks fiercely and subtly resisted segregation and its attendant degradations (Rosengarten 1974; McMillen 1989; Kelley 1990) and even, during imposition of Jim Crow laws at the turn of the century, formed armed groups to protect potential lynching victims (Brundage 1990). But other blacks, adopting a survival mechanism that extended beyond acceptance of Jim Crow, aided white vigilantes. Blacks even infrequently joined white mobs in the search for other fleeing African-Americans (McMillen 1989, p. 383). Those actions, like the one here, where one black, doubtlessly with accurate foreknowledge of the narrow range of the almost certainly devastating consequences for Harris, betrayed another African-American hounded by a lynch mob, are fully intelligible only if we understand the structural relationship between African-Americans and white legal institutions.

Whatever the material incentive for the betrayal, and whether or not the particular black who told whites where Harris was hiding knew that the deputy and sheriff had washed their hands of the affair, all African-Americans knew the racist nature of the Jim Crow legal system. Racial betrayal required, daily and cumulatively, racist actions and non-actions, such as that dramatized by the deputy, by agents of the white legal establishment.¹² Unlike the more complex relationship between the white mob and white law, which really allows no firm historical generalization, that between African-Americans and southern "justice" was generalized in daily practice and institutionally anchored in the very fabric of social life. Racial betrayal, then, signified one of the cardinal social relationships, that between law and race, which both defined the South's white supremacist structure and was recreated every time the law failed to treat all of its citizens, black and white, equally.

There can be little doubt that the disclosure of Harris's hiding place facilitated his capture and subsequent lynching. Would this have happened in the absence of the betrayal? There were other racial betrayals that culminated in lynchings (Raper 1933), but such incidents appear rare

¹² This is true whether or not the African-American was forced into his betrayal or was promised some private reward for his knowledge. The absence of an effective legal check on racist action is, in this context, the root condition of both racial coercion and racial paternalism. Finally, I also judged that either popular racism or legal indifference was causally sufficient inducement to racial betrayal (REV) and modified the diagram to allow the betrayal to be disjunctively caused by the occurrence of its three prerequisites (Rea, Ret, Not). My reasons for imputing a direct causal relationship between the racist reassurance of the white townspeople and the racial betrayal is essentially the same as that given for the connection between legal indifference/racism and the betrayal.

indeed. Generally African-Americans were not apprehended by either the law or the mob through action of this sort. Using what happened in comparable cases as the historical standard here, I therefore do not impute a causal connection between the “revealing” (REV) and Harris’s capture (CAP), the lynching rituals (“Too 20” and “Tie 21”), or the lynching proper (S10 22). Rather, I interpret the actual lynching of Harris to be a direct consequence of the mob’s possession of him coupled with their prior motivation and, through the chain of historical regress, their opportunity to kill him.

The racial betrayal (REV 19), however, does exert a consequence of mortal significance in my interpretation, the “vengeance” killing of the informer by the friends of the victim (REV 23; see the causal link between “REV” and “Kil” in fig. 1). I interpret this murder to mean that the capacity for African-American resistance to white dominance, though necessarily muted and only sporadic by the 1930s, was never entirely extinguished. Even in the place with what was likely the most racially oppressive conditions known to “free” African-Americans in this country, the Mississippi Delta, blacks contingently continued to implement, in one of the few ways available to them, solidaristic understandings of social control against members of their own race thought to have gone too far—here, by abetting the interests of a white lynch mob—in their subservience to Jim Crow. Such offenders were punished, sometimes lethally.

Just as the racial betrayal is grounded in the historical context of white supremacy, the solidaristic vengeance killing, too, has deep cultural and structural roots. Again, I point especially to the racist underpinnings of southern law. Because African-Americans were not deemed fully human, their being killed by other blacks was seldom punished to the full extent of the law: white authorities often turned their backs on the murder of blacks by blacks (Davis et al. 1941; McMillen 1989). Raper (1933, p. 105), for example, reports that he heard that it was not until a month after this lynching that Bolivar County courts sentenced an African-American to death for the murder of another black. White supremacist law-enforcement practices thus unintentionally facilitated this expression of black racial solidarity.

Comments on the Interpretation

Although this causal interpretation of white racist vigilante violence and African-American subservience and resistance is admittedly constrained by Raper’s (1933) original narrative, the constraints are quite loose. Neither my interpretations nor my causal claims are his. Raper’s account, as is generally true, “permitted” but did not determine my understanding of

the lynching. Many quite different concrete event structures, several of which are discussed below, can in fact be teased from Raper's narrative. (On the general indeterminacy of interpretation of texts, see Taylor [1979].) When the analysis is compared with the original narrative, we see that the former generated something new. My interpretation is best understood as a selectively emphatic causal and interpretative "unpacking" and reconstitution of the original. It is a precise and explicit consequence of the way I merged the particularities of the lynching with historical generalizations, theoretical knowledge, and information from comparable cases to understand the event's sequences and their connect-edness. Three aspects of the event structure presented in figure 1 ground the interpretation.

First, the event's logic of action indicates how causality is formed by and embedded in the event's temporality even as the "inner connections" of actions transcend sequence *per se*. Some sequences were imputed to have no consequence for ensuing action (e.g., Funderberg's initial demand for liquor, and Harris's subsequent refusal), or to be embellishments on, rather than prerequisites for, a more fundamental action (e.g., carrying Harris to the levee and tying him was not judged to be necessary to the lynching). Main story lines are accentuated, and "subplots" (such as the theme of racial dominance and resistance played out by Funderberg and Harris in their initial interaction) defined and clearly understood as such. Additionally, different patterns of causal relations among actions—simple causal chains (e.g., the actions from "Funderberg/others went to home of Harris" [Wen] through "others reported killing" [Rep]) and conjunctive and disjunctive configurations of prerequisites for actions (e.g., the townspeople's assurance to the deputy [Rea] and the racial betrayal [REV], respectively)—are revealed in a way that is masked in narrative sequence.

Second, the diagram reveals those actions which, in a very precise sense, are most significant to the entire sequence. In figure 1, all important early actions are funneled through a single action, Funderberg's friends' dissemination of the information of his death (Rep). I do not know what Funderberg's friends actually reported, but it is all too plausible to imagine the general racist construction of the incident.¹³ That

¹³ This interpretation is supported by two things. The first is what actually happened to Harris: he was tracked by a mob of more than 200 men with dogs and then shot more than 200 times while the law did nothing (Raper 1933, p. 95). There is only minimal evidence that anyone ever attempted to moderate the mob, or to stop the lynching, or even expressed doubt or qualms about what was happening. The second is that whites constructed an alternative version of the altercation between Harris and Funderberg. Their version, which casts Funderberg as the innocent victim merely trying to undo a wrong done him, most likely emanated from his friends.

action—the “telling” of the first killing—thereafter created three different branching points or paths of action (the formation of the mob [For] and the knowledge of two police officers [Cal, Rec]), each of which established the limits of possible actions still to occur. The final actions, including Harris’s capture and the two deaths, stem mainly from the racist assurances of community whites (Rea) and the racist actions/nonactions and indifference of the police officers (Ret and Not).

Third, the event structure displays the crucial role played by human agency, contingency, and path dependency. If the actions I have identified as significant had been very different in content and meaning (intended and understood) from what they in fact were, the event likely would have been transformed into a nonlynching. Through their action in informing authorities of the first killing, for example, whites introduced two contingencies—the possible involvement of the two law officers—which they could not control and which might have altered the course of the event. But the event’s structure also shows how these possible “lynching-averting” contingencies were circumvented by the actions of both white civilians and the two law officers. The path-dependent consequences of nonintervention increasingly cumulated as the event unfolded, reducing the possibility for alternative choices and actions. By the time the mob had physical control of Harris, the likelihood that the African-American would be lynched was high.¹⁴

Neither knowledge of the event’s initial conditions (including its context) or of its early actions, however, would have enabled accurate prediction of what ultimately happened. David Harris was not destined to be lynched, even after he killed Funderberg, even after the lynch mob was formed. His murder was the contingent outcome of a number of unfolding temporal sequences that, finally, were brought together through conscious choice, purposive action, and unintended consequence (i.e., the sheriff’s nonaction) to create a dense, interdependent pattern resulting in two criminal deaths.

Generalized event structure.—Of the 23 concrete sequences in the Bolivar County lynching, 12 were retained and used in the general event-structure analysis. Table 2 presents those actions and the general concepts constructed from them. The posited epistemic links between concept and indicator are also reproduced at the bottom of the diagram

¹⁴ This claim rests on generalizations reached after reading hundreds of accounts currently held by Tuskegee University of completed and averted lynchings. When new information was introduced into the event or extant information finally accepted as valid, mobs sometimes adjudicated an African-American to be innocent and voluntarily released him or her. But I have yet to encounter an event in which a mob holding an African-American with the evident intent to lynch disbanded “spontaneously” before completing the murder.

TABLE 2
CONCRETE ACTIONS AND THEIR GENERAL MEANING IN THE BOLIVAR COUNTY, MISSISSIPPI, LYNCHING

| Concrete Action | General Meaning |
|---|---|
| Funderberg threatened Harris (Thr) | White intimidation of black (Int) |
| Harris shot Funderberg (Sho) | Black violence against white (Vio) |
| Others reported the killing (Rep) | White construction of racial conflict (Con) |
| Search party formed (For) | Racist organization (Org) |
| Townpeople assured deputy that search party was already trailing Harris (Rea) | White popular support of racist organization/action (Pop) |
| Deputy returned to his office (Ret) | White law support of racist organization/action (Law) |
| Sheriff did not respond to call (Not) | White law support of racist organization/action (Law) |
| Black revealed hiding place of Harris (Rev) | Racial betrayal (Bet) |
| Mob took Harris to levee (Too) | Lynching ritual (Rit) |
| Mob tied Harris to a tree (Tie) | Lynching ritual (Rit) |
| Mob shot Harris to death (Sio) | Lynching of black (Lyn) |
| Harris's friends killed black who revealed Harris's hiding place (Kil) | Black solidaristic action/social control (Sol) |

NOTE.—Abbreviations in parentheses are ETHNO designations.

in figures 2 and 3 (see "Instantiations," ETHNO's way of saying that the concrete actions are "instances" of the generality). The concepts should be understood as generalized sequences, not as static concepts devoid of temporal referent. Criteria for deciding which actions to retain for this stage of the analysis included my assessment of their causal significance in the concrete structure and their general theoretical and empirical relevance to lynchings as a class of events.

Two aspects of these generalizations deserve brief mention. First, two different sets of two concrete actions were each conceptualized as a single general action: the actions of the two enforcement officials (Ret, Not) constituted "white law support of racist organization/action" (Law), and the actions of taking Harris to the levee (Too) and tying him to a tree (Tie) constituted "racist ritual before lynching" (Rit).¹⁵ Second, what appears to be no more than instrumental action in the original narrative, the "telling" of Funderberg's killing by his friends (Rep), becomes in the abstracted sequence an action of central conceptual import, "white construction of racial conflict" (Con). My altered understanding of the *general* relevance of this act stems from its historical significance in the *concrete* event structure, a significance I did not suspect until after the initial analysis.

The general event structure was configured by answering 18 queries about the causal relations among these generalized sequences. In responding to these questions, I strictly followed the causal logic used in concrete analysis. That is, I structured the causal relationships among the generalized sequences according to how I had previously imputed causal connections among the concrete actions serving as "instances" of the generalized actions. The parallel concrete and general event structures are presented in figure 2.

The ETHNO program's assessment of the logical consistency between the two event structures revealed one serious contradiction. The generalized sequential structure in figure 2 indicates that "white popular support for racist organization/action" (Pop) is a causal prerequisite of "white law support . . ." (Law). This is a straightforward generalization of the concrete causal sequence beginning with "townspeople assured" (Rea),

¹⁵ "Local" knowledge and historical generalization are both necessary to impute the general meaning of taking Harris to the levee and tying him to a tree. In 1930, Bolivar County was sparsely populated, overwhelmingly rural, and dotted with frequent large patches of woodlands and vast areas of open fields. The mob could have killed Harris in any number of desolate spots without real fear of being seen even if secrecy were thought necessary, which, from what we know of the event, seems doubtful. So I judge that something other than instrumental reasoning motivated the trip to the levee. Many lynching accounts, moreover, contain actions by whites that are plausibly interpreted as cultural ritual. Given the general pattern of lynching, then, I was looking for ritual and, on the basis of local knowledge of Bolivar County, I found it.

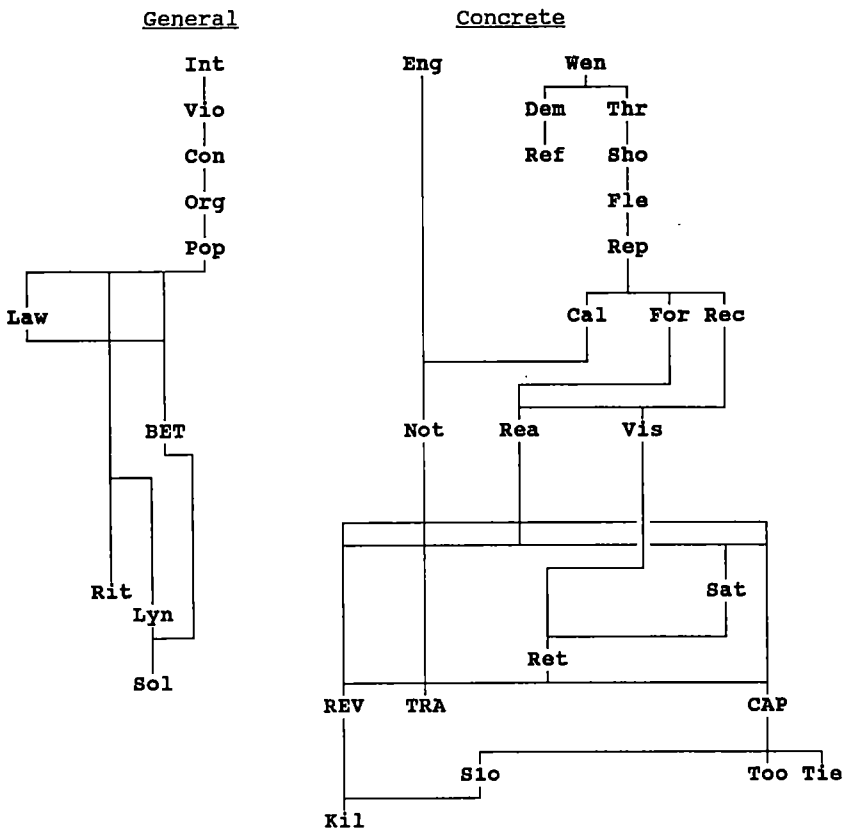


FIG. 2.—Bolivar County, Mississippi, lynching concrete and general event structures.

ABBREVIATIONS:

- BET: Racial betrayal (general)
 Cal: Sheriff called about incident
 CAP: Search party captured Harris
 Con: White construction of racial conflict (general)
 Dem: Funderberg/others demanded liquor
 Eng: Sheriff (W) engaged in court
 Fle: Harris fled the scene
 For: Search party formed
 Int: White intimidation of black (general)
 Kil: Harris's friends (B) killed black who revealed Harris's hiding place
 Law: White law support of racist org./action (general)
 Lyn: lynching of black (general)
 Not: Sheriff did not respond to call
 Org: Racist organization (general)
 Pop: White popular support of racist org./action (general)
 Rea: Townspeople (W) assured deputy that search party was already trailing Harris
 Rec: Deputy (W) received news of killing
 Ref: Harris refused demand
 Rep: Others reported killing
 Ret: Deputy returned to his office
 REV: Black revealed Harris's hiding place
 Rit: Lynching ritual (general)
 Sio: Mob shot Harris to death
 Sat: Deputy satisfied with handling of affair

which is the indicator of "white popular support," and ending with "deputy returned" (Ret), *one* of the two indicators of "white law support." The implication of the general structure in figure 2 is that this sequence is *general* within the event; that is, holds for every concrete sequence. Yet this generalization violates the imputed causal independence between another concrete sequence, that between "Sheriff not responding" (Not), the *other* indicator of "white law support," and "townspeople assured" (Rea). The general and concrete structures logically and inferentially diverge.

Varied solutions to this logical problem are quite complex, requiring close attention to the diagrams in figures 2 and 3. But the possible solutions, and the consequences of these solutions, are worth serious examination because they fortuitously demonstrate several methodological and procedural points: (1) how ETHNO views sequence and causality, (2) the importance of factual detail in constructing theoretical explanations (Stinchcombe 1978, p. 124), (3) how the tension between the general and particular can be exploited to inform each, and (4) how answers to historical questions are really hypotheses to be tested against the entire framework of evidence. Now to the solutions.

The ETHNO program gives the analyst the option of allowing the diagrams to contradict in the manner described above. But this "solution" assumes, as noted above, a generality that does not exist in the concrete structure. Another possible solution would be to assume "sheriff not responding" to have no general meaning, thereby dropping it as an indicator of the general "legal support of racist organization/action" action. Because it would not figure in the general analysis at all, the contradiction would naturally disappear. This, however, belies the causal importance I placed on that action in the unfolding of the concrete event. Or, because the problem at the concrete level is the absence of a causal tie between "townspeople assured" (Rea) and "sheriff not responding" (Not), I could also simply link the two concrete actions, assuming that "Rea" is a causal prerequisite of "Not." This, too, would induce logical consistency. But this solution assumes a connection between actions for which there is absolutely no evidence. It, too, is unsatisfactory.

One alternative that distorts reality less is to sever the general causal connection in figure 2 between the mob's "white popular support" (Pop) and "white legal support" (Law) and simply view them as different forms of mob facilitation rather than as cause and effect. What actually happened (the chronological) need not be generally translated as what must have happened (the logical; Abrams 1982). The absence of a *required* relation, moreover, is consistent with the internal causal heterogeneity of the concrete structure because, with two distinct concrete paths

to legal indifference in figure 2 ("Rec \rightarrow Rea \rightarrow Sat \rightarrow Ret," for the deputy; "Eng \rightarrow Not," for the sheriff), no generality is found there. I opted for this solution (see fig. 3).

By relaxing the causal assumption between the two general sequences ("Pop" and "Law") in the abstract diagram, however, I produced two other, serially dependent logical problems requiring attention. Popular racism is the only prerequisite for legal support in the abstract diagram (see the "Pop \rightarrow Law" link in fig. 2). If the causal connection between the two is relaxed, legal support (Law) has no causal prerequisite and cannot occur when it in fact happened. In ETHNO's terminology, it is not "primed" to occur. I could ignore its order in the generalized chronology and assume the law *generally* exogenous to the actions that preceded it (as I did concretely for the sheriff in fig. 2). That option, however, violates the pretty clear influence that the "assurance" from whites exerts on the deputy's nonintervention in the concrete analysis (fig. 2) and generally portrays the law as outside the framework of social relations and contingencies that, quite literally, elicits a response from legal agents. This is unacceptable given what we know generally of the links of southern law to the white community.

One way around both the problem of priming and the naive view of the structural independence of the law and the white community is to make racist organization (Org) the causal prerequisite of legal support (Law) in the general event structure. I did this, assuming, in effect, that all that was required to kick in (or prime) racist actions (or nonactions) on the part of Bolivar County's white police officers was the existence of the lynch mob (see fig. 3). But this causal imputation, though maintaining temporal fidelity with the original narrative, nonetheless introduced yet another inconsistency with causal claims about concrete actions made in the event structure in figure 2. No causal tie between mob formation (For) and the sheriff's lack of response to the situation (Not) was imputed in the concrete event structure there because available evidence indicates the sheriff was told only that Harris had killed Funderberg and not also told that a search party had been organized. This causal independence is a direct challenge to the altered generalization of the lynching shown in figure 3.

Is there plausible reason to reconsider that imputation? Perhaps. In the three decades preceding the lynching of David Harris, an African-American was lynched, on the average, once every four years in Bolivar County (Raper 1933, p. 105). Even if one or more of these was not known to the sheriff, which seems unlikely, he at least knew the area's general racial norms and dynamics. So it is probable that the sheriff, knowing also that a black killed a white, would have suspected that a lynch

mob probably would be organized. I therefore modified the *concrete* configuration by causally linking the formation of the mob (For) to the sheriff's nonaction (Not; see fig. 3). Logical consistency between the concrete and general structures was then achieved.

This analysis offers unusually rich insight into how the general and the particular are interwoven in causal interpretations of historical events (see Abrams 1982, p. 199). Even as the general is constrained by concrete historical sequences, it challenges understandings of the causal relations among those sequences. In a manner similar to Thompson's (1978, p. 43) "disciplined dialogue" between theory and evidence, ESA requires almost constant movement from the concrete to the general and back again in order to solve logical contradictions between the two and grasp each more fully.

Unraveling the logical inconsistencies between concrete and general understandings proved more than an expository exercise. It also altered the moral dimension of the concrete event and of the sheriff's ostensibly "excusable" nonintervention. In my initial interpretation based on the logic of action diagrammed in figure 1, the sheriff failed to act on important information because he was legitimately engaged elsewhere. The criminal consequences of his nonengagement therefore rested solely with how the white lynch mob used his absence: the sheriff neither anticipated nor intended the lynching. In the current interpretation (fig. 3), however, the sheriff's inactivity is cast in a new light. Given his probable knowledge of the mob and its likely intent, his unwillingness to act *morally*, as well as causally, implicates him in the lynching and in the ensuing murder that logically followed his nonintervention.

Stripped of what I have judged to be nonessential detail, and with actor identities, actions, and causal connections conceptually generalized, the generalized event structure represents a skeletal view of the event, a bare anatomy of the lynching and its aftermath. It nevertheless remains contextually and narratively anchored: temporality is preserved, as are pertinent contingencies and particularities. Little of historical or theoretical significance appears lost.¹⁶ While an analyst need not necessarily take

¹⁶ Earlier, I noted that there were contradictory black and white versions of the actions that preceded Funderberg's death. All of the analyses discussed above relied on the African-American version. To see whether my *general* inferences would be sustained by the white version of what happened, I used it to construct a new concrete event structure. I then assessed its logical consistency with the general event structure in fig. 3. I found that the general event structure easily accommodates either the African-American or the white account of the event's early sequences (analysis not presented). The abstract event structure in fig. 3 needs only the fact of racial intimidation, not its source or precise manifestation (i.e., Funderberg's threat to Harris in the black version; Funderberg's confrontation with Harris over stolen groceries in the white version). That this is so *in this case* and *at this level of generality* does not suggest

event-structure analysis to this level of generality, it is here that the event's significance for comparative analysis, empirical generalization, and theoretical development is perhaps most visible.¹⁷ I turn now to several implications of the framework and analysis advanced above.

EVENT-STRUCTURE ANALYSIS, HISTORICAL SCOPE, AND SOCIAL STRUCTURE

Lynchings are microlevel events that generally unfold in a sharply delimited spatial and temporal frame and that involve relatively few actors or actions. So, too, are the other published examples of ESA (e.g., children's play activities [Corsaro and Heise 1990], interactions among adolescents [Eder and Enke 1991], and an exchange in an office [Heise 1989]). Is the utility of ESA limited to such events? Where is social structure in these analyses?

Historical scope and macrosociology.—Labor historian Robert Korstad and I (Korstad and Griffin 1992) have already applied event-structure analysis to an event that is neither micro nor macro, the life and death of a tobacco workers' union local. The time span of the analysis was eight years, the geographic scope encompassed several North Carolina communities and Washington, D.C., and the actors were collective (strikers and African-American women) and corporate (the National Labor Relations Board and the R.J. Reynolds Company) in nature as well as being specific individuals. Clearly, ESA is not limited to microevents. Moreover, I see no logical reason why it could not be fruitfully employed in the interpretation of comparative macrosociological happenings taking place over decades or centuries and in a much wider geographic arena. The analytic core of ESA is the temporal ordering and sequencing of action, not historical scope. In terms of the function that sequence serves in the narrative, it does not logically matter if the statement pertains to a concrete individual or a corporate or collective actor (such as the "mob" in the Bolivar County lynching) or if the action is historically unique or historically general in the sense of referring to many actors,

that discrepancies in other accounts are not causally significant or are not important for other purposes. These are empirical points to be decided case by case.

¹⁷ I performed a large number of such analyses by formally comparing, for seven lynchings, highly abstract event structures containing six general sequences. Event-structure analysis indicated that the generalized event structures were logically identical across all events. This "processual robustness" (Goldstone 1988) can be explained in several contradictory ways, some substantive and some methodological, even definitional. Because my own estimation of their importance is unclear, I do not present them here.

repeated actions, wide geographic scope, or even whole chunks of historical time. What is relevant is that its temporal order and position in the narrative pushes the story forward.

With sufficient knowledge and care, actions constituting "great happenings" or slowly unfolding macrolevel events could be properly colligated and chronologized in precise narrative form (cf., Abrams 1982; McMichael 1990; Sewell, in press). George Fredrickson (1981, pp. 283–87) has done exactly that with the 300-year history of the establishment and partial disestablishment of white supremacy in the United States. He did not attempt a formal narrative analysis, and his powerful argument rests on much more than mere chronology, but his research and that of others as well (e.g., Moore 1978; Zeitlin 1984) suggests a potentially important role for ESA and other formalized narrative approaches in macrosociological historical inquiry.

Social structure and social action.—Event structures are not social structures, and ESA does not directly model social structure. But neither does ESA ignore social structure as it is (contestedly) understood (cf., e.g., Smelser [1988] and Sewell [1992]). It can be useful in deciphering how individual action, such as the racial betrayal, reproduces social structural understandings and constraints. One way objective possibility is determined, moreover, is through the very things implicitly or explicitly meant by "social structure": generalizations about social and institutional relations, resources, and constraints. Social structure is also used to help answer historical questions. My interpretation of the Bolivar County lynching contained many causal judgments, such as that about the relationship between the functioning of law and racial betrayal and solidarity, that were deeply structural in this sense.

Social structure, then, is best understood in analyses such as this as part of the "glue" cementing one action to another. But evocations of it are not substitutes for hard historical thinking. Social structure is not the cause of action (DiTomaso 1982), and it is not even the only cause of the conditions of action. Action, in my view, is "caused" by the conjuncture of reasons—emotive, symbolic, and utilitarian—for action and the consciousness of objective possibilities for action. Objective possibilities, in turn, are partly social structural in origin, but they are also partly the contingent consequences of past action and partly a question of an agent's understanding of the possibility of future action. An analysis of southern lynchings that ignored the South's social structure—its domination by the economics of cotton, its impoverishing sharecropping system, and its corrupt Jim Crow laws, for example—would surely be an exercise in futility. Nor would southern whites have lynched African-Americans so frequently or even done so at all had their history of military defeat and occupation, decapitalization by Emancipation, and subsequent resent-

ment been different or had their culture of racism, patriarchy, male chivalry, and the "honor of Southern white womanhood" (Hall 1979; Whitfield 1988) been other than it was. But because some whites did not lynch, and even actively tried to stop lynchings, other white southerners, despite their structural situation and cultural conditioning, did not "have" to lynch, either. They acted purposively to lynch.

CONCLUSIONS

Historical sociology is currently in a state of methodological flux. Ideas about how to proceed vary tremendously and in somewhat opposite directions. They range from the advancement of strong research programs grounded in rational choice theory (Kousser 1990; Kiser and Hechter 1991) to a return to holistic historical interpretation (McMichael 1990); from the development of ever refined causal generalizations through innovative formal procedures (Heckathorn 1983; Ragin 1987) to the cultivation of a nonformal "evenemental sociology" (Sewell, *in press*) and even a highly formal "narrative positivism" (Abbott 1992). Event-structure analysis borrows something from each of these methodological strategies, though more from the latter two than from the others. Event-structure analysis, evenemental sociology, and narrative positivism, while diverging significantly, are all intrinsically historical in that temporal sequence and unfolding are central to how they approach and explain social process.

It is understandable, then, that some of the methodological concerns animating this article are often associated with narrative history, while others have been more heavily tied to analytically formal sociology. These questions are inextricably interdependent, suggesting, once again (e.g., Tilly 1981, Abbott 1991; Quadagno and Knapp 1992), that the analytic chasm separating historians and sociologists may be increasingly unreal and counterproductive. The latter is true also of recent theoretical developments centering on the centrality of time and temporal process in sociology (Giddens 1979; Abrams 1982; Maines 1987; Aminzade 1992). The moment seems right, then, for a methodology that exploits narrative temporality without simply returning to narrative (see Stone 1979) or drawing such a distinction between interpretation and causal explanation that much of what can be gained from formal analysis is jettisoned (see McMichael 1990).

Event-structure analysis may not be that methodology, and causal interpretation certainly need not be the only objective of narrative analysis. There are other formal narrative strategies that should be explored and fruitful ways to exploit the sociological potential of narrative that transcend the purposes and limits of event-structure analysis (e.g., Abell

1987; Abbott 1992). But through the questions *ETHNO* poses about the connections between actions, *ESA*, more than most methodological or heuristic tools, demands ever deeper probes into events and their historical and structural contexts, separation of causal significance from temporal sequence, the interweaving of the general and the particular, and confrontation with the silences imposed by the paucity of hard evidence. In so doing it forces substantive and methodological questions otherwise easily not noticed or avoided. Event-structure analysis is time consuming, highlights ignorance, and is humbling. It offers no ready answers to the questions it poses or to the difficulties induced by the tension between the general and particular. What it does do is make users extraordinarily self-conscious about what we know in general and in particular and about how to use that knowledge to structure historical events and to discern their meaning. Event-structure analysis thereby facilitates causal interpretations more grounded historically and temporally than is typical of most formal research and more nuanced and explicit than those usually contained in narrative.

APPENDIX

All sources used to construct a narrative—oral histories, ethnographies, newspaper accounts, official and personal documents, and secondary accounts—are abstractions from a myriad of facts. They are necessarily selective and possibly erroneous owing to the limited information contained even in the primary sources, to faulty recall, and even to deliberate prevarication. Accounts, in brief, are constructions of events rather than necessarily truthful accounts of what really happened. Moreover, narratives formulated explicitly by the investigator from any source, for the purpose of further analysis, as in my case, are doubly constructed. The Raper narratives are a case in point.

Arthur Raper was a staff member of the Commission on Interracial Cooperation (CIC), a southern reformist organization active from the 1920s to the 1940s and dedicated to reducing the brutality of racial segregation in the South. Alarmed by the increase in the number of lynchings early in 1930 and wanting to understand their causes so as to eliminate this form of racial barbarism, the CIC formed the Southern Commission on the Study of Lynching, with Raper as research director. Either Raper or his assistant, Walter Chivers, a sociologist at Atlanta University, personally visited the sites of each of the 21 lynchings described in Raper's 1933 book. They interviewed participants and their family members, bystanders, "leading citizens," and others, pored over court records and local and regional newspaper accounts, and gathered extensive information on the histories and political/economic conditions of the communities

that had witnessed lynchings (Raper, p. vi). From this information, Raper then constructed and published his narratives. I, in turn, chronologized some of his narratives and subjected these chronologies to event-structure analysis.

Raper obviously was not a disinterested party to the phenomenon he studied, and his narratives are more than mere descriptions of what happened. He often made causal statements (something happened "because" something else happened) and, occasionally, more or less overt moral judgments in the guise of facts. The assurance given by whites and the deputy's satisfaction with what he heard in the Bolivar County lynching are examples of this practice. I do not know either that assurance was given, or satisfaction expressed; the evidential bases of the claims are equally unknown. I know only what Raper said happened. Moreover, there is little chance that any more information will ever be obtained from any source about the Bolivar County lynching for reasons described by Raper (pp. 96–97). I am thus a partial captive of Raper's understandings and narratives of the lynchings he reported. How my explanation of the Bolivar County lynching might be changed were more information available is simply not known. Analysts of secondary data, quantitative or otherwise, know the problem well. There is no reason, however, to believe that Raper's narratives are any more distorted than are most accounts one might uncover of such a morally charged event as a lynching. In fact, Raper's and Chivers's strategy of using a variety of sources to obtain information likely resulted in superior, not inferior, narratives (see, e.g., Franzosi 1987). Some of the problems in the narratives, moreover, can be mitigated, as I have done here, by rephrasing sentences to avoid prejudging causality and avoiding using, as much as possible, moral assessments.

Nevertheless, the basic problem is real and cannot be wished away. "Real history" is neither known nor knowable. The historical record—what we know of real history—is subject to factual disputes and widely varying interpretations. As an often highly contested construction of the real past, it is unavoidably fraught with epistemological and practical problems. "Correspondence" between a narrative of an event and historical truth cannot be assumed. But all too frequently in historical research imperfect information is all that is available. The options are to use it cautiously or not at all. The historical record is the only body of knowledge both deliberately created to account for the real past and inadvertently displaying the tracks of that past (Isaac and Griffin 1989). What is known of the real past through narrative or any other medium, despite obvious limitations, therefore must serve as the criterion against which both the plausibility of historical counterfactuals and the truth content of our explanations are assessed, even as our knowledge of the past is

itself being extended, corrected, and deepened by the very research on which it sits in judgment.

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The Dynamics of Organizational Rules¹

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This article examines the rate of rule founding and the rate of rule change in a university organization in its 100-year history. Organizational learning and institutional theories are used as research guidelines to explore the effects of path dependency, attention allocation, governmental intervention, and historical context on the evolution of organizational rules. The findings suggest that rule founding and rule change follow two distinctive processes. The former largely reflects an organizational response to external crisis and shocks, while the latter is strongly regulated by an internal learning process. Once rules are established, they are path dependent, sensitive to agenda setting, and adaptive to governmental constraints. There is also evidence that rules are institutionalized over time.

Writing at the turn of the century, Max Weber noted the phenomenon of "the reduction of modern office management to rules," and he listed "rule-following" as the primary feature of bureaucracy (1946, p. 198). As we near the end of this century, we are impressed by Weber's foresight; the prevalence of rules has not only dominated "office management" but also penetrated all sorts of formal organizations. Indeed, in modern societies, formal organizations are governed by rules: resource allocation and decision making are structured by a set of rules that establishes authority, priority, and planning in the organization. The familiar organization chart is in fact a visualization of the rules by which hierarchical levels are ordered and units coordinated. Even at the most basic level, employees enter and leave their workplaces following the rules specified in the time schedule.

Inspired by Weber's work, sociologists and students of organizations demonstrated a particular interest in bureaucratic rules and rule-following behavior in their early studies. Organizational rules are a recurrent theme in the classic works by Merton (Merton et al. 1952), Gouldner

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(1954), Blau (1955), Cyert and March (1963), and Crozier (1964). Recently, there has been a revival of interest in the rule phenomena. These recent studies have pointed to the importance of rules in retaining organizational experience (Nelson and Winter 1982), reducing uncertainty (Perrow 1986), and channeling institutional change (March and Olsen 1989; North 1990).

Despite the central place of rules in organizations, there has been little empirical research on how rules evolve over time. Most of the studies have treated rules more as explanatory variables than as the subject of research. We are thus left with plenty of speculations but little empirical evidence. In this article, I will try to narrow this gap by examining the dynamics of rules at Stanford University over its nearly 100-year history, 1891–1987. My focus is on formal rules—those policies, regulations, guidelines, or routines that are written and established through formal procedures. Rule dynamics studied here refer to the rate of rule founding and the rate of subsequent rule change over time. The purposes of this article are to provide a quantitative analysis of the events (births and changes) in the life course of organizational rules and to link the empirical data with some recent ideas in the organization literature.

The choice of one university organization in this research inevitably introduces context-specific factors into the analysis. However, the issues and mechanisms I examine are quite general for other types of organizations. I focus on one organization to gain detailed information on the mechanisms involved in the organizational rule-making process. Since there is little prior research to serve as a research guideline, I think this trade-off is especially worthwhile.

ARGUMENTS AND HYPOTHESES

The Issues

In 1974, when the Stanford Faculty Senate was discussing the establishment of the Policy on Faculty Discipline, there were two sharply divergent views: opponents of such a policy charged that the disciplinary rule would infringe the long-honored principle of academic freedom and its supporters argued that such a rule would protect the principle of academic freedom by prohibiting disruptions of others' rights. Although this debate was concluded by a marginal majority vote in favor of the disciplinary rule, the issues behind it are far from settled. What is the nature of rules and their function in the organization? What are the implications when an organization evokes its rule system by establishing, revising, or terminating a rule?

Two traditions in the classical sociological works provide my starting point. Max Weber saw the elaboration of formal rules as a part of the

transcendental rationalization process giving rise to modern capitalism. Weber (1978) linked law with domination and discussed in length the connections between law and the authority structure in society. The other tradition, represented by Durkheim, regarded the structure of law as derivative and expressive of the social structure and a society's collective consciousness. In his *The Division of Labor in Society*, Durkheim traced the transition from mechanic society to organic society through the evolution of legal rules from the penal law to the restitutive law. The differentiation of society leads to the demands for "those rules whose number grows as labor is divided, and whose absence makes organic solidarity impossible or imperfect" (Durkheim 1933, p. 407).

In this light, organizational rules are part of the formal structure that constitutes and defines stable patterns of relationships and activities. The study of organizational rules can provide insight into the mechanisms underlying the stability and change in modern organizations. A crucial issue in organizational research is the problem of structural inertia. Structural inertia serves a major function in formal organizations by ensuring organizational stability and accountability. However, structural inertia may also present obstacles to organizational adaptation in times of need (Hannan and Freeman 1984). Furthermore, even if organizations change in adaptation to their external environment, once these adaptations are incorporated into the formal structure, they may become structural obstacles to the organization's adaptability in the future when the environment changes (Weick 1982; March 1991).

The study of organizational rules addresses these issues directly. The speed and direction of the rule dynamics reflect and direct the trajectory along which an organization evolves over time. Changes in organizations are often embodied in changes in the rule system. The Policy on Faculty Discipline at Stanford is such an example. It was a direct result of the turbulence of social protests in the late 1960s and early 1970s, a period during which existing rules failed to regulate tensions and social conflicts. Formal rules also reflect changes in the governance structure. For instance, the student protests in the late 1960s led to dramatic changes in the rule system at Stanford, changes that incorporated student representatives into the decision-making processes.

The theoretical issues I will address here are: How does the organizational rule system evolve over time? What accounts for variations, if any, in the rule dynamics? In particular, I make a distinction between two types of rule dynamics: rule foundings and rule changes. The founding of organizational rules has particular significance, because the birth of new rules can be seen to be a discontinuous process, shifting from the state of no rules to the state of new rules. Rule founding indicates a new identity—a new element in the rule system—whereas rule changes take

place within the framework of the existing rules. In a sense, rule founding may signal an innovation while rule change may suggest an incremental organizational change. Although the natures of these two dynamics are still unclear, it is conceivable that the processes underlying the two types of events may differ and should be analytically distinguished.

In this study, I will explore three sources of rule dynamics. The first one is related to organizational complexity. My focus, however, is on the two approaches recently developed in organization research that treat the dynamics of organizational rules as being regulated by organizational learning and institutional processes.

Organizational Complexity

The most intuitive view, the one that was once most dominant in organization research, sees rules as closely related to the problems of coordination and efficiency. In Weber's vision, bureaucracy is an efficiency-driven and instrument-oriented organizational form where rules are used to establish authority and responsibility and manage complex tasks. This is clearly reflected in economic theories of organizations where it is argued that transaction-cost and free-rider problems in the marketplace give rise to formal organizations (Alchian and Demsetz 1972; Williamson 1975). In the earlier organization literature, organizational size and complexity were seen as causally related to the formalization of the organization (Blau 1957).

In this light, events of rule founding and rule change reflect changes in organizational structure over time. The more complex an organization's structure becomes, the more likely that new rules are created or existing rules modified to accommodate the demand for coordination. A complex organization may also have multiple ongoing learning processes leading to an increasing overall rate of learning for the organization. These considerations can be summarized in the following hypothesis.

HYPOTHESIS 1.—The rates of rule founding and of rule change are positively related to the complexity of organizational structure.

Rules as the Retention of Organizational Learning

From an organizational learning point of view, organizational rules, such as routines, policy statements, and regulations, are the storage of organizational memory. Changes in the rule system, either establishing new rules or modifying existing rules, reflect the gains in the learning process (Levitt and March 1988; March and Olsen 1979; Nelson and Winter 1982; Powell 1986). The two often-noted learning processes are learning by doing and problem solving.

Path dependency.—The core idea underlying the learning by doing

process is *path dependency*. This model assumes an endogenous process within the specific history path where the accumulation of experience induces, and is reflected in, the subsequent modification of behavior. Organizations are constrained by the structure and knowledge that are built up in their history. In this view, learning is an improving match between practice and current technology and an exploitation of existing possibilities (March 1991). What is gained in this process is precision, skill, and confidence. For example, in dealing with rules, members of an organization may accumulate experience in interpreting rules, developing exceptions to them, and understanding their boundaries and flexibilities. The more one improves the match between an existing rule and his or her skills related to that rule, the less likely it is that the rule will be challenged. That is, the improvement process stabilizes rules. Here I propose to use a rule's age as an indirect measure of the improvement process. This specification is consistent with the preceding consideration: The longer a rule exists in an organization, the more likely it is that the organization gains confidence in that rule, and the more likely that skills are developed by practicing it. This leads to the following proposition:

HYPOTHESIS 2.—The rate of change in a rule decreases with its age:²

Organizational learning does not always derive from its past experience, often it involves experimenting in new directions, incorporating new knowledge, and expanding into new territories. For instance, the faculty tenure policy at Stanford in the early days did not have any written specifications of the criteria for appointment and promotion. Over time, the historical changes both within the university and in the higher education sector have pushed the university to clarify these criteria.

Adding new elements or altering the contents of existing rules is more related to the acquisition of new knowledge than to gaining experience of existing knowledge. It accelerates the learning speed, thereby increasing the instability of existing rules. Several factors contribute to this acceleration process: first, by incorporating untested new knowledge into the existing rules, it increases the hazard rate of failure; second, prior rule changes may provide new incentive for learning in the same area. I measure this learning process in terms of the cumulative number of prior changes a rule has experienced. Revisions alter the existing content of a rule, thereby increasing the risk of rule changes in the future. That is:

HYPOTHESIS 3.—The rate of rule change increases with its cumulative number of prior changes.

Problem solving.—If learning by doing depicts an incremental process

² Since path dependency is related only to existing rules, the hypothesis in this section is developed only with respect to rule change.

of change, the second process, that of problem solving, is characteristic of drama and discontinuity. Students of organizations have long recognized the role of rules in the problem-solving process. Cyert and March (1963) proposed that organizational rules serve the function of reducing uncertainty. This argument suggests that organizational learning is often activated in response to crisis, uncertainty, and the disruption of routine processes—"putting out fires," as Radner and Rothschild (1975) put it. This is the occasion where previous experience or formal knowledge fails to accommodate unexpected situations. Organizational search is activated to find solutions to the new problems. Those successful solutions become the repertoire for organizational retention, often in the form of formal rules.

One way to characterize the problem-solving process is to note the mechanism of attention allocation. Attention is activated and shifted to the problem area when an organization experiences crisis or uncertainty. March and his colleagues have emphasized that attention is a scarce resource (March and Olsen 1979). This simple property suggests an interesting feature of the learning process. That is, organizational learning is not a continuous process. Typically, learning occurs when attention is focused on the problem area. When attention shifts away, however, learning is either slowed down, or totally suspended by a return to routine behavior (Radner and Rothschild 1975; Winter 1981).

In this article, I use the distribution of organizational agendas as the indicator of attention allocation. Ecologically, organizational rules exist in relation to each other and coexist with other types of organizational agendas. I distinguish two sets of organizational agendas. The first is *rule-related agenda* versus *rule-unrelated agenda*. The former refers to those items that deal with organizational rules, such as proposing, discussing, or making decisions on rules. The latter refers to those items that are irrelevant to the rule system, such as committee appointments, budgetary reviews, or annual reports from various committees. Second, I distinguish organizational agendas across two areas: *the student area* versus *the faculty area*. The first refers to those agendas that are related to student academic affairs, such as degree requirements and grading policies. The second includes agendas related to the faculty such as appointments, promotion, benefits, and housing. The research issue is how the distribution of different types of organizational agendas affects the rule dynamics.

First, consider the implication of rule-related and rule-unrelated agendas on the rule dynamics. Argyris and Schon (1978) distinguished two types of organizational learning. The first one is a problem-detection-and-correction process, while the second one involves permanent changes in the norms and rules. In dealing with a crisis or an emergency, an

organization can either treat it on an ad hoc basis or by evoking its rule system. For instance, in the financial crisis of 1974 at Stanford, the Faculty Senate responded to this emergency by setting up several ad hoc committees and demanding and receiving numerous reports from the administration. These activities, however, were not related to the rule system. In contrast, the student protests in the late 1960s challenged the existing university governance structure and its rule system. Organizational rules became the subject of inquiry during the crisis.

Organizational learning involves competition for attention among different types of agendas. Given its scarcity, attention to one type of agenda will distract available resources from other types of agendas. Rule-related agendas indicate that organizational attention is directed to the rule system, hence increasing the probability that the rule system will undergo changes. Rule-unrelated agendas indicate the shifts of organizational attention away from the rule system, decreasing the rate of rule change. This can be summarized in the following hypothesis.

HYPOTHESIS 4a.—The rates of rule founding and of rule change are positively related to rule-related organizational agendas, but inversely related to the rule-unrelated agendas.

Now consider the distribution of organizational agendas across the two areas. How does organizational attention to the rules in the student area affect the rule dynamics in the faculty area and vice versa? Again, the property of attention scarcity implies a further competition for attention across different areas. That is, when the organization directs its attention to the issues or crisis in the student area, it tends to neglect issues in the faculty area. Therefore, we expect the following.

HYPOTHESIS 4b.—The rate of rule founding and the rate of rule change are positively related to rule-related agendas in the same area but inversely related to rule-related agendas in the other area.

In the statistical analysis, I will use the distribution of agendas in the previous year to predict the current rule dynamics.

Rules as an Organizational Quest for Legitimacy

It is common knowledge that organizations are dependent on the external environment for resources in order to survive. Among various input-output constraints, the institutional theory of organizations identifies legitimacy as one of the most vital resources (Meyer and Rowan 1977). Institutional theorists emphasize “cultural rules” that give “collective meaning and values to particular entities and activities, integrating them into the larger schemes” (Meyer, Boli, and Thomas 1987, p. 13). Organizational quest for legitimacy accompanies the process of institutionalization—the adoption and incorporation of externally constructed institu-

tional elements into the organizational structure (DiMaggio and Powell 1984; see Scott [1988] for a recent review).

Organizational rules are directly linked to institutional processes in the larger social context. The adoption of institutional elements often takes the form of establishing corresponding organizational rules. That is, rules reflect the historical imprinting of institutional pressures on the organization. For instance, the regulation of affirmative action was initiated by the federal government in the 1960s. The adoption of such an institutional element in the university organization was reflected in the establishment of corresponding policies. At Stanford University, the Faculty Senate passed a series of resolutions in 1973 that became the university's policy in this area.

According to institutional theorists, a major source of institutional environment is the increasing role of the modern state in shaping the life chances of organizations. There is evidence that the creation of formal rules is often associated with strong external pressures. At Stanford, for instance, rules on faculty consulting, affirmative action, and students' access to their records were made largely in response to government regulations. Even when formal rules are based on the codification of existing practice, they are often triggered by external pressures (Zhou 1991).

The effect of the modern state on organizations has been mainly through two devices: regulations and reward and sanction mechanisms. The former is through governmental legislation, such as the proliferation of legal rules, administrative regulations, and bureaucratic procedures. The latter is through the distribution of resources, research funding, financial aid, and other social programs. Over time, both governmental legislation and federal funding in higher education have greatly increased. According to the institutional logic, then, we should find a positive and significant relationship between the state intervention and the rule dynamics. This leads to the following hypothesis.

HYPOTHESIS 5.—The rates of rule founding and of rule change are positively related to the increase of governmental legislation and funding in higher education.

Historical Context and Population Heterogeneity

In addition to my preceding conceptualization, two factors that may also reflect differential learning and institutional effects on the rule dynamics are historical context and population heterogeneity.

Institutional theorists have argued that historical context reflects institutional processes. They propose an increasing role of institutional environment over time, due to the emerging dominance of the modern state

in social life. However, historical periods may also capture discrete contextual variations, especially episodes of external shocks and crises. As a result, historical periods may coincide with the problem-solving process in the organization. I will include historical periods in the model to examine both possibilities.

I also suspect that rule dynamics are affected by the population heterogeneity across the faculty and the student areas. First, the authority structure governing these two areas differs. According to the Charter of the Organization at Stanford, the Faculty Academic Council has the authority to make decisions on academic matters in the student area. On the other hand, most of the rules governing the faculty, though often recommended and made by that same legislative body, must be approved by the Board of Trustees. Second, these two areas are also different in their technology. Rules governing student areas such as the rules about registration, graduation, and course requirements, are often procedure-oriented, whereas those in the faculty area are often normative in nature. In my data set, more than four-fifths of the rules in the student area are procedural rules, whereas only about one-half the rules in the faculty area contain procedures. Finally, these two areas emerge in different environments. For instance, the evaluation of students depends on the faculty within the university. Faculty members, however, are subject to broad professional rules developed outside the university. As a result, the rule dynamics across these two areas may differ substantially. In some of my analyses, I will examine the rule dynamics across these two areas separately.

RESEARCH DESIGN

Context and Data

To test the theoretical hypotheses and to conduct an empirical examination of the evolution of organizational rules, I have chosen Stanford University as the research setting. Data are collected from the minutes of the Academic Council and Faculty Senate and other university publications.

The Faculty Senate and its precursor, the Academic Council, are the major rule-making arenas at Stanford. According to the Charter of the Organization, they have "the supremacy in academic matters." The Academic Council was established in 1891 to discuss and make decisions on matters in the academic fields. The Faculty Senate was established in 1968 to serve as the legislative body of the Academic Council. The rules made in the Academic Council and the senate consist mainly of policies, regulations, and procedures related to academic areas such as undergraduate studies, graduate studies, and faculty affairs. In addition, policies

made by other offices but related to these areas are regularly reported to the senate. In this data set, both rules made by the Academic Council (before 1968) and by the senate (since 1968) and those reported to these two legislative bodies are recorded and included in the analysis.

From 1891 to 1987, there were 343 rules established in the student academic area and the faculty area (see App. A). A total of 740 events (revisions and deaths) occurred to those rules after their foundings during the same period.³ Figure 1 shows the frequencies of rule founding and of rule change over time. These two types of events fluctuated over time rather than following a single general trend.

In addition to recording the timing of the rule-making process, I have also coded information on the agendas of the Academic Council and senate meetings. These agendas reflect how the organizations dealt with different issues across different areas. By examining these agendas, we can gain information on the distribution of organizational attention over time.

Specifically, the data contain the following information:

1. Rule-related agendas. These include agendas in discussing, confirming, proposing, or making rule changes. They are measured as the number of items on the agenda at each meeting of the Faculty Senate or Academic Council and aggregated on a yearly basis.
2. Rule-unrelated agendas. These items are part of the senate business but not related to organizational rules. Examples are reports of budgetary allocation by the Provost's office and annual reports of various committees that are not related to rule changes. Again, they are measured as the number of items on the agenda of the senate or Academic Council meetings and aggregated on a yearly basis.
3. Federal legislation and funding. I collected information on the number of major legislative acts related to the education sector on a yearly basis. Federal funding is measured as the proportion of federal expenditure in higher education in the total income of higher education institutions. Data are collected from U.S. Bureau of the Census (1975) and National Center for Educational Statistics (various years).
4. Organizational structure. The two structural variables are the number of academic programs and organizational size (undergraduate, graduate, and faculty populations).⁴

³ In this study, rule deaths are treated as events of rule change. Since only 88 deaths are recorded in this data set, it is impractical to conduct multivariate analysis for them separately.

⁴ Academic program refers to the number of undergraduate programs across different schools in the university. Organizational size is measured by yearly undergraduate, graduate, and faculty populations. To measure more accurately the effect of size on rules, I use organizational size in the specific area to which the rule belongs. For instance, in model estimation, the size variable used for the rule on undergraduate admission is the number of undergraduate students. For the tenure rules in the faculty area, I use the faculty size.

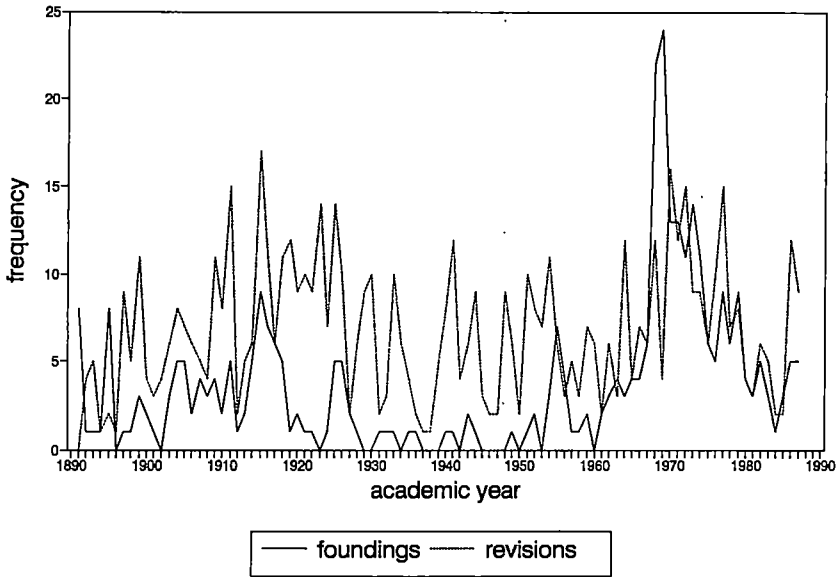


FIG. 1.—Distribution of rule events at Stanford University, 1891-1987

All these variables are used as time-varying covariates in the model estimation. Variables on organizational structure, agenda, and governmental legislation and funding are collected on a yearly basis and path-dependency variables are measured to the exact date. Information on these variables is updated whenever changes in these variables occur.

In addition, I use a set of dummy variables to measure historical period effects. The periodization is based on the patterns of environmental and university changes in these historical years. I divided the historical context into the following four periods:

1. 1889-1915. The first period was the "setting up" years. It also coincided with the tenure of the first president of Stanford. This was a period when "the presidents wielded preeminent power at most of the major universities" (Veysey 1965, p. 303). During this period major decisions at Stanford, including rule making, were largely made by the president himself and there was little consultation with the faculty (Mirrieles 1959).
2. 1916-45. The second period can be labeled as the "years of external crises," including the Great Depression and two world wars. During the world wars, student enrollment dropped sharply. These external events also disrupted the normal processes in the university. For instance, discussions on war situations, military training, and special policies for those joining the army were a major part of the Academic Council meetings.

3. 1946–65. The third period was the “years of expansion.” This was the most prosperous period in U.S. higher education, when resources were abundant and demands for higher education were high (Altbach and Berdahl 1981). Both U.S. higher education and Stanford University grew rapidly during this period.
4. 1966–87. The last period can be labeled as the “years of crisis and readjustment.” During this period, two major events profoundly changed the university as well as U.S. higher education. The first one was the challenge of student protests in the late 1960s, which altered the governance structure of higher education. The second major change was the increasing institutional pressures resulting from state intervention in higher education. Governmental funding also became a major source of income for institutions of higher education.

Following Hannan and Freeman (1989, p. 210), I create the set of dummy variables for historical periods in the following way: The first period is the baseline. The second period is coded “1” for the years 1916–87, the third period “1” from 1946–87, the fourth period “1” from 1966–87. In this coding scheme, the antilog of the effect of a period is the ratio of the rate in that period to the rate in the previous period. This facilitates comparing changes in the rate for consecutive periods.

Table 1 reports the descriptive statistics of the covariates. The first-order correlation coefficients among the covariates are reported in Appendix table B1. There are some high correlations among agenda variables. However, this does not necessarily imply a multicollinearity problem (Hanushek and Jackson 1977). In the model estimation, I experimented with alternative variables and found no substantial changes of effect when these variables were included at the same time.

TABLE 1
DESCRIPTIVE STATISTICS OF THE COVARIATES

| VARIABLE | DESCRIPTIVE STATISTICS | | |
|-------------------------------------|------------------------|-------|-------|
| | N | Mean | SD |
| Age | 7,529 | 21.41 | 21.80 |
| Cumulative revisions | 7,529 | 2.02 | 4.03 |
| Log(size) | 7,314 | 7.69 | 1.16 |
| Log(program) | 7,314 | 4.04 | 0.53 |
| Percentage government funding | 7,325 | 0.13 | 0.09 |
| Legislation | 7,525 | 1.57 | 1.77 |
| Rule agendas (student) | 7,529 | 10.24 | 5.98 |
| Rule agendas (faculty) | 7,529 | 5.00 | 6.80 |
| Other agendas (student) | 7,529 | 9.73 | 8.37 |
| Other agendas (faculty) | 7,529 | 15.73 | 14.71 |

Modeling the Dynamics of Rules

The rule dynamics are reflected in the life events of organizational rules. During their life course, rules experience vital events, such as creations, revisions, and death. In this study, I distinguish two types of rule events: rule foundings and rule changes. These two types of events require different modeling strategies, since they contain different information with respect to their life courses. The major difference between them is that, for the event of rule founding, there is no available knowledge about its prior history. On the other hand, for the event of rule change, the prior history specific to that rule needs to be considered. I propose two different models for these two events and analyze them separately.

The rate of rule founding.—Since the number of rule foundings is relatively small, I treat these events as *event count* data that often follow a Poisson process, that is, they are assumed to be “independently” and “randomly” generated over a time span. My preliminary analysis shows that the event count over time has an overdispersion problem. Consequently, I adopted a version of a generalized Poisson model—the negative binomial model—to examine the effects of the covariates on the founding rate (Cameron and Trivedi 1986; Hannan 1991). The basic negative binomial model specification is:

$$Pr(Y_t = y | \mathbf{x}_t) = \frac{\Gamma(y_t + v_t)}{\Gamma(y_t + 1)\Gamma(v_t)} \left(\frac{v_t}{v_t + \phi_t} \right)^{v_t} \left(\frac{\phi_t}{v_t + \phi_t} \right)^{y_t}, \quad (1)$$

where Y_t is the number of events (rule founding) that occur in a specified time interval t ; $\phi_t = e^{\beta' \mathbf{x}}$, where \mathbf{x} is the set of covariates of theoretical interest and β is the vector of parameters to be estimated. The variance-mean relationship in the dependent variable is specified by v_t . In my model, I assume that the variance-mean ratio is linear in the mean. I choose the academic year as the time interval. For the period from 1891 to 1987, this results in 96 time intervals.

The rate of rule change.—There are different ways to model the rate of change. Here I adopt the formalization of rate as the instantaneous transition rate, as proposed by Tuma and Hannan (1984). This formulation and the related event-history method have the advantage of using full information on the timing of events, the prior history of the individual's life course, and the time-varying covariates. Furthermore, it can handle right-censoring problems. This is particularly important in this study, since the data I collected end in 1987; rules still alive at the end of that year are right censored.

Before I select particular models for event-history analysis, it is instructive to first examine the empirical pattern of rule changes. Figure 2 shows the smoothed Nelson-Aalen hazard rate of rule change. Overall, the haz-

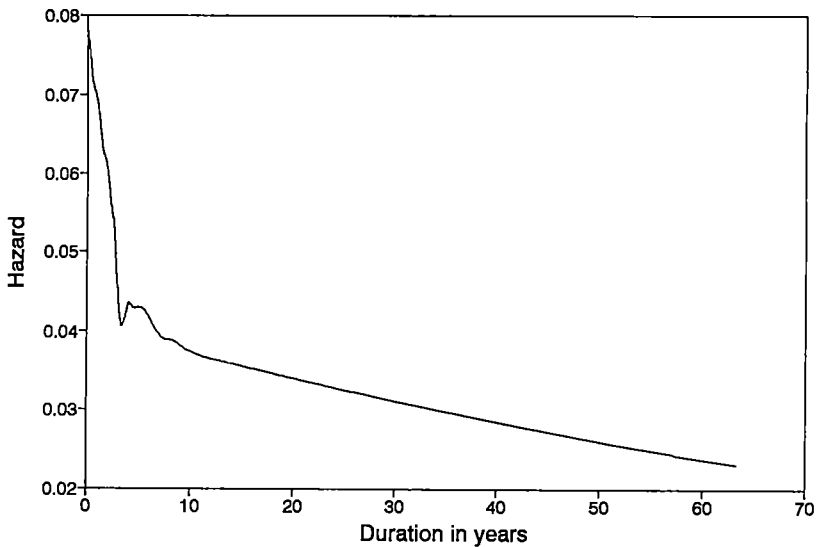


FIG. 2.—Smooth Nelson-Aalen hazard rate of rule change

ard rate declines with duration, with the exception of the nonmonotonic fluctuations in the early years.

One particular problem in modeling event histories is that, in this type of data, the hazard rate tends to decline with duration, even when the characteristics of the individuals in the population are identical. This is mainly caused by unobservable heterogeneity (Heckman and Borjas 1980; Petersen and Koput 1991). Although this problem has yet to be fully understood, several strategies have been developed to deal with it (see Yamaguchi [1986] for a critical review). In this study, I adopt a strategy that explicitly assumes a functional form for the distribution of the unobserved disturbance and incorporates it into the parametric model. Specifically, I adopt a parametric model, developed by Tuma (1985), that treats the rate of rule change as the product of two terms: the first term assumes the rate of rule change may depend on attributes of individual i , but does not depend on time. The second term assumes the disturbance, ϵ_i , follows a gamma distribution. A gamma distribution has the advantage that it can range over all positive values and is flexible in shape. This model also estimates the variance of the disturbance term. The model used in estimation takes the form,

$$\lambda_i = \exp(\beta' \mathbf{x}) \exp(\epsilon_i), \quad (2)$$

where \mathbf{x} is a set of covariates specified before, β the set of parameters to be estimated. For the gamma distribution, we assume that $E(\epsilon_i) = 1$ and $\text{var}(\epsilon_i) = \sigma^2$.

Although the exponential form in the first term assumes that the rate is constant over the duration, this assumption is not as restrictive as it appears. In the model estimation, I include time-varying covariates and the constant rate is conditional on the covariates included in the model.⁵ Notice that, in the estimation of both the negative binomial model and the event-history model discussed above, the rate λ is expressed in the logarithm form. In the following section, I will estimate and report the linear effects of covariates on $\log \lambda$.

RESULTS

The Rate of Rule Founding

Table 2 reports the results of three separate negative binomial models for the rate of rule founding.⁶ In the first model, all founding events are included in the analysis. The second and the third models analyze the foundings in the student and faculty areas separately, treating them as distinctive populations.

Overall founding rate.—It is surprising that, of the four vectors of the covariates, three show poor explanatory power (col. 1 of table 2). First, neither of the two structural variables, the number of academic programs and organizational size, has a significant effect on the founding rate. Second, among the four agenda variables, only rule-related agendas in the student area have a significant effect, increasing the founding rate. Although other types of agendas in the two areas show competitive (negative) effects on the founding rate, they are not significant. Third, in contrast to the prediction of the institutional argument, the two variables measuring federal legislation and funding also show no significant effect on the founding rate.

Clearly, the founding rate has accelerated since World War II. Recall

⁵ Modeling repeated events in the framework of event-history analysis presents a special problem, due to the likely violation of statistical independence assumption across these repeated events. Substantively, however, repeated events are a major part of the rule dynamics. First events constitute only about one-fourth of the total number of events in this study. Clearly, ignoring these repeated events will seriously undermine the effort to understand the dynamics of rule change. Following Tuma and Hannan (1984) and Allison (1984), I adopt the strategy to explicitly incorporate the identifiable prior information into the model. I will assume that the rates of successive events are independent, conditional on the covariates, including the information on the prior history. In other words, each event is treated as a distinctive unit of analysis, conditional on those covariates included in the model.

⁶ Since there is no prior history for rule founding, I did not examine the path dependency effect. It can be argued that the total number of prior foundings indicates some sort of path dependency. However, this variable is highly correlated with organizational size, hence not included in model estimation.

TABLE 2
NEGATIVE BINOMIAL REGRESSION ESTIMATES OF RULE FOUNDING

| INDEPENDENT VARIABLE | MODEL | | |
|-------------------------------------|----------------|----------------|-----------------|
| | Overall | Student | Faculty |
| Intercept | 6.784* (3.096) | 9.224* (3.679) | -.993 (3.474) |
| Log(program) | -.491 (.677) | -.337 (.933) | -2.679 (1.419) |
| Log(size) | -.624 (.425) | -1.082* (.484) | 2.009* (.898) |
| Rule agendas (student) | .050** (.018) | .055** (.021) | .033 (.029) |
| Rule agendas (faculty) | .033 (.026) | .001 (.026) | .033 (.047) |
| Other agendas (student) | -.017 (.024) | .002 (.033) | -.035 (.031) |
| Other agendas (faculty) | -.009 (.031) | .005 (.024) | .010 (.019) |
| Legislation | -.022 (.092) | .006 (.105) | -.121 (.100) |
| Percentage government funding | -1.008 (2.818) | -2.373 (3.546) | 3.391 (2.860) |
| Period 2 (1916-45) | .042 (.384) | .505 (.462) | -2.170** (.853) |
| Period 3 (1946-65) | 1.412* (.663) | 1.577* (.913) | .630 (.910) |
| Period 4 (1966-87) | 1.774** (.555) | 1.302* (.649) | 1.610** (.515) |
| Log-likelihood | -189.38 | -161.74 | -111.77 |

NOTE.—Numbers in parentheses are asymptotic SEs. $N = 96$.

* $P < .05$.

** $P < .01$.

that the dummy variables for the historical periods are coded in a way such that the rate in each period is compared with the rate in the previous period. The antilog of the coefficient gives the ratio of the rate in the current period to the rate in the previous period. The effects of historical periods indicate a process of increasing formalization. Although the founding rate is not significantly higher in the second period (1915–45) than in the first period, the third (1946–65) and the fourth (1966–87) periods do show significantly higher founding rates as compared with their previous periods. For instance, the rate in the third period is 4.1 times higher than in the second period and rate in the fourth period is 5.9 times higher than in the third period. This pattern may reflect the institutional effect, since it is consistent with the increasing trend of state intervention in higher education. However, the founding rates in the two areas discussed below cast doubts on this interpretation of the historical pattern.

Founding rates in two areas.—There are notable differences in founding rates across the faculty and the student areas (table 2, cols. 2 and 3). First, as in the overall founding rate, the number of programs shows no significant effect in either area. Organizational size has mixed effects across the two areas; it decreases the rate in the student area but increases the rate in the faculty area. This pattern may be due to variations of organizational size across these two areas. The size of the student body fluctuated over the historical periods because of the external shocks such as the world wars, whereas the faculty size grew steadily. As for organizational agendas, the previous rule activities in the student area significantly increased the founding rate in that area. However, the same pattern is not found in the faculty area. Rule-unrelated agendas show no significant effect on the founding rate in either area. Again, there is no significant effect of governmental legislation or funding on the founding rates in these two areas.

The founding rates in the two areas indicate quite different trajectories over historical periods. For the student area, all three periods have positive effects on the founding rate. The rate is significantly higher in the last two periods. The founding rate in the faculty area, however, varies nonmonotonically over time with the second period having a significant decrease in rate as compared with the first period. The founding rate in the third period does not significantly differ from that of the second period. The rate after 1966 increases significantly as compared with that of the third period.

It seems that, to a large extent, variations of the founding rates are captured by the historical periods. Even here, however, the patterns are not consistent. The overall founding rate across the historical periods does suggest an accelerating process, especially since World War II. This

appears to be consistent with the institutional argument that organizational rules proliferate over time in response to the process of institutionalization in the larger social context. However, the nonmonotonic period effects over time and across the two areas raise questions about the nature of the historical process.

A plausible explanation for the findings above is that organizational rule foundings, particularly those in the faculty area, are more responsive to crisis or external shocks and less responsive to either cumulative learning or monotonic institutionalization processes. That is, the rate of the rule founding is situation oriented and context bound. This interpretation is supported by the findings that neither the internal learning process (attention allocation) nor external intervention (federal government) shows significant or consistent effects on the founding rate. Moreover, the effects of historical periods reflect environmental fluctuations. The second period is characteristic of external crises, including two world wars. During this period, the overall founding rate shows no significant increase. The founding rate in the faculty area decreases. It seems that the events of the world wars disrupted the university's attention to its rule system. The last period, on the other hand, is a period of crisis and readjustment. Financial crisis, state intervention, and student protests forced the higher education sector to fundamentally alter its governance structure. This pattern implies that the increase in founding rate may be more a response to crises than to the increasing institutional pressures. It also suggests that different crises affect the rule system in different ways. While the crises in the last period directly affect the rule system, the exogenous crises in the second period (the two world wars) seem to distract the organization's attention away from its rule system and decreases the founding rate.

The Rate of Rule Change

In contrast to the founding rate, the rate of rule change shows quite a different pattern: it is significantly affected by both internal learning mechanisms and governmental funding. Moreover, the effects of the covariates also vary across historical periods.

Overall rate of rule change.—As I hypothesized, path dependency, attention allocation, governmental funding, and historical periods all influence the overall rate of rule change (table 3, col. 1). In contrast to the founding rate, the number of programs significantly increases the rate of rule change. On the other hand, organizational size has no significant effect on the rate. The dummy variable "area" is significant and negative, suggesting that the faculty area (area = 1) has a significantly lower rate of rule change compared with the rate in the student area.

TABLE 3

EVENT-HISTORY REGRESSION ESTIMATES OF RULE CHANGE

| INDEPENDENT VARIABLE | MODEL | | |
|--|-----------------|-----------------|-----------------|
| | Overall | Student Area | Faculty Area |
| Intercept | -4.518** (.414) | -4.776** (.467) | -3.499** (.889) |
| Log(program) | .750** (.142) | .663** (.176) | .419 (.286) |
| Log(size) | -.044 (.054) | .037 (.071) | -.108 (.110) |
| Area (1 = faculty) | -.916** (.135) | | |
| Age | -.040** (.003) | -.037** (.004) | -.055** (.008) |
| Cumulative revisions | .168** (.010) | .155** (.011) | .320** (.028) |
| Rule agendas (student) | .034** (.008) | .039** (.008) | .008 (.022) |
| Rule agendas (faculty) | .037** (.008) | .023* (.010) | .070** (.017) |
| Other agendas (student) | .007 (.010) | -.000 (.012) | .032 (.022) |
| Other agendas (faculty) | -.003 (.003) | -.014 (.009) | .029* (.012) |
| Legislation | .017 (.032) | .043 (.036) | -.035 (.075) |
| Percentage government funding | 2.111* (.845) | 1.972* (.974) | 4.651* (1.990) |
| Period 2 (1916-45) | -.496** (.125) | -.511** (.133) | -1.365** (.394) |
| Period 3 (1946-65) | -.895** (.168) | -.780** (.189) | -1.721** (.598) |
| Period 4 (1966-87) | -.698** (.183) | -.310 (.200) | -1.183* (.602) |
| Variance σ^2 | .00 | .00 | .00 |
| χ^2 | 808.07 | 391.64 | 270.75 |
| <i>N</i> | 740 | 628 | 112 |
| <i>df</i> | 15 | 14 | 14 |

NOTE.—Numbers in parentheses are asymptotic SEs.

* $P < .05$.** $P < .01$.

The effects of path dependency are highly significant. The theoretical argument on path dependency suggests an endogenous process independent of other (external) processes. A further consideration is that the effects of path dependency depend on different elements of the previous retention and do not necessarily point in the same direction. Both arguments are supported. A rule's age significantly decreases the rate of change, while the number of cumulative rule revisions significantly increases the rate of change. For instance, one more year of age decreases the rate by 3.9%. On the other hand, one additional prior change increases the rate by 18.3%.

The effects of organizational attention only partially support my hypotheses. Rule activities in the previous year, in both the student and the faculty areas, show significant and positive effect, increasing the rate. Their magnitudes of effect are also very close. Other types of agendas, however, do not show significant effects, either simulating or competitive, on the rate. That is, rule activities and other types of agendas are decoupled in this organizational context.

As for the effect of governmental legislation and funding, the number of legislative acts does not show a significant effect on the rate. However, the proportion of federal funding in the total income of the higher education sector has a significant and strong effect, increasing the rate of rule change. This pattern implies that the main source of institutional effect on the university's rule system is through resource allocation rather than governmental regulations.

Organizational rules tend to become stable over the historical periods. In contrast to the model of the founding rate, all three periods show a decreasing rate of change as compared with their previous periods. For instance, the rate in the second period is about 61% of the first period. The rate in the third period is only about 40% of the second period and the rate in the fourth period is about one-half of the third period.

The overall rate of rule change suggests that, after control for organizational structure, organizational learning mechanisms—path dependency and attention distribution—contribute significantly to the rate of rule change. Institutional effect, in terms of government funding, is also significant. However, the significant effect of the “area” variable suggests that the rates of rule change significantly differ in these two areas. Furthermore, my discussion on attention allocation suggests a competition for attention across areas. This hypothesis has not been examined. To address these issues, I will now analyze these two areas separately.

Comparing the rates of change in two areas.—Do the effects of these covariates vary across different areas? The results are mixed in this regard. The structural and attention effects show different patterns across the two areas, but the effects of other covariates are quite similar (table 3, cols. 2 and 3). Rule changes in the student area are more sensitive to the effects of internal organizational structure. The number of academic programs exerts a highly significant effect on the rate in the student area, but not in the faculty area. This is consistent with the view that the student area is closely related to the technical task of the university organization, while the faculty area is more institutionalized because of the professionalization process. Again, organizational size shows no significant effect in either the student or the faculty area.

As in the overall rate model, the path-dependency variables have consistent and highly significant effects. In both areas, a rule's age decreases the rate of change, while the cumulative number of prior changes increases the rate. Rules in both areas show similar patterns despite the differences in their respective technologies and environments. This renders further support to the view of path dependency as an endogenous self-generating process.

The effects of the distribution of agendas across these two areas vary considerably. Rule-related agendas increase the rate of change in their

respective areas. Rule-related agendas in the faculty area also significantly increase the rate in the student area, but not vice versa. Other agendas in the student area have no effect on the rate in either areas. In contrast, other agendas in the faculty area show some competitive effect in the student area ($P < .10$) but a stimulating effect on the rate in the faculty area. This seems to suggest that organizational activities in the faculty area have a stronger effect on the rate of rule change in both areas, whereas rule changes in the faculty area are buffered from activities in the student area.

As I found in the overall rate model, the number of legislative acts shows no significant effect in the two areas. Federal funding, on the other hand, increases the rate in both areas. Notice also that the effect of federal funding is much stronger in the faculty area. This coincides with our intuition because a large part of the federal funding comes as research grants to the university faculty. Finally, historical periods also show differential effects on these two areas. Although both areas saw a decreasing rate over these periods, the rate in the last period in the student areas is not significantly different from that of its previous period. One interpretation of this insignificant effect is that, during the 1966–87 period, many rules governing the student area are revised to incorporate students into the university's governance structure. These changes may have offset the otherwise declining rate of change.

Historical context: The cohort effects.—Event-history analysis facilitates modeling the dynamics of social processes. However, dynamic analysis does not necessarily mean historical analysis (Isaac and Griffin 1989). In fact, information used in modeling event history, such as the duration and the timing of events, has nothing to do with historical context. To incorporate the historical dimension into the study requires additional efforts common to all other modeling strategies.

The most common strategy is to create a set of dummy variables for each historical period and test the period effects, as I did in the preceding model estimation. However, this choice suffers from the assumption that the effects of the covariates, conditional on the control of the historical period effects, are invariant over time. This assumption seems implausible here. This study covers a nearly 100-year history. It would be unrealistic to assume that the effects of the covariates on organizational rules are constant during this long process. For instance, in the earlier periods, state intervention in higher education, as measured by the number of legislative acts and governmental funding, was minimal. It increased dramatically only after World War II. Obviously, variations of the effects of the covariates over time are not only intuitive, but also theoretically interesting. In this section, I will relax this assumption by estimating

models for different cohorts of rules and examine how the effects of the covariates vary across the historical periods.

I divide the rule events into four cohorts using the four-period scheme discussed above. I estimate a set of models similar to those reported in table 3, except that now the covariates of periods are dropped. My preliminary analysis shows no significant differences for agendas across the two areas, therefore, I aggregate the two types of agenda for model parsimony in this analysis. I also conduct an equality test that tests the null hypothesis that variations of the coefficients across these cohorts can be derived from the same population (Johnston 1984). The rejection of the null hypothesis will suggest that variations of the coefficients across these four periods are substantial rather than due to random disturbances. Table 4 reports the results of the estimates for the four periods.

The equality test indicates that variations of the parameters across these cohorts are indeed substantially different (table 4). Since the two governmental intervention variables lack variation in the first period, maximum-likelihood estimation cannot converge when the first period is included. Instead, I test the last three periods. The chi-square value is 56 with 18 degrees of freedom. The current model is significant at the .001 level compared with the restricted model that constrained all coefficients across these three periods (except for the constant term and σ^2 in the gamma distribution) to be equal.⁷

The effects of structural variables vary considerably across the four periods. In the first period, the number of academic programs has a significant but negative effect on the rate of rule change. It is not significant in the second and third periods, but becomes significant and positive in the last period. Organizational size is not significant in the last three periods. The dummy variable "area" is significant for the last three periods, suggesting that the differences between these two areas became significant soon after the early setting-up period. Variations of structural effects across historical periods indicate that the effect of organizational complexity is not monotonic. The number of academic programs increases the rate only when the organization becomes considerably more complex after the 1960s.

⁷ Specifically, I compared this model with a reduced model in which all the coefficients of the covariates in the model are constrained to be equal, except the intercept and σ^2 , which are allowed to vary to account for differences in degrees of freedom and grand means. The chi-square with associated degrees of freedom tests whether differences of the coefficients across these cohorts differ significantly from each other or are due to some random errors. In testing the last three cohorts, I allowed the parameters in the second period to vary and constrained those in the last two periods to be equal to the corresponding parameters in the second period (except the intercept and σ^2). This resulted in 18 *df*.

TABLE 4
EVENT-HISTORY REGRESSION ESTIMATES OF RULE CHANGE (COHORT ANALYSIS)

| INDEPENDENT VARIABLE | HISTORICAL PERIOD | | | |
|-------------------------------|-------------------|----------------|----------------|------------------|
| | 1891-1915 | 1916-45 | 1946-65 | 1966-87 |
| Intercept | 7.056* (3.942) | -2.039 (2.392) | -2.000 (3.248) | -7.951** (1.061) |
| Log(program) | -2.431** (1.208) | -.405 (.658) | .318 (.651) | .829** (.280) |
| Log(size) | -.136 (.082) | .111 (.107) | -.138 (.282) | -.062 (.115) |
| Area (1 = faculty) | -.287 (.290) | -.663* (.291) | -1.501* (.688) | -1.060** (.210) |
| Age | -.067** (.020) | -.061** (.008) | -.020** (.006) | -.036** (.006) |
| Cumulative revisions | .224** (.030) | .219** (.020) | .110** (.020) | .159** (.023) |
| Rule agendas | -.023 (.017) | .036* (.015) | .011 (.021) | .027** (.008) |
| Other agendas | .006 (.022) | -.004 (.019) | -.042 (.026) | .007 (.008) |
| Legislation | ... | .088 (.113) | -.004 (.072) | .012 (.036) |
| Percentage government funding | ... | -.517 (1.473) | -1.213 (3.055) | 7.506** (1.529) |
| Variance σ^2 | .00 | .00 | .00 | .00 |
| χ^2 | 69.52 | 182.11 | 53.39 | 352.53 |
| N | 152 | 250 | 124 | 214 |
| df | 8 | 10 | 10 | 10 |

NOTE.—Test of equality: $\chi^2 = 56$, 18 df; numbers in parentheses are asymptotic SEs.

* $P < .05$.

** $P < .01$.

Among the sets of the covariates included in the model, the one most consistent over the four periods is related to path dependency. Throughout the four historical periods, a rule's age consistently and significantly decreases the rate of rule change. Equally consistent is the significant and positive effect of the cumulative number of prior rule changes, which increases the rate. Clearly, the effects of path dependency are quite robust across areas and over different historical periods.

It is interesting to note the effects of organizational agenda on the rate of change. Rule-related agendas are significant in the second and fourth periods, both increasing the rate of rule change. But they are not significant in the first and the third periods. On the other hand, other types of agendas show no significant effect throughout the four periods.

How can we use information on the historical context to understand the patterns of attention distribution found here? The first period is relatively easy to understand. In the early days of Stanford history, the presidential power was the major source of organizational decision making. The Academic Council, from which the agenda data are collected, had much less authority on rules than today. This may account for the insignificant role of organizational agendas in this period.

The second and the fourth periods were crisis ridden. The significant effect of rule-related agendas in these two periods strongly supports the argument that attention allocation is a function of organizational response to crises. For instance, during World War II, the Academic Council created and revised rules related to military training and credit for those joining the army. In the late 1960s, again, the Faculty Senate promulgated and revised quite a number of rules related to student representation in the university governance. However, it seems that organizational activities related to rule changes are decoupled from other types of activities, as evidenced by the insignificant effect of "other agendas" on the rate of rule change.

In the same vein, we can account for the insignificant role of the rule-related agendas during the third period. This was the period of rapid growth for both the university and the higher education sector. There were also less tensions or pressures on the rule system. For instance, negative tenure decisions were rare, and there were few disputes over resource allocation during this period (Zhou 1991). It is only in this period that the competition for attention by other agendas shows some marginally significant effect ($P < .10$).

Obviously, variations in agenda effects reflect both internal learning mechanisms and external shocks. The decoupling between rule dynamics and other agendas may be due to the management of attention in the organization. A closer look at the rule-making process at Stanford suggests that, over historical time, the distribution of agendas becomes

highly structured. Since the late 1960s, for example, at the beginning of each academic year, the Steering Committee of the Faculty Senate routinely announces the major legislative items for that year. The president's report, the provost's report, and annual reports of the various committees also become the routine items at the senate meetings. The agenda setting may have structured the allocation of attention, producing the decoupling between rule-making activities and other types of agendas. On the other hand, attention distribution clearly absorbs variations in environmental pressures during different historical periods, as is evident in the variations of agenda effects across the historical periods. The pattern of attention distribution does reflect a problem-solving process.

Finally, these results show that government funding becomes significant only in the last period. This means that the effect of the state on organizational rules is quite a recent phenomenon. These findings also imply that the influence of the state on the rule system studied here is more through the reward and sanction mechanism (funding) than through direct regulations. I should add a cautionary note that this conclusion is tentative, since the measure of legislative acts used here is the major legislative acts in the education sector in general rather than a more precise and complete history of legislation in higher education.

In sum, these findings portray a much more complicated picture than we originally anticipated. There were remarkable variations in the effects of organizational agendas and federal funding on the rates of change over the four historical periods. Both were highly sensitive to historical context. These findings are consistent with the picture of the founding rate. That is, rule dynamics are underscored by organizational response to crisis and external shocks. On the other hand, as expected, the effect of path dependency is quite consistent and robust over the historical periods.

A surprising finding in the statistical analysis is that the rate of rule founding and the rate of rule change show two distinctive patterns. Variations in founding rates are largely captured by historical period effects. Organizational complexity, learning mechanisms, or governmental legislation do not systematically influence the founding rate. One interpretation is that the creation of new rules is an organizational response to crisis and external shocks. On the other hand, the rate of rule change is path dependent, sensitive to attention allocation, adaptive to institutional constraints, and tends to become stabilized over the historical periods.⁸

⁸ I want to call attention to the fact that the two rates are defined differently in their statistical models: The founding rate is the variation of the *number* of rule foundings over time (year), whereas the rate of rule change is measured as the *duration* in the current state since rule founding or, in cases of repeated events, since the last rule

There are several plausible explanations for this phenomenon. Institutional theorists emphasize the dependence of modern organizations on external legitimacy. On the one hand, the increasing institutional pressures tend to generate more rules, as organizations adopt these externally constructed elements into their rule system in order to conform. On the other, the adopted rules tend to become stable over time, either because they are taken for granted (Zucker 1983) or because they are decoupled from organizational activities (Meyer and Rowan 1977). Alternatively, as Durkheim argued, rules play an important role in maintaining the fragile interconnections among individuals in a highly differentiated society. In times of crisis, social conflicts may result in the breakdown of tacit rules within an organization. In order to maintain the organizational boundary, formal rules are established to reaffirm and reinforce the collective consciousness. Once the rules are made and collective consciousness is reinforced, the process of subsequent rule changes may follow different processes. This is consistent with the hypotheses of organizational learning as a problem-solving process.

What are the implications of the patterns of rule dynamics observed here for organizational stability and change? If, as I speculated at the beginning, rule foundings signal discontinuous innovation and rule changes incremental adaptation, these findings are consistent with the received wisdom that organizations tend to experience major changes and engage in innovation during times of crisis. Equally important is the finding that other parts of the organization may be relatively stable during the crisis period, as indicated by the declining rate of change in existing rules over the historical periods and the decoupling between rule-making activities and other types of organizational agendas. It is necessary to be cautious about the mechanisms underlying these processes. They may vary across different segments of the organization and over time. For instance, the role of the state, as measured by federal funding, is significant only in the last period. The effect of organizational complexity also fails to show the monotonic effect that has been proposed by some organizational theories.

Does the increasing number of new rules mean more social bondages among individuals in the organization, as Durkheim would argue, or does it merely play a symbolic role in order to acquire external resources, as institutional theorists propose? Given the data constraints, I am unable to address these issues directly but I hope the examination of rule dynamics will lead to a better understanding of these issues in the future.

change. Therefore, the comparison between the results from these two models should take into consideration these model specifications.

CONCLUSION

The proliferation of formal rules has been a salient feature of modern organizations. In this article, I have presented a preliminary examination of the sources of this phenomenon. Although this study only focuses on one university organization, the issues and mechanisms identified here—path dependency, problem solving, governmental interventions, and organizational response to crisis—are quite general in other types of organizations. In this concluding section, I will suggest some implications of these findings for the rule dynamics in other organizational contexts.

In my analysis, the effect of path dependency on rule dynamics is robust across different areas and historical periods. I believe this phenomenon to be quite ubiquitous in other types of organizations. Path dependency provides the constant endogenous source of stability and change within the rule system. The effect of attention allocation, however, is highly sensitive to both attention management and discrete environmental changes. This implies that the effects of problem-solving processes will vary considerably depending on the life events experienced by an organization or by different types of organizations.

These results also suggest that rule dynamics vary across areas with distinctive technologies and environments. The technical core tends to be more sensitive to the effects of attention allocation while the institutional area is more subject to external shocks. On the basis of these findings, I concur with the recent propositions by Meyer and Scott (1983) that, for those organizations in the sector dominated by the task environment, such as business firms or bureaucracies, the rule dynamics is more likely to follow a learning process based on its technology. But in those organizations in the sector in which the institutional imperatives prevail, the rule dynamics should be more sensitive to turbulences in external environment.

Although tentative, the results on the governmental intervention are interesting and worth further exploration. The rate of rule change in the university organization studied here accelerates with the increase of federal funding in higher education but is not significantly affected by the governmental regulations. I propose that the reward and sanction mechanism is more effective in the public sector where federal funding is a crucial factor, although the regulatory mechanism may dominate in the private sector where legislation has been the main source of governmental intervention. This proposition should be tested in a comparative study of organizations across different sectors. These findings also suggest that the effect of governmental intervention on the rule system is a recent phenomenon. The discussion of the institutional effect should be placed in its historical context.

The salient feature of the rule dynamics that emerged from this study is that organizational rule founding and rule change follow two distinctive processes. The former is, to a great extent, an organizational response to crisis or external shocks. This is reflected in the significant role of historical context in explaining the founding rate and is consistent with the long-claimed proposition in the organization literature that organizational rules serve to reduce uncertainty, manage conflicts, and steer organizational change (Cyert and March 1963; Crozier 1964; Perrow 1983). Once rules are created, however, they are strongly regulated by internal learning processes. Path dependency, attention allocation, and institutional constraints, together with organizational complexity, all show significant effects on the rate of rule change.

The transition from a crisis-sensitive rule-founding process to a more internally regulated rule-change process suggests that organizational rule systems have remarkable capacities in dealing with the problem of absorbing external shocks and contextual variation. Here, organizational learning plays a crucial role through the effects of path dependency and differential distribution of organizational attention. If organizational rules are a part of organizational structure and rule dynamics reflect organizational change and adaptation, an examination of rule dynamics in different organizational settings may help us understand the differential trajectories of the evolution of organizational structure and provide clues to such unsettled issues in the organization literature as structural inertia (Hannan and Freeman 1984) and adaptation versus adaptability (March 1991; Weick 1982).

APPENDIX A

Notes on the Coding of Rule Events

In this appendix, I report the decision rules by which the events of organizational rules are coded and the considerations associated with these decisions.

Definitions and Categories

In this study, organizational rules refer to those written rules that are established through formal procedures. The data include the complete population of rules governing the academic matters at Stanford University. Specifically, I include the following two categories of rules:

1. Those procedures, regulations, or requirements that contain explicit policy statements. For instance, the requirement for the bachelor of arts degree contains instruction on units required, procedures to follow, and

so on. Tenure rules are another example in which conditions for length of service, renewal, and dismissal are explicitly specified.

2. Those resolutions that explicate the purpose, intention, and expectation of the legislative body but contain no implementation procedures. For instance, in the 1968–69 academic year, the Faculty Senate passed a resolution to express its position on campus disruption and to urge the community to negotiate the conflicting views within it instead of resorting to “violence.”

I also coded rules with respect to the areas in which they govern, the student and the faculty areas. The distinction between rules in these two areas is based on the subject to which the rule applies. The grading policy regulates student academic performance, hence is treated as a student rule. The policy on student evaluation of teaching regulates the behavior of the faculty, hence is treated as a rule in the faculty area.

Events in the Rule System

To record the life course of a rule, I have focused on the following events that a rule may experience.

1. Birth: the event of a new rule as an independent identity being incorporated into the rule system;
2. Death: the event of terminating an existing rule through a formal process, such as approval by the senate or a formal announcement; and
3. Revision: the event in which an existing rule is physically changed by altering, adding, or deleting its contents;

Though these events are similar to those social subjects sociologists routinely study, such as the life course of individuals, organizations, or polities, rule events examined in this study involve additional complications that call for special coding consideration.

1. Birth. The content of a rule may have already existed before that rule is formally established. The decision rule adopted here is to record the event and the time when it formally becomes an independent rule, since these vital events are of primary theoretical concern in our analysis.
2. Death. The same problem exists for the event of death. Rules may be formally pronounced dead. They may also dissolve into other rules or disappear quietly from rule books. I coded the event of dissolution as death, but treated the case of disappearance as “censored,” since I did not have information on their final destination.
3. Magnitude of change. Rules’ contents may be totally replaced but still have the same name, or their contents may be partly replaced under the same name. I decided to code the change of content as “death,” if it satisfies one of the following conditions: (a) the name of the rule is substantially changed or (b) the content of the rule is totally rewritten

or (c) the legislation explicitly declared the termination or "replacement" of that rule.

The Boundaries of Rules

Compared with other subjects of social research, a unique feature of organizational rules is that their boundaries are blurred. For instance, "the statement of appointment and promotion" (the tenure policy) includes a dozen or so specific rules, covering such matters as the length of service, renewal, and dismissal. The Ph.D. requirement also contains specific regulations on oral examination, defense, foreign language, dissertation, and other requirements. Some criteria are needed for deciding whether an element in the rule book is an independent rule or only part of a rule. This is related to defining the population and the types of events.

Two processes appear to be involved: the problem-solving process and the integrating process. In a broad sense, organizational rules are made or revised in response to specific problems. To make the problem-driven solutions consistent with the existing rule system, the second process, that of integration, incorporates the adopted rules into the rule system and establishes their connections with other existing rules.

Essentially, this study focuses on the first process, that is, organizational response to emerging problems. The boundary of a rule is based on the following criteria.

First, a rule originally established as a distinctive rule will be continuously identified as such, even when it is incorporated into a larger group, as long as it still retains its own identity (such as a subtitle, a separate section, etc.). For instance, rules with respect to tenure processes are developed separately and are identifiable with their subtitles, such as renewal, dismissal, and so forth. Therefore, they are recorded as separate rules within the same (tenure policy) field.

Second, if a rule is discussed in the legislative meetings as an independent issue rather than as part of an existing rule, it will be treated as an independent rule, even if it appears under the same heading with other rules. For instance, the rule for grading in the Graduate School of Business was discussed in the senate as a specific case rather than as part of the grading policy for the whole university. According to this criterion, this rule is coded as a separate rule, even though it is listed as part of the overall grading policy in the *Stanford Information Bulletin* (1891–1987).

APPENDIX B

TABLE B1
PEARSON CORRELATION COEFFICIENTS

| VARIABLE | PEARSON CORRELATION COEFFICIENTS | | | | | | | | | |
|--|----------------------------------|------|------|------|------|------|------|------|------|------|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
| 1. Age | 1.00 | | | | | | | | | |
| 2. Cumulative revisions | .63 | 1.00 | | | | | | | | |
| 3. Log(size) | .15 | .08 | 1.00 | | | | | | | |
| 4. Log(program) | .10 | -.04 | .45 | 1.00 | | | | | | |
| 5. Percentage government funding | .19 | .04 | .41 | .72 | 1.00 | | | | | |
| 6. Legislation | .06 | -.03 | .27 | .58 | .44 | 1.00 | | | | |
| 7. Other agendas (student) | -.01 | -.07 | .34 | .80 | .59 | .43 | 1.00 | | | |
| 8. Other agendas (faculty) | -.03 | -.08 | .34 | .78 | .57 | .60 | .80 | 1.00 | | |
| 9. Rule agendas (student) | -.09 | -.03 | .01 | .02 | .05 | .06 | .14 | .22 | 1.00 | |
| 10. Rule agendas (faculty) | -.09 | -.05 | .16 | .34 | .42 | .26 | .51 | .58 | .29 | 1.00 |

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Book Reviews

Social Change, Social Welfare and Social Science. By Peter Taylor-Gooby. Toronto: University of Toronto Press, 1991. Pp. xii + 244. \$45.00 (cloth); \$18.95 (paper).

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Is *Social Change, Social Welfare and Social Science* another book challenging claims that the welfare state is in crisis? Ah, but there is a new set of critical claims, and Peter Taylor-Gooby, concerned that "the state as central guarantor of citizen welfare is at risk of premature interment" (p. 2), has produced this volume to answer them. "The former crisis of the welfare state," proclaimed in the 1970s and 1980s, was weathered—despite allegations that capitalism and welfare states were in contradiction, "the traditional structure of state welfare survived" (p. 5). But new concerns about its viability have arisen among sociologists, philosophers, and policy analysts.

Taylor-Gooby offers a compelling argument for the welfare state based on a range of (mostly British) sources—census data, public opinion surveys, and policy evaluation studies. (The book assumes knowledge of recent British policy developments.) Some critics allege that the welfare state does not advance equality; conservative critics claim that it subverts desirable social goals (e.g., independence). Taylor-Gooby counters, showing advances associated with public provision, and disputes the usual criterion of success. The achievement of equality is an unrealistic goal; by the more reasonable standard of having helped to diminish inequality, the welfare state has succeeded. Others focus on objective forces that allegedly undermine the capacity of the state to provide welfare—financial limits, an aging population, and changing family forms. The author finds concerns about the fiscal consequences of a growing elderly population are disproportionate to the problem, arguing that retirement and the economic dependency of the elderly are subject to political decision making rather than compelling benefit cuts. He agrees with feminists who note that, as women move into the paid labor force, the pool of available care providers decreases, and he shares their concerns about care givers' conditions.

Many argue that growing income polarization augurs poorly for the future of the welfare state: if citizens can (and are encouraged to) rely on private provision, they are less likely to support public programs.

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Taylor-Gooby demonstrates that the Tories' taxation and social policies did strengthen trends toward increasing income inequality, but claims that opinion has not polarized between "comfortable" and "miserable" Britain. He relies on public opinion surveys carried out in the 1980s to show that support for the welfare state, particularly for nontargeted programs like the National Health Service (NHS), has increased over the decade. Indeed, he shows that the better off are happy to make use of both private and public services (e.g., the NHS). But—after less than a decade—can we know the full consequences of policies promoting private provision among the better off (e.g., the shift from public to private pensions)? Recent sociological attempts to complexify our understanding of stratification, for example, by considering status groups or consumption sectors in addition to social class, mesh well with policymakers' skepticism about the welfare state, which they conceive of as an institution responding to working-class demands. But Taylor-Gooby argues that such attempts do not improve explanations of mass voting behavior; stratification that is not based on class, in fact, adds justifications for social rights.

The final section of the book, dealing with normative debates about citizen needs and the role of the state and arguing for social rights to guarantee basic human needs for survival and autonomy, is quite innovative. Right-wing critics often express concern about the (paid) work disincentives ("moral hazard") inherent in welfare provision. Taylor-Gooby says that such concerns are overblown but, building on recent feminist work, argues that "so long as it can be shown that state welfare policy makes a substantial contribution to the pattern of differential incentives that encourages men to avoid unpaid carework, the welfare state must be seen as an apparatus of moral hazard in a centrally important area of social life" (p. 203). Instead, the state must deploy its authority to fulfill the community's obligation to meet human needs and avoid moral hazards (in both paid and unpaid labor). "Equal enjoyment of rights requires that some people should be prevented from infringing on the human need for freedom of others by not participating in the paid and unpaid work that is necessary to the continuance of society" (p. 208).

I found myself in agreement with Taylor-Gooby's conclusions about care work and impressed by his marshaling of evidence in defense of the welfare state. Yet there is clear evidence of its political difficulties. Taylor-Gooby did not set out to explain why politicians like Reagan and Thatcher, who campaigned against the welfare state, were elected or why the restructuring and cutback of systems of social provision have occurred across the West. Still, I found myself wishing that he had offered some commentary on the gap between the moral and empirical justification for the welfare state on the one hand and the political developments that have led to policy cutbacks and, indeed, to the academic and political attacks he challenges so effectively.

The New Politics of Poverty: The Nonworking Poor in America. By Lawrence M. Mead. New York: Basic Books, 1992. Pp. xii + 356. \$25.00.

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University of Chicago

In *The New Politics of Poverty*, Lawrence Mead expands the argument he presented earlier in *Beyond Entitlement* (New York: Free Press, 1986). Mead wants to establish that since the early 1960s there has been a sea change in the politics of poverty, a move from debates about class and opportunity to debates about the competence of the poor. He also contends that stable, legal employment is the implicit minimally acceptable contribution on which claims to economic and social justice can be based in Western society. Mead links these two arguments with empirical evidence about poverty, welfare use, and a variety of training and work programs in an effort to prove that joblessness is the main cause of contemporary poverty and that joblessness is due neither to lack of opportunity nor to economic rationality but to cultural and psychological barriers in the minds of the poor themselves. He concludes that work requirements for welfare recipients provide the best solution for the "real" poverty problem, a problem more social than economic.

Mead opens by describing a sharp decline in the work effort of the poor since 1960, attributing the bulk of today's poverty to nonwork and low work hours. He links this trend of nonworking poverty to a shift from "progressivism" to "the new politics of dependency." Traditional progressive politics, according to Mead, involves a continual pull between free market and regulatory/labor-protectionist approaches to the economic well-being of citizens. Such politics, however, presumes that adults are working. The new politics of dependency focuses instead on the competence of the poor to be self-sufficient, with conservatives arguing that the poor simply do not want to work and liberals arguing that the poor want to work but face overwhelming obstacles. The author rejects both views, arguing instead that the poor are "dutiful but defeated," willing and able to work but caught in a culture of poverty that renders them passive and afraid to try.

Mead devotes his central four chapters to evaluating reasons for the poor's withdrawal from the labor market. He discounts in turn low wages, insufficient jobs, deindustrialization, the growth of part-time and part-year jobs, the operation of a dual labor market, skills and/or spatial mismatch, immigrant competition, barriers such as racism, lack of transportation and child care, and welfare as a competing source of income. His primary arguments for dismissing this array of factors are that, on the one hand, they explain low wages or job quality rather than failure to work at all, and, on the other, that employed persons seem undeterred

by such problems. Mead concludes that psychological and culture-of-poverty explanations for nonworking poverty are the most believable. Requiring the poor to work will, he asserts, overcome both the internal inhibitions of the poor and the larger social dilemma posed by a population without an earned right to government assistance.

Mead turns next to policy evaluations and welfare-reform history to bolster his arguments about the shift from progressive to dependency politics and the relative success of mandatory work programs when compared to education, training, and work-incentive programs. A summary chapter follows, including a speculative (and questionable) attempt to apply dependency politics to trends in Western Europe and relations between First World and Third World countries. Mead closes by predicting longevity for dependency politics.

Mead's political analysis of the trajectory of welfare reform, official rhetoric, and public opinion is far more convincing than his effort to explain low employment in the poverty population. To his credit, Mead is familiar with the empirical poverty literature and, unlike many conservative apologists, does not rely on vivid portraits of sometimes hypothetical individuals to make his case. However, the conclusions Mead draws from the evidence are far from obvious. One must swallow Mead's ideological premises before his reasoning becomes plausible.

Many specific criticisms could be made. Mead consistently assumes the nonworking poor have access to the same resources as those employed. He decries the fact westerners now spend less time raising children while embracing the new norm of the working mother and denying the need for child-care assistance. He dismisses racism easily, yet links underclass formation to nonwhites and Third World immigrants. Mead's repeated assertion that any rational poor person would simply seize available opportunities and, "soon be nonpoor, if not middle class" (p. 136) is based on ideology alone. He also falls into the culture-of-poverty conundrum whereby attitudes are strongly shaped by structural conditions of the past yet impervious to those of the present. The list goes on.

The basic problem with Mead's enterprise, however, is that the "cause" of nonwork depends heavily on what one believes constitutes an acceptable work effort under specific circumstances. The author himself makes this point, but his goal seems to be to "prove" his own values correct. At bottom, Mead's reading of the data is not that the poor lack reasons for failing to work harder, but rather that the reasons they do have do not constitute valid excuses under the standards he proposes. Mead's fundamental insight, that policies for alleviating poverty necessarily depend on some conception of social rights and responsibilities, remains an important one. Unfortunately, moral propositions are not amenable to scientific proof.

E. Franklin Frazier Reconsidered. By Anthony M. Platt. New Brunswick, N.J.: Rutgers University Press, 1991. Pp. xi + 278. \$27.95.

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Yale University

Anthony M. Platt's important and timely work, *E. Franklin Frazier Reconsidered*, attempts to dismantle two dominant assessments of Edward Franklin Frazier's life and work—interpretations that characterize Frazier as either a "saint" or a "stone." One general interpretation emphasizes Frazier as the "distinguished Negro sociologist," top student of Robert E. Park, and first black president of the American Sociological Association. The other view simply condemns Frazier and his work, largely because of his posthumous association with Daniel Patrick Moynihan's *The Negro Family: The Case for National Action* (Washington, D.C.: U.S. Department of Labor, 1965), better known as the "Moynihan report." Platt argues that the generalization that idealizes Frazier's life is "insufficient and incomplete," and that the association with Moynihan is "essentially wrong" (p. 2). Instead, Platt presents a biography that tries to display Frazier as a highly talented, iconoclastic, and often contradictory intellectual who left a fascinating legacy of writings, both scholarly texts and biting polemics and "who forged an exemplary record of sustained political activism against the horrors and injustices of racism" (p. 2).

Platt's narrative about Frazier's young adult years and career offers a detailed account about the nature and extent of Frazier's political involvement and theoretical perspectives. From his undergraduate tenure at Howard University to his teaching appointments at Tuskegee, Atlanta University, and Fisk University, Platt reveals a scholar who is deeply committed to leftist ideals: throughout his adult life and in the face of consistent opposition, Frazier is staunchly anticapitalist and staunchly anti-white supremacist.

Platt's account of Frazier's early career and extensive "pre-Chicago" writings also show that much of Frazier's intellectual maturation occurred prior to his graduate studies at the University of Chicago. Platt convincingly demonstrates that Frazier's training at the Chicago school was but one of his many intellectual influences that actually began with the scholarship of W. E. B. Du Bois. Still, Platt's account wrongly underestimates the significance of Frazier's graduate training. In his post-Chicago writings Frazier employs analytical constructs—particularly the notion of "disorganization"—which do not appear in his earlier writings.

Indeed, it is this very notion of "disorganization" that provides the linkage between the ideas of Frazier and Moynihan that Platt disputes. Platt argues that when Moynihan cited Frazier in his *The Negro Family: The Case for National Action*, he omitted key passages that would have revealed greater complexity and nuance in Frazier's analysis of the black family. Hence, Platt suggests that the major criticisms of Frazier's studies

of the black family—that he (1) regarded the black family as an institution deformed by slavery and therefore culturally deviant, (2) said that the *main* handicap of the black population was unorganized and disorganized family life, and (3) described most African-American families as matriarchal and therefore dysfunctional—all more fittingly describe Moynihan's thinking, not Frazier's. Nevertheless, although Moynihan failed to articulate Frazier's theoretical complexity, he did appropriate a perspective that was preoccupied with black family instability, and undergirded by assumptions about the superiority of the nuclear family. Moynihan's theoretical orientation grows out of the tradition of scholarship concerned with disorganization, a construct that invites scholars to accumulate and emphasize data confirming family and institutional instability. Therefore, while Frazier has probably been wrongly seen as the "father" of the Moynihan report, he is nevertheless an important relative.

Frazier did not, in fact, view the effect of slavery as permanently deforming to the African-American family, nor did he understand matriarchal and disorganized family life to be the *chief* handicap of black Americans. But he did accept the idea that slavery was part of the cause of black peoples' deviance in family relations. And he did believe that family disorganization was an extremely significant result of the effects of economic and demographic changes.

Platt concedes that there are fundamental problems with Frazier's use of the concept of disorganization. He even tells us that Frazier's writings "are filled with prudish and class-biased allusions to 'moral degradation' and 'promiscuous sexual relations'" (p. 138). Platt simply states that, like his contemporaries, Frazier assumed the superiority of "an idealized, middle-class, nuclear family" (p. 139). But this assumption—wedded to the notion of disorganization—is precisely why Moynihan and Frazier are related. Despite profoundly different understandings of politics and economics, both scholars are part of an ongoing tradition of sociological discourse that has cultivated a view of low-income black families and people as "lacking mainstream values."

Platt's attempt to vindicate Frazier from his association with Moynihan is the least compelling aspect of his study. The rest of his biography, however, offers a fascinating window into the struggles and accomplishments of E. Franklin Frazier, as well as the broader social and intellectual context in which he acted. *E. Franklin Frazier Reconsidered* should be read by students of race and ethnic relations because it forces us to revisit not only the life of a noteworthy scholar, but also a number of deeper theoretical questions concerning the nature and meaning of African-American identity, politics, and social institutions.

The Visible Poor: Homelessness in the United States. By Joel Blau. New York: Oxford University Press, 1992. Pp. xi + 236. \$22.95.

Peter H. Rossi
University of Massachusetts

Joel Blau's book adds significantly to the growing literature on the contemporary homeless in America, not so much by increasing our firm knowledge about them but by providing a relatively new structural interpretation of homelessness. Blau's major thesis is that homelessness increased as America's policymakers bowed to the will of our ruling business elite and strove to drive down wages by undermining the income maintenance safety net and striking out at labor unions. Lowering wages meant lowering the welfare income transfers in order to maintain a discernible incentive to work. If welfare payments were too generous, too many low-wage workers would stop working and live on welfare. Lowering wages also meant that workers competed more intensively for jobs with the result that the least qualified workers were pushed out of the labor force. At the end of the bumping chain were those who were dispossessed from job and housing, ending up among the homeless.

Blau's interpretive framework is just that. He presents little empirical evidence that bears directly on his major thesis. Nor does he consider alternative frameworks that are also structural in character, in particular, the structural transformation of the American economy and its labor supply needs. Like almost every writer on homelessness, Blau vigorously denies that the homeless ought to be blamed for their plight, but no one is identified as holding the contrary view. Also like other authors, Blau asserts that the homeless are heterogeneous, but does not spell out what his standard is for that judgment. It is the conventional wisdom among scholars of homelessness that only a paycheck stands between most Americans and homelessness, a patently foolish statement as anyone who has missed a paycheck or two can tell. This last statement does not mean that unemployment is not at the root of homelessness but that most persons who have lost jobs over the past two decades did not experience homelessness.

All that said, there are some very important features in Blau's book. Because he paid close attention to local community responses to homelessness, he provides the reader with rich, fine-grained accounts of how some of the major cities met the challenge of homelessness. An especially detailed account of what happened in New York City occupies two chapters.

A final chapter summarizes Blau's policy recommendations calling for offering permanent affordable housing for the homeless accompanied by social supports "if necessary." He implies that this policy would be cheaper than the expensive short-term "emergency" programs currently in place, but he despairs that any such policies would be put in place because they "would conflict with the underlying principles of the U.S.

economy. Private profit, self-sufficiency through work, and the commodification of basic human needs such as food, housing and medical care: social policies designed to eliminate homelessness violate these principles and hence cannot be granted serious consideration" (p. 177). In short, the author does not state his policy recommendations in specific terms but simply states that there are effective policies that cannot be considered because our policy-making institutions are not willing to consider them.

The Rational Public: Fifty Years of Trends in Americans' Policy Preferences. By Benjamin I. Page and Robert Y. Shapiro. Chicago: University of Chicago Press, 1992. Pp. xvi + 489. \$59.95 (cloth); \$19.95 (paper).

W. Lawrence Neuman

University of Wisconsin—Whitewater

Pollsters have measured U.S. public opinion since the 1930s, creating a mass of quantitative data. In *The Rational Public*, Benjamin Page and Robert Shapiro examine data from national opinion surveys conducted between 1935 and 1990. Relying on the logic of sampling distributions and the law of large numbers, they discover the public's policy preferences after smoothing out random fluctuations from individual surveys or responses. They do not criticize Americans for being uninformed and frequently changing their policy preferences. Instead, they find that the public, as a collective body, is stable, absorbs information presented to it in the media, and makes rational policy decisions.

The authors focus on approximately 1,000 questions with the same question wording to avoid effects from wording changes. The questions include the policy areas of crime, education, foreign aid, international affairs, women's labor-force participation, desegregation, and civil liberties. They ask whether policy preferences have been stable or changed and find no change for most issues. Change is greater for foreign than domestic issues. Slightly under one-half of the changes are abrupt. Abrupt changes are twice as likely for foreign policy and are tied to specific events (e.g., the Japanese Pearl Harbor attack, the Tet offensive in Vietnam, and the U.S. hostage crisis in Iran).

The authors recognize that public and corporate officials attempt to shape public opinion. To explain shifts in opinion, they cite instances of successful media manipulation. As an example, in 1949, 50% of the public favored a national health insurance plan, but this dropped by 8% in 10 months after it was attacked in the media as "socialized medicine." Likewise, the anti-inflation fervor of the 1970s and 1980s was influenced by a well-organized public relations campaign led by financial institutions. The most serious media manipulation occurs with foreign affairs issues. The Gulf of Tonkin incident was a case of successful media manipulation instigated by U.S. officials to win public support for bombing

North Vietnam. Page and Shapiro present an adjunct content analysis study in which they find a substantial effect of television news commentators on public opinion changes. They see the media focusing attention on certain issues and having a bias of being nationalistic, ethnocentric, procapitalist, anticommunist, and in favor of minimalist government.

The Rational Public's greatest contribution is the concept of "parallel publics." Most cross-sectional research looks at attitude differences among subgroups divided by income, education, race, gender, and so on. Page and Shapiro find that the subgroup differences persist over time and rarely converge or diverge across subgroup cleavages. For example, a 30% difference between blacks and whites on school integration has remained about the same for years. When public opinion shifts, all groups tend to shift in a similar direction with intergroup gaps remaining constant.

Despite heroic data compilation efforts, the authors do not fully exploit the data's potential. They rarely use regression analysis, examine interaction effects, or probe period effects. They see long-term opinion trends as a result of education, urbanization, and migration, but the discussion is without depth. Many interpretations of opinion change have a "post hoc" flavor; the authors seem to have scoured recent history for events to make opinion changes appear to be "rational" reactions.

The theorizing is a bigger disappointment. The authors quickly move from data presentation to broad generalizations about policy preferences, and they are not cautious in treating opinion responses as policy choices. They leap over any discussion of constraints on what enters public discourse as a policy question or of the narrowness of viable policy options. Instead, they devote attention to media manipulation and barely acknowledge the underlying structure of power or repression. Moreover, they fail to question the stability of public opinion and apparently assume it is normal and reflects an underlying value consensus and satisfaction with the status quo.

In sum, *The Rational Public* reviews mountains of survey data on "policy issues" with fleeting forays into recent history. The authors find support for "democratic theory" or pluralism. They see well-meaning but busy citizens following their own self interests and tempering this pursuit with shared values about national identity and the common good. The public's political knowledge comes from the media, which is slightly biased and sometimes manipulated by elites. In the authors' optimism for the ideal of pluralist democracy, a rational public realizes most of its preferences in policy outcomes. The one flaw they see in this ideal is the information provided to the public. Page and Shapiro imply that honest media reporting and divergent information sources can thwart attempts by elites to manipulate a reasonable but divided public and can lead to a realization of the democratic ideal.

Latinos and the U.S. Political System: Two-tiered Pluralism. By Rodney E. Hero. Philadelphia: Temple University Press, 1992. Pp. vii + 207. \$49.95.

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While the 1980s was characterized as the decade of the Hispanics, the 1990s is a period of more attention and discussion about Hispanics by social science scholars. *Latinos and the U.S. Political System* by Rodney E. Hero represents an ambitious attempt to explore Latinos as a political force in American politics with a wide range of topics. The author lays out a challenge to scholars and to himself to examine Latinos in order to understand "them" in and of themselves and to relate Latinos to the larger U.S. political system. Hero raises some important questions about the "category" of groups and individuals called Hispanics/Latinos. He does so from the perspective of internal group considerations as well as that of the relevance of Latinos to the larger sociopolitical system.

Rodney Hero outlines several major concerns and problems in examining Latinos and politics. The first one is defining group parameters. The umbrella terms, "Hispanics" or "Latinos," encompass a diverse set of individuals in terms of national origin, historical experience in the United States, structural relations, and group reference. Irrespective of these variations, Hero acknowledges that in common life situations and from the outside (i.e., by political representatives and the media) Latinos are treated as a common group. The second area of concern is the role and pattern of group identity among Latinos. What are the range of identities and labels associated with Latino group status? What are the sociological, political, and individual dimensions that contribute to the development of group identity? Are the forces of assimilation longer term factors that erode any real semblance of group identity? Finally, the author raises the proverbial question about the significance of ethnicity in the political arena. Does it serve the role as an important independent variable in the explanation of political attitudes and behaviors?

To pursue these concerns, Hero turns his attention to the analysis of established frameworks to understand group status and behaviors. The dimensions of power, access, systemic relations, and differential treatment are introduced by such models as pluralism, coalitional bias, internal colonialism, and a parallel set of theories derived from sociological literatures. To assess the relevance of these models, he establishes some key dimensions including the history of each group in the United States, society's perceptions of the groups, the role of prejudice, power relations, the nature of group conditions, and group reference points. In essence, Hero finds strengths and weaknesses with each framework analyzed. Thus he introduces his idea of two-tiered pluralism as an appropriate framework from which to understand Latinos and politics.

The middle section of the book represents his attempt to relate Latinos to the larger U.S. political system. He lays out brief histories for the various Latino national origin groups, then looks at different arenas at the national level and at state and local levels with a strong emphasis on urban political systems. The various aspects of political participation that he discusses include perception of discrimination, group identification, political orientation, ideology, and policy concerns. Hero relies heavily on the works of Clarence Stone and Paul Peterson on matters of resource allocation and systemic power and relations.

He returns to his theme of the significance of Latino politics in itself and how it appears relatively invisible to scholars in the discipline. In his discussion, he makes comparisons between Latinos and African-Americans in terms of power and politics. The author seeks to probe into the key areas and issues pertinent to Latinos and the U.S. political system. His primary vehicle is an extensive discussion of the Browning, Tabb, and Marshall book, *Protest Is Not Enough* (Berkeley and Los Angeles: University of California Press, 1984). He cites certain inadequacies of scope of political relations (i.e., relational politics vs. nondecisions, systemic power, and anticipated reactions). In addition, he finds that Paul Peterson's schema of policy allocation arenas is limited to the allocation of resources for existing policies in the book by Browning et al. As a result, Hero revisits the two-tier pluralism model to explain Latino politics.

The strengths of this book are in the introduction and the final two chapters. It is here that Hero raises important issues that relate to Latino politics (whether by itself or in mainstream scholarship). The attempt to cover both areas with an ambitious range of topics makes the content run a little thin. The middle chapters provide a quick map of the extant literature but not much discussion or insight. Pursuing the issues raised at the beginning would have provided significant contributions to the subfield and the discipline. The two-tier model is a variation of previous models that illuminate the ironies and contradictions of a democratic system for minority populations. The additional schema of power dimensions, policy levels, and stages of policy processes that would augment extant literature are not developed. Clearly, Hero points out important dimensions, factors, and relations that can focus analysis of race and ethnicity in U.S. politics. Unfortunately, the scope of his attempt limits his ability to expand on the specific understanding of Latinos in relation to the U.S. political system. This book is both interesting and directive of key issues concerning Latinos that need to be examined further.

Discrimination and Congressional Campaign Contributions. By John Theilmann and Al Wilhite. New York: Praeger, 1991. Pp. ix + 186. \$39.95.

Alan Neustadt
University of Maryland

Political action committee (PAC) campaign contributions have been used to study elite class structure, corporate politics, labor politics, and now we can add discrimination to this list. The basic argument of this book is that being black or a woman reduces electability through diminished campaign contributions in the form of contributions from PACs, political parties, and individuals. This is an interesting and testable idea, as John Theilmann and Al Wilhite show in their thorough analysis.

The first three chapters of this book are preparatory. Chapter 1 discusses four theoretical views of representation (descriptive representation, representation of constituent policy views, congruence of views of the electorate and the representative body as a whole, representatives acting in the "best" interests of their constituents), suffrage, and the early participation of women and blacks. Chapter 2 specifically addresses female and black representation beginning with the 1970s. Black representatives are normally elected from largely black districts (or at least 30% black), according to the authors, and have two types of problems in their elections: (1) how to convince black voters to register and to vote, and (2) how to avoid alienating white voters in mixed districts. These are compounded by the means used to dilute black voting strength including at-large elections, "cracking," "stacking," and "packing" (p. 22). For women, a significant path to Congress has been filling out their husbands' unexpired terms after their deaths; approximately 25% of all women elected to Congress between 1968 and 1975 gained office in this manner (p. 16). But they too had significant obstacles including a closed "old boy" club, reactionary voter attitudes toward women, lack of experience, and lack of party support (pp. 16, 24–25). Chapter 3 briefly reviews campaign financing from pre-Civil War through the Federal Election Campaign Act of 1971 and later amendments.

The meat of the book lies in chapters 4–7. Chapter 4 merits particular attention since the authors devote considerable time developing a "rent-seeking" (p. 60) model that attempts to describe how candidates and contributors use resources to capture a preexisting value—a seat in Congress for the former and access or influence for the latter. As far as aggregate contributions go, Theilmann and Wilhite use 1980–88 data to show that candidates are spurred to spend more by how much their opponents spend, the value they place on their office, and their other candidate characteristics (like incumbency). They dismiss the idea that voter bias leads to underrepresentation of blacks and women, but show

that blacks receive less aggregate funding than nonblack candidates and that there is little difference in funding of women and men.

The remaining chapters use variations of this model to examine contributions from the various types of PACs (corporate, labor, trade, membership, and health organizations, cooperative, and nonaligned special interest groups), the Democratic and Republican parties (analyzing incumbents and nonincumbents separately), and from individual contributors (analyzing large contributors separately from all individual contributors). Theilmann and Wilhite's control variables behave mostly as they have for other researchers: candidates in close races for open seats receive greater contributions than others, and Democrats receive greater contributions from labor and less from corporations. Finally, Democrats receive less from trade, membership, and health organizations. The results for the critical race and gender measures are more of a mixed bag, generally the coefficients are in the expected theoretical direction, and they are often statistically significant.

The authors present several key findings about PACs: (1) Incumbent black candidates generally did better than average in their contributions from labor PACs and below average from corporate PACs (after 1984) and trade, membership, and health PACs (for nearly every year examined). All the cooperative PAC coefficients are negative (1982 and 1984 are significant), and contributions from the nonaligned special-interest PACs are all close to the average. (2) There were fewer significant differences from contributions to women from the various PACs. The labor coefficients switch from negative to positive from 1984 on. None of the gender coefficients from the corporate or trade, membership, health, or nonaligned special interest PAC models are significant. All the gender coefficients for the cooperative PAC model are negative, but not significant from 1984 through 1988. (3) Nonincumbent blacks received more from labor than incumbent blacks. (4) Corporations largely underfunded blacks except for nonincumbents. (5) Nonaligned PACs tended to favor black nonincumbents. (6) Contributions to female incumbents were very similar to those to all incumbents and greater than those to black incumbents, although incumbent contributions were occasionally lower than expected from labor and cooperative PACs in 1982.

The authors' findings about parties include the following: (1) race and gender appear to make little difference to the political parties in determining incumbent funding; (2) party contributions to black nonincumbents are often greater than average; (3) few gender differences are found.

For individual contributions, effects of the candidate's race and gender are weak, with the coefficients switching signs often for women but remaining consistently negative for blacks. Also, race and gender have their greater effects when large contributions are examined.

I have several minor quibbles with the authors. Why do they not present their sample sizes? Why not include a measure of each district's percentage of black population in their model of voting percentages?

(This is a measure they employ later in the book.) However, these are inconsequential to the contribution made by this work. This is not a breakthrough book, but it is packed with careful operationalizations, skillful quantitative techniques, and most important, an enormous amount of information about the association between campaign contributions and race and gender. The material is clearly and logically presented and will be valuable not only to researchers in politics, but also to those who study race.

The Responsibilities of Wealth. Edited by Dwight F. Burlingame. Bloomington: Indiana University Press, 1992. Pp. ix + 149. \$19.95.

Martin J. Burke
University of Chicago

The majority of the papers in this collection were presented at a 1988 symposium at Indiana University's Center on Philanthropy, one of a number of recent academic establishments dedicated to the study of philanthropy by scholars and professional practitioners. As the book's editor, Dwight F. Burlingame, notes, the participants in that symposium and the text that resulted from it assumed that the wealthy "are in some way obliged to be philanthropic," and were concerned with the responsibilities of the wealthy in a capitalist society to use that wealth for the public good. What those responsibilities have been and should be are the subjects of these essays.

The most celebrated exponent of the obligations of the wealthy in American society was Andrew Carnegie, and his 1889 essay, "The Gospel of Wealth," is reprinted here. Carnegie and his message of social responsibility serve in turn as the subject of much of this volume. Rather than dismissing that piece as an amalgam of Chartist and Spencerian platitudes or as an exercise in self-serving rhetoric, Barry Karl and Kenneth Fox take its claims quite seriously and use it as a focal point for analyzing the ideologies and institutions of American philanthropy in the 19th and 20th centuries. Karl, in "Andrew Carnegie and His Gospel of Philanthropy: A Study in the Ethics of Responsibility," finds in that evangel a social theory of philanthropy quite distinct from traditional assumptions about charity. He suggests, indeed, that Carnegie may well be a leading theorist of the moral and social obligations of American capitalists. The "scientific philanthropy" proposed by Carnegie and institutionalized by his 20th-century bureaucratic successors assumed that the rich had a responsibility for more than the melioration of the conditions of the poor, they had the responsibility to change those conditions. Karl sees in Carnegie's libraries, Rockefeller's universities, and Sage's and Guggenheim's foundations a network that was "an adjunct to capitalism, not an apology for it." How that network operated and developed, what its relationships were with the state, and how it funded and

shaped the social sciences in modern America are questions posed but not answered in this often suggestive piece.

Questions about the distinctive constellation of American philanthropy and its relationship to society are also explored in Kenneth Fox's "A Businessman's Philanthropic Creed: A Centennial Perspective on Carnegie's 'Gospel of Wealth.'" To Fox, Carnegie broke decisively with the accepted norms of 19th-century philanthropy; he was more an iconoclast than an apologist. His creed of its being the personal duty of the individual with wealth to transform the moral order of society, not merely reinforce it, and his emphasis on the businessman as the agent of that transformation ran counter to the principles and practices of the elites who dominated American charity. Carnegie's reconfiguring of philanthropy and his recasting of the businessman made for a powerful combination, Fox argues, and led to the strategic alliance between philanthropists and social reformers that was a critical feature of Progressive reform. Yet the very success of Carnegie's creed among businessmen led to its routinization in the forms of corporate giving as the age of the engaged, industrial capitalist gave way to that of the modern corporate bureaucrat.

The new philanthropic ethos of Carnegie and his companions, as depicted by Karl and Fox, was one still grounded in a discourse of moral certainties and one in which the flow of moral and financial investment was still decidedly one way. While perhaps the most influential model of the duties of the rich, it was not the only one, and in her essay on "Jane Addams's View on the Responsibilities of Wealth," Louise Knight argues that in the settlement-house movement there was another persuasive and pervasive way of understanding these relationships. Knight finds in Addams's career at Hull House a far more open attitude toward the poor and the possibility for what she calls "cross-class social relations." The women of wealth who staffed Hull House were there not only to minister to the poor but to be enriched by contact with them. While certainly not culturally relativist or egalitarian, their model was far less patriarchal than Carnegie's.

Since *The Responsibilities of Wealth* is only a collection, it may not be fair to fault it for its scope or depth. But since it is the first volume in a series that will explore the history, ethics, and dimensions of philanthropy in the modern world, one could hope that future volumes address themselves not only to the scholarly concerns of philanthropy specialists, but to those in the social sciences interested in such areas as social welfare, public policy, and the interactions of capitalism and the American state.

Inventing the Nonprofit Sector and Other Essays on Philanthropy, Voluntarism and Nonprofit Organizations. By Peter Dobkin Hall. Baltimore: Johns Hopkins University Press, 1992. Pp. xiv + 349. \$36.95.

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University of Michigan

Within the past two decades we have seen a burgeoning interest in the study of philanthropy and the nonprofit sector. New journals have been created, handbooks published, and research and training programs established. Largely funded by well-established foundations, scholars from a number of social science and humanities disciplines have joined with management theorists and practitioners to map, improve, and sometimes celebrate the growth of philanthropic and nonprofit activities. Peter Dobkin Hall, a sociologically informed historian at Yale, brings a distinctive voice to this effort. Informed by modern institutional theory with its emphasis on the social construction of organizational form and process, by a sense of the relationship of class and status to politics and social control, and by the historian's ironic sense that enables him to highlight the strange twists and concatenation of events and positions, Hall illuminates the making of the modern nonprofit sector.

The volume consists of nine essays, several revised from previous publications. Four of the essays (chaps. 2, 6, 7, and 8) contain extensive reflections on the state of current scholarship and its interrelationship with the foundations that fund it and with public policy. These essays are balanced in tone and thoughtful, but neither their historical nor their sociological insights are deep. They read as if they were prepared for foundation executives and for scholars contemplating the development of a program of research.

Three of the essays focus on the current scene and the recent past. Chapter 2, "Reflection on the Nonprofit Sector in the Postliberal Era," chapter 4, "Conflicting Managerial Cultures in Nonprofit Organizations," and chapter 9, "The 'Me Generation' Grows Up: Thoughts on the Near Future of the Nonprofit Sector," are essays in current history. They ask how the nonprofit sector and organizations have been affected by trends in professionalization, by politics and political attitudes, and by organizational developments. These essays, too, are thoughtful and balanced.

The heart of the volume is found in three chapters that are substantial contributions to our historical understanding of the transformation of the nonprofit sector. Chapter 1, "Inventing the Nonprofit Sector," chapter 3, "'A Bridge Founded upon Justice and Built of Human Hearts': Reflections on Religion and Philanthropy," and chapter 4, "Cultures of Trusteeship in the United States," make up more than half of the volume. These three essays challenge quick generalizations and could well serve as an introduction to the contested and constructed nature of philanthropy and the nonprofit sector.

Hall traces the process by which private corporations (profit and non-profit alike) emerged. Remember that the Jeffersonians saw chartered corporations as potential challengers to republican government. Most early corporations were hedged by restrictions and tied to legislative processes. Although we now see Harvard University as a private secular university and tend to believe that it is only since World War II that the state has intervened in its management and policies, a longer view reveals that it was in fact a creature of the state and the Congregational church. Its governing boards were drawn from the church and from government officials, and it was perceived as a public institution, since endowments provided a small fraction of its income. Even as late as the Civil War, Massachusetts senators served as its overseers.

The construction of the nonprofit sector also reflected local and regional political attitudes and images of social reform. Hall is especially good at showing how different models of industrial society and the relationship of elites to local institutions shaped models of the public good and the interaction of philanthropy and the public sector. Boston and New England elites developed a model of "civil privatism," in which a network of self-selected elites controlled institutions for what they perceived to be the public good. On the other hand, in the Midwest a more collaborative model emerged in which private and public efforts were seen as complementary. Mark Hannah and Andrew Carnegie were leaders in these efforts.

The emerging secular nonprofits had an uneasy relationship to established religion. Although we think of religious organizations as having charitable orientations parallel to those of philanthropists, industrial philanthropists were caught up in the rationalizing and scientific ideologies of the late 19th and early 20th centuries. Since they were often social Darwinists and sought organizational forms and projects consonant with a scientific orientation, soft charity had no place in their emerging ideology.

Hall maintains his constructivist orientation even when he looks at more recent events. He notes that the modern study of philanthropy stems in fact from the efforts of philanthropists and professional managers to justify and defend their organizations from congressional attacks. Although the nonprofit sector is now well established and, indeed, is increasingly the operating arm of the social services, it continues to be economically and politically redefined as ideologies and interests collide. The so-called independent sector is deeply interdependent.

The question with this book, as with many collections, is whether these essays belong together. They are uneven in quality and only loosely integrated and do not really make a book, yet several of them are sterling. The three that I have highlighted ought to be required reading for anyone interested in the making of the modern organizational and associational world.

Strategic Bankruptcy: How Corporations and Creditors Use Chapter 11 to Their Advantage. By Kevin J. Delaney. Berkeley and Los Angeles: University of California Press, 1992. Pp. x+214. \$23.00.

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University of Amsterdam

Chapter 11 bankruptcy is a part of federal law now familiar to ordinary newspaper readers, who have come to realize that it refers, in some way, to corporate enterprises that stay operating while officially bankrupt. Millions of passengers flying Continental in the 1980s may have wondered why its airplanes could still fly when the corporation itself was bankrupt. The paradox was resolved by the cryptic formula, "Chapter 11," but then Chapter 11 of what? And what was it like to be bankrupt under chapters 1-10?

Questions like these, and many more, are answered in Delaney's fascinating and scholarly book. Chapter 11 was a major facet of America in the Reagan years, and it has not yet been eclipsed. This figures, since Chapter 11 comes from the new Bankruptcy Act of 1978. This was only the fifth major bankruptcy law in American history, and Chapter 11 consolidated, with considerable revisions and extensions, Chapters 10, 11, and 12 of the Chandler Act of 1938, which dealt with business bankruptcies. This was an emergency act of Congress passed to deal with the vast number of business failures arising out of the Great Depression. The idea was that it should be possible for an insolvent corporation to continue operating while a formula was found that allowed for both its own survival and the satisfaction of its creditors. Now with the 1978 act, the only alternative to Chapter 11 is Chapter 7, which provides for liquidation, a process that means no more than the realization of the bankrupt's assets with the proceeds to be distributed among different classes of creditors according to such priorities as the law may entitle them to. This of course is still the standard procedure for an individual who dies insolvent.

Chapter 7 is the standard bottom-line procedure. It assumes that, at a given point of time, the balance sheet shows an excess of liabilities over assets. The law states who may then file for bankruptcy, whether it be the insolvent debtor or one or more of the creditors. Historically, a debtor placed in such a position was an economic and social outcast, subject to a fate such as Charles Dickens describes in *Little Dorrit* (where most of the action takes place in London's Marshalsea Prison).

Chapter 11 questions the basic assumption of Chapter 7 that insolvent businesses should not be allowed to continue in operation. Actually, in the bad old days they often did so, with the hope, sometimes justified, that their insolvent status would not be discovered before they had in fact "rounded recovery corner" (to quote President Herbert Hoover's misplaced appraisal of the U.S. economy in his 1932 election campaign) and returned to solvency. After all, the alternative, even when it no longer meant a debtors' prison, was still not very attractive. And as

Robert Maxwell has now shown us, this is not just a question of the bad old days.

Chapter 11 assumes that an insolvent business should, with court protection, be given the chance of recovery. This can be seen as no more than public policy, protecting not only creditors, but employees, pensioners, business suppliers, and many other economic categories involved in the operations of a giant corporation. The point to Chapter 11 is that it puts the normal liabilities of the corporation on ice, so that unsettled claims to damages in tort, labor rights under union contracts, contractual rights to stockholdings—to give three actual cases discussed by Delaney—can all be renegotiated. The corporate poker player can simply ask to be dealt a new hand. Then, if the new cards are played correctly, the bankruptcy is discharged, and the corporation is out of the woods.

This prospect is so attractive to corporations facing a major financial crisis that Chapter 11 has become a major instrument in business strategy, one that is used so much that Delaney talks of corporations that are “bankrupt but not *really* bankrupt” (p. 81). This position is achieved by inflating liabilities in relation to assets to a level that produces a negative balance. Once the court accepts this, everything is up for grabs, in a game that can leave old employees with half their former salaries or asbestosis victims with damages settlements far below their strict legal entitlements. This of course makes it possible to revalue liabilities at a far lower level, thus restoring corporate solvency. This is a game with winners and losers, and Delaney’s message is that the losers are little guys who have been outsmarted by the use, or misuse, of legal strategies in the interest, if you like, of Wall Street. He proves his case in a way that almost any reader must find both attractive and convincing.

The Structure of Corporate Political Action: Interfirm Relations and Their Consequences. By Mark S. Mizuchi. Cambridge, Mass.: Harvard University Press, 1992. Pp. xi + 299. \$37.50.

James Burk
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Social scientists have quarreled for over four decades about how business interests were organized to influence the political process. To oversimplify, elite theorists argued that business interests were highly organized by members of the dominant or ruling class to exert significant influence over political decisions, assuring that the state served their class interests. Pluralists, in contrast, argued that business interests were quite diverse and often competing and that the political resources of business were certainly not controlled by any one class that was consistently able to direct political decision making for its own behalf. More recently, state-centered theorists have argued that the state was, in any case, relatively autonomous, able to withstand political pressure from the business com-

munity and to make decisions that opposed at least the immediate interests of particular business corporations. The quarrel has been hard fought because of its important implications for claims about the prospects for democracy. In this book, Mark S. Mizruchi turns the debate away from the question, Is business politically unified (and thus influential)?, to the more fruitful one asking about "the conditions under which political unity and opposition occur within business" (p. 32). Treating business unity as a variable, he provides a rigorous empirical analysis of the factors that facilitate similar political behavior among large manufacturing firms and tests whether their behavior affects political decision making.

His argument rests on a relatively simple, but powerful, structural model of corporate political behavior. It assumes that the business community is conflict-laden but can nevertheless control the state when "mediating mechanisms" overcome conflicts to permit common action. The two most important types of mediating mechanisms are network-based ties of economic interdependence (market constraint) and social cohesion (direct and indirect interlocking boards and common stockholders). Other factors, measuring shared corporate interests (geographic proximity, same industry, capital intensity, etc.), are also taken into account as predictors of business unity. Business unity is defined by a dyadic analysis of the similarity of political behavior (measured by PAC contributions in 1980 and testimony before Congress from 1980 to 1987) by 57 large manufacturing firms. Mizruchi posits that when businesses engage in similar political behavior they also heighten their political influence, that is, their ability to achieve their goals.

The major findings are compelling. First, the network measures of economic interdependence and of social cohesion are consistently strong and significant predictors of similar political behavior. These results do not require (although they may allow) that firms collude on political strategies. On the contrary, similar behavior may result from "coercive strategic convergence" rather than identity of interests (p. 244). Second, shared interests among firms lead to similar political behavior, but the effects of interests are weaker and not as consistent as those of the network variables. This suggests that corporate unity results more from the external pressures of an equivalent structural position than from the internal pressures of common political assessments. Third, when industries are more unified they exercise more power—they have more success getting what they want from the political arena—than when they are less unified. Moreover, the same factors that predict similar political behavior also predict the level of political influence. Overall, these findings lead Mizruchi to conclude that the key source of power in business derives from the position firms occupy in interorganizational networks of market and social constraints.

Mizruchi's great accomplishment in this book is to make appear obvious and incontestable what has been bitterly debated, the idea that businesses do act with considerable, though not perfect, unity to exert, what is from their point of view, effective influence over political decisions. In

addition, he has indicated the conditions under which that unity and influence is likely to obtain. Furthermore, he has done so in a work that is exceptionally well written and well argued. Oddly enough for a work that emphasizes the conditional nature of business unity, the general model is not placed in historical context to consider whether factors shaping business unity vary over time and from place to place. To his credit, Mizruchi identifies this as the next thing to do. Yet there is, I think, another thing to do besides—to consider directly the relative importance of corporate political action and influence. Although Mizruchi correctly claims that “the issue of business unity is at the core of the debate over the extent to which American society is democratic” (p. 32), he simply asserts that what citizens do fails to matter (p. 34). Structural factors may determine some political outcomes, but are they all-determining? The question is important, if democracy is a term with any meaning. Under what conditions does corporate political action override what citizens would otherwise choose to do?

Responsive Regulation: Transcending the Deregulation Debate. By Ian Ayres and John Braithwaite. New York: Oxford University Press, 1992. Pp. viii + 205. \$39.95.

Anne M. Khademian
University of Wisconsin—Madison

Good interdisciplinary studies can catalyze research across disciplines and provide new insights for practical policy applications. Through the combined use of assumptions and methodologies that drive different disciplines, interdisciplinary work can pierce polarized debates or arguments that have expanded to the point that they no longer provide “explanatory specificity or power.” In *Responsive Regulation: Transcending the Deregulation Debate*, Ian Ayres and John Braithwaite do just that. Their argument combines economic and sociological applications to surpass the polarized theoretical debate over free markets versus government regulation and to develop a richer understanding of regulatory alternatives that will be valuable for practitioners and scholars of regulatory policy alike.

“Responsive regulation” is distinguished by two fundamental premises. First, responsive regulation differs from other approaches to market governance both in what prompts intervention and in the form that regulatory intervention takes (p. 4). In other words, one size does not fit all. Industries vary in structure and levels of competitiveness, individual firms vary in their motivations, goals, and responsiveness to rules and regulations, and individuals within firms are differently motivated depending upon their backgrounds and positions within the firm. For example, an individual or a firm might be motivated by the desire to make

money, by "caring goals," or by a "lexical ordering" of both (pp. 21–30). From this premise, the authors examine several approaches to responsive regulation—"the benign big gun," "enforced self-regulation," and "partial-industry intervention." Though conceptually distinct, the approaches each emphasize the "efficiency" associated with a restrained use of command and control procedures by regulators, backed-up, nevertheless, by various means of enforcement available to the regulators.

The second premise is that regulatory responsiveness must embody a "version of republicanism" that requires community participation and debate among those interested in and affected by regulatory policy. Consequently, there is a need for the "organizational empowerment of disorganized constituencies" (p. 18). As both a venue for cooperation and a critical check to the policy process, the authors make an argument for "tripartism," or "a regulatory policy that fosters the participation of PIGs (public interest groups) in the regulatory process. . . ." (p. 57). The argument for tripartism is built upon the authors' interdisciplinary approach that combines an understanding of economic and socializing institutions; specifically, they argue that a regulatory ethic can be learned that facilitates "efficient joint cooperation" (p. 70) and overcomes the inefficiency of a regulatory prisoner's dilemma, that is, the dominant strategy to cheat, on the part of the regulated, and to apply sanctions, on the part of the regulators. The argument is distinct from corporatism in its emphasis on participation as a means to nurture *community* concerns, rather than as a means to maximize the economic self-interest of associations and in its emphasis on competing public interest groups and direct public participation, particularly at the local level (pp. 17, 81–97).

Ultimately, *Responsive Regulation* is an argument for alternative forms of accountability (secured through public participation and a regulatory ethic that is community oriented) that will allow for alternatives to the rigidities of regulations written to limit the worst possible offender. This is a tall order. While the authors do a convincing job of demonstrating the wide variety of motivations driving the decisions of the regulated, as well as the effectiveness of different regulatory strategies employed by regulators to build cooperation and trust, the reader is less convinced that citizen participation, or tripartism, is as workable.

Citizens have many avenues of participation that are incredibly underutilized, beginning with the right to vote. A rational choice explanation emphasizes the irrationality of participation when the costs to the individual outweigh the benefits. As the authors point out, the reduction of individual decisions of self-interest can become irrelevant when everything, "including even altruism," is explained by the psychic self-interest derived from the act, for example (p. 23). Nevertheless, there are clearly costly barriers to participation in the regulatory process that are difficult if not monumental for citizen participation. Further, even if means to overcome barriers are in place, many interests will still have a dominant strategy to free ride.

The authors argue for the institutionalization and support of public interest groups to overcome these barriers, as well as the development of a regulatory ethic that is community regarding. Yet the selection of appropriate "public" representation is clearly problematic, especially where there are not competing groups seeking to play the role of public watchdogs. Indeed, in many instances it might require a regulatory agency's cultivation and development of such a group. Given that the watchdogs could easily become clients, it is not clear how we have avoided the "inefficient" forms of capture discussed by the authors. Nor is it clear how a community-regarding ethic is to evolve and take hold without imposing one vision of an appropriate "community" over another. Nevertheless, the authors compel the reader to confront this skepticism—perhaps a product of our current regulatory ethic postulated on the assumption that individuals and firms will seek to maximize their self-interest.

The Market Experience. By Robert E. Lane. Cambridge: Cambridge University Press, 1991. Pp. viii + 630. \$65.00 (cloth); \$24.95 (paper).

Neil Fligstein
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Economists had a debate in the early 1950s over whether or not the utility-maximizing (and hence, wealth-maximizing) model of action was a realistic view of human motivation. The winners of that argument suggested that this stylized view of action was important only for its use in prediction, not as a guide to real action. On the basis of their success in economics, economists have more recently argued that the utility- or wealth-maximizing model of action can be applied to all spheres of social life.

I revive this debate because Robert Lane has produced an incredibly important book that questions whether or not the model actually gives predictive leverage from its two key assumptions: people make calculations about their market behavior to maximize wealth and the generation of wealth makes people happy. If we accept Lane's theoretical argument and voluminous empirical evidence, then the stylized economic model may be good for discussions of allocative efficiency where it allows us to predict outcomes on the basis of price theory. But, it may not be so good in helping us to understand what people want and get from markets and, by extension, from other social settings.

At the basis of his critique is a psychological model of action whereby individuals' pursuit of happiness is affected by both affect and cognition. Sometimes he seems to be saying that action is impulsive and then rationalized after the fact, and at others, that cognitive choices reflect underlying states, and at still others, that the two dimensions are orthogonal and

each contributes to our framing of action (chap. 4). One cannot tell if Lane's psychological model is more based on Freud with an emphasis on brooding emotions or is more attuned to modern psychology with its emphasis on affect and cognition. Gyration between the two seems to mean that actors are sometimes motivated by positive goals like happiness and sometimes by more negative emotions like insecurity and self-destructiveness (chap. 24 plays off of this contradiction in an interesting way). Lane's murky account of motivation is helped somewhat toward the end of the book (chap. 25), when he presents a "psychocentric model of satisfaction."

The interesting question he poses based on this view of human behavior is, What do people get in their encounters with markets? He assumes, like economists, that people strive for happiness and satisfaction (i.e., utility). It turns out that people get happiness from their friends, family, and feelings of efficacy in the world (chap. 25). From their role as consumers in the market, they get material satisfaction, but not happiness.

Much of the book explores the issue of efficacy (chaps. 5, 7, 8, and 13–16). Lane argues that engaging in tasks that require cognitive complexity helps people feel good about themselves, makes their lives meaningful, and by implication, makes them happy. This implies that our work roles are more important to us than our roles as consumers (chap. 16). He presents the provocative argument that once someone has achieved a certain level of material wealth, money does not buy happiness. Indeed, participating in meaningful interactions at work that are satisfying does much more for self-esteem, personal efficacy, and, hence, happiness than additional income.

His very complex argument is corrosive of economics in a number of ways (pp. 599–600). First, the economic model of action that postulates that people are income maximizers does not capture what they really want from the market nor how they behave. Second, while market economies are set up to satisfy consumer preferences, these are secondary to most people, who on the basis of a different cognitive makeup would prefer meaningful work. This shift to the intrinsic aspects of work depends on having already attained a certain level of income. In spite of these criticisms, Lane is a great fan of market society in the sense that markets have had a generally positive effect on individual psyches as well as on material well-being (chaps. 10–14).

Lane ends the book by arguing that public policy should be more concerned with providing more meaningful work. Unfortunately, his psychological theory and evidence undermine this prescription. He makes the case that education and cognitive complexity can give rise to self-reflection and brooding (chap. 4), thereby contradicting what he says in a later chapter (chap. 16). If this is true, a society with more complex work settings would absorb more of people's time, take them away from their friends and family, and create more neurotic people as opposed to happier ones. Indeed, Lane could conclude instead that people would get

more happiness from additional leisure time to spend with friends and family, a chance to pursue other mentally challenging tasks, and free health care. Many of us in the advanced industrial societies now have the luxury to consider how to make our lives better, but does that knowledge and the ability to find more meaningful work produce happiness or deeper feelings of angst?

The Rise of Historical Sociology. By Dennis Smith. Philadelphia: Temple University Press, 1991. Pp. x+231. \$49.95.

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The Rise of Historical Sociology is a book about the postwar emergence of historical sociology in the United States and Britain. At the core of the book is a set of paired comparisons of 18 major historical sociologists and "sociological" historians. These discussions are quite useful, especially since some of the writers and works covered by Smith have not been included in earlier overviews. Many American sociologists will welcome the discussions of slightly less familiar British historical sociologists. The sections on Perry Anderson, Braudel, T. H. Marshall, Moore, and Skocpol are particularly good (the treatment of Tilly, by contrast, ignores his work in the time between *The Vendée* [Cambridge, Mass.: Harvard University Press, 1976] and *Coercion, Capital and European States* [Cambridge: Blackwell, 1990]).

The book is less successful in its ambition of writing a "historical sociology of historical sociology" (p. ix). This project consists largely of dividing post-1945 historical sociology into three periods and relating the thematic foci, style of work, and "characteristic mood" (p. 4) in each period to the general political conjuncture. The historical sociology of the first period (the late 1940s to the early 1960s) was shaped by Cold War assumptions of "liberal orthodoxy." Its central goal was to celebrate the "American Way" and promote it worldwide. The second phase, starting in the 1960s and lasting into the 1980s, responded to the new social movements and the rediscovery of domination and inequality within capitalist society. The third era began in the mid-1970s "under the impact of the fragmentation of the stable bi-polar world of the Cold War" (p. 5) and continues into the present. Its *pièce de résistance* is the macrosociological comparison in the extremely *longue durée*, focused on international political and economic systems.

This chronological scheme has a certain immediate surface validity, but it breaks down on closer inspection. The unity of each of the phases is constituted at the level of research style. Only with difficulty can the political context be brought into alignment with these intellectual forma-

tions. It is unclear, for example, how books appearing 15 years before the collapse of the Soviet Union could have been influenced by the breakdown of the "bi-polar world." Even Michael Mann, a key representative of Smith's third phase, wrote in 1988 that the stability of the socialist system "cannot be in doubt" (quoted by Smith, p. 130). Disjunctures between the political and intellectual levels also appear in the sorting of authors into particular phases. Anderson and Wallerstein are placed squarely in the third period because of the global and chronological sweep of their writing, even though their political "mood" is more closely associated with the second. By contrast, Skocpol is discussed primarily as a "sixties" person associated with the second phase (but overlapping with the third), even though *States and Social Revolutions* (New York: Cambridge University Press, 1979) is conditioned partly by a rejection of neo-Marxism and explicitly criticizes the earlier books of Anderson and Wallerstein.

To make sense of historical sociology sociologically, it would have to be conceptualized as a field with its own internal dynamics and conditions of intellectual production. The book's final chapter promises an admittedly provisional analysis of "'the historical sociologist' as '*homo academicus*'" (p. 156), but this does not solve any problems. The 18 historical sociologists are distributed here into a series of tables that plot characteristics like "scientific detachment" against "involvement," or "outsider" against "insider" status. The arbitrariness of the dimensions and the coding decisions is the least of the difficulties with this analysis (one wonders, for example, why Wallerstein is considered more of an "advocate" for subordinate groups than, say, E. P. Thompson). More troubling is the fact that the tables do not serve any explanatory purpose.

The book's other implicit goal is to construct a coherent tradition, a progressive intellectual history. Smith traces an ongoing concern throughout postwar historical sociology with questions about why the West is "different," and with the relations between capitalism and democracy. Like all such exercises this one produces a series of exclusions. Research on non-Western areas or guided by "non-Western" questions is marginalized. The description of an evolutionary process culminating in Braudelian studies "in the grand manner" (p. 183) obscures the recent emergence of less macroscopic forms of historical sociology focused on themes of identity and culture and relying heavily on primary sources. It is symptomatic that Smith only mentions Geertz and Foucault in passing, and pays no attention to historical work on race, ethnicity, and gender, or to "anthropological" historians like Darnton, Ginzburg, and Sewell. The kinds of quantitative historical sociology associated with the Social Science History Association are also ignored. At the 1992 ASA meetings, the comparative-historical sessions included papers on Iraq, South Asia, the Caribbean, and Taiwan, with theoretical approaches ranging from "subaltern studies" to rational choice theory. *The Rise of Historical Sociology* ultimately imposes an extremely narrow definition on an intellectual field increasingly characterized by its very heterogeneity.

William Dorsey's Philadelphia and Ours: On the Past and Future of the Black City in America. By Roger Lane. New York: Oxford University Press, 1991. Pp. xiv + 483. \$29.95.

Alice O'Connor

Social Science Research Council

Ever since W. E. B. DuBois published *The Philadelphia Negro* in 1899, social scientists have recognized Philadelphia as an excellent site for studying African-American urban life. Home to a small community of free blacks since the early 1700s, Philadelphia had a larger African-American population than any other northern city for most of the 19th century. In Philadelphia, scholars also have a wealth of archival resources at their disposal, including collections on African-American culture and institutions at Temple University and the Pennsylvania Historical Society as well as the invaluable historical data base compiled by the Philadelphia Social History Project. In *William Dorsey's Philadelphia and Ours: On the Past and Future of the Black City in America*, Roger Lane uses a newly discovered archival resource to create an engaging portrait of black life in late 19th-century Philadelphia.

Lane begins by arguing that the roots of the modern African-American experience are to be found not on the plantation but in the 19th-century city, specifically in the four-decade period from the Emancipation Proclamation to the turn of the century. Philadelphia is "archetypical" of the African-American urban experience, Lane argues, not only because of its geographic position as a hub along the South-North migration stream, but also because it acted as a magnet for the diverse array of immigrant groups with whom blacks lived and competed in the late 19th century. And although he characterizes the period as "the most hopeful in the history of black Americans" (p. xi), the story Lane constructs features struggle, frustration, and frequent setbacks in the face of racial discrimination—experiences that constantly threatened to undo the achievements of Philadelphia's black community.

To tell this story, Lane relies heavily on the documents compiled by William Henry Dorsey (1837–1923), founder and custodian of the American Negro Historical Society. Dorsey was a son of Philadelphia's black elite—his father ran a successful catering business—and an alumnus of the prestigious Institute for Colored Youth. Dorsey gained local notice as an artist, and held a variety of other income-producing positions, most notably as "personal messenger" to Republican mayor Bill Stokely from 1879 to 1881. But it is as a collector, keeper of a vast and extraordinary archive of biographical files and scrapbooks, that Dorsey is now known to history. In addition to chronicles of eminent African-Americans, Dorsey's scrapbooks contain invaluable material for social historians: newspaper clippings from around the country, recipes, menus, meeting notes, social announcements, and other artifacts from the day-to-day lives of "ordinary" black Philadelphians.

Mixing documents from the Dorsey collection with census data and other sources, Lane offers detailed analyses of the social, economic, and institutional life of late 19th-century black Philadelphia. A section on "Making a Living" digs beneath official census data to construct occupational histories for blacks at all class levels. Poverty, a severely limited range of opportunities, and racial discrimination were the lot of the vast majority, as both "unskilled" and professional were excluded from the industrial economy. Lane also captures the considerable diversity in black work experience, and indicates that internal class division as well as the external forces of racial and economic oppression were factors in hampering the emergence of a cohesive black power base in the city.

In contrast to the constraints of economic life, black Philadelphians enjoyed a rich and lively "associational life," with African-American churches playing an important secular as well as spiritual role. Where class interests drew people apart, he argues, the "web of organization" woven around religion, race pride, and cultural and recreational activities drew the community together, fostering the creation of a distinctive African-American urban civilization. Though at times anecdotal, Lane's discussion is thoroughly documented and offers a generally convincing depiction of the sometimes conflicting "pulls" exerted by race and class on the black community.

Lane's final section is uneven, relying more on personal reflection than on scholarly analysis to consider the contemporary African-American urban condition. He starts off with a very useful analysis of the up-and-down story told by indicators of black economic, social, and political status since the 1870s, underscoring the tragic reality that, just when it finally opened up to black participation in the 1940s and 1950s, the urban industrial economy began to die, soon to be replaced by a postindustrial economy that remains "inhospitable" to blacks. But his subsequent discussions of the War on Poverty and of the "underclass" are more impressionistic; he bases his discussion of the latter on interviews and uncritical use of secondary sources. While he gives great weight to structural factors in his discussion of contemporary black urban poverty, Lane's analysis of poor blacks' "cultural" handicaps is based on the undifferentiated imagery of passivity, alienation, and hopelessness characteristic of much contemporary social scientific literature—an imagery he successfully penetrates in his discussion of the 19th-century poor. Despite these drawbacks in his concluding section, Lane's book does make a contribution to the "underclass" debate, drawing attention to the pre-Great Migration urban experience and bringing historical depth to our understanding of the continuing patterns of racial discrimination and economic exclusion that plague American cities today.

New Philosophy of Social Science: Problems of Indeterminacy. By James Bohman. Cambridge, Mass.: MIT Press, 1992. Pp. x+273. \$32.50.

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Eckerd College

At the outset, the social sciences were in tension with the humanities and the natural sciences. Like those in the humanities, we studied human phenomena, but we claimed that, unlike them, we used the empiricism of scientific method. We borrowed our empiricism from the natural sciences, but, we claimed, the study of human phenomena requires different methods because of the need to understand subjective meaning. Early social scientists embraced empiricism as a way to extricate the study of human phenomena from seemingly endless debate among competing schools of interpretation in the humanities. As James Bohman suggests, empiricism and its allied claim to the unity of scientific method held out the promise of progress at the heart of positivism: The "facts" will allow us to recognize the "better" of two competing interpretations; advances in theoretical understanding will lead from explanation to prediction and from prediction to amelioration of social problems.

But there were those who, from the beginning, questioned the epistemological underpinnings of positivism. They challenged the old logic of social science by calling for the interpretive study of human phenomena. In *New Philosophy of Social Science*, Bohman argues that the repudiation of positivism has eroded what was once a clear distinction between the humanities and the sciences. Bohman does not mourn the passing of the old logic of social science. Indeed, he rejects the unity of scientific method as well as the search for general laws governing rational social action. According to Bohman, the fundamental problem with positivism is its failure to come to grips with the epistemological implications of agency and indeterminacy. He contends that when reasons are treated as causes, the resulting laws are "either vacuous or false" (p. 57). The postempiricists embrace indeterminacy, but Bohman believes that many critics of positivism have gone too far. He describes their position as a form of skepticism; from their standpoint, there are no privileged representations because it is impossible to demonstrate that one interpretation is better than another.

Bohman wants "to reintroduce normative and rational criteria for deciding between competing theories" (p. 3), but he wishes to do so in a way that acknowledges agency and indeterminacy. He sees the emergence of a new logic of social science—one in which the old essentialist search for general laws is supplanted by "an empirically oriented analysis" (p. 66) of multiple approaches to explanation. In the new logic of social science, human conduct is viewed as being conditioned by rules rather than laws. These rules allow for agency and indeterminacy because their use in everyday life is characterized by flexibility, negotiation, and the making of exceptions. Instead of describing the new logic in

formal terms, Bohman (p. 66) elects to evaluate the "explanatory practices" of what he deems "successful research programs": rational choice theory, ethnomethodology, and the theory of communicative action. Each of these research programs represents an effort to explain rational action and surmount the problems of indeterminacy by apprehending the various ways in which rules shape human conduct.

By evaluating the relative merits of these research programs, Bohman hopes to demonstrate that indeterminacy does not preclude the possibility of recognizing the better of two or more competing theories. He identifies four criteria by which research programs can be judged, and each of the criteria can be phrased as a question for the proponents of a particular theory. First, how is the fact of agency consistent with the assertion that social action follows rules? Second, how can social scientists use "norms of public evidence" (p. 132) in the development of "fallible and revisable claims to interpretive correctness?" (p. 124). Third, does the research program explain the relationship between structure and agency by specifying "empirically verifiable" (p. 173) linkages between micro- and macrolevels of analysis? Fourth, does the research program explain human phenomena in such a way that it facilitates the derivation of a critical theory for the sake of evaluation, reform, and progress? Bohman tries to show that the theory of communicative action is superior to ethnomethodology and rational choice theory because, more than the others, it embraces the implications of indeterminacy in its answers to the foregoing questions.

Bohman favors the theory of communicative action not because of a superior fit with empirical materials (he has none), but because that research program exhibits greater conformity with his prescriptions for theory construction. Thus, his book is exhortatory rather than exemplary. Without an empirically driven demonstration, Bohman relies on an all-too-familiar tradition of exegetical analysis based on what the author thinks everybody knows. Still, Bohman has tackled the core issue confronting the social sciences—an issue that will decide how we position ourselves relative to the humanities and the natural sciences. Moreover, he is quite persuasive in arguing that we can transcend a narrow contextualism by reconciling indeterminacy with verification. If he is right, those of us committed to an interpretive sociology need not resign ourselves to skepticism.

Back to Sociological Theory: The Construction of Social Orders. By Nicos P. Mouzelis. New York: St. Martin's Press, 1991. Pp. 214.

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University of Denver

Nicos P. Mouzelis claims that contemporary theorizing is enervated by an obsession with epistemological and ontological foundations, a penchant for merging sociology with philosophy, and a hostility to allegedly

contrived disciplinary boundaries that encourages scholars to appropriate insights from other disciplines without recasting them in a manner suitable for sociological analysis. This genre of theorizing, which Mouzelis calls social theory, undermines the autonomy, specificity, and distinctive logic of sociological discourse, widens the gap between theory and research, and in significant respects represents a regression to pre-Durkheimian forms of social studies.

There are many alternatives. For Mouzelis the most promising option is to go back to the Mertonian way. Merton's appeal resides not in any specific theory but in the style of intellectual craftsmanship he exemplifies: clearly formulated theorizing that illuminates existing analytic puzzles by introducing innovative concepts and articulating their interrelationships in a manner that contributes to ongoing research.

Mouzelis uses the micro-macro debate as an opportunity to demonstrate the utility of the Mertonian strategy. He proceeds by clarifying and reworking three key concepts: the duality of structure, social and system integration, and social hierarchies. Giddens's treatment of subject-object duality is overly restrictive, Mouzelis asserts, because it ignores the variability of actors' orientations to rules and the existence of hierarchically organized collective actors and their differential contribution to the reproduction and transformation of social structures. These omissions can be corrected by supplementing duality with dualism at both the paradigmatic and syntagmatic levels.

Mouzelis accepts Lockwood's initial distinction between social integration and system integration but underscores the importance of moving (theoretically) discussions of inconsistencies and contradictions (on the systems level) to examinations of collective actors and their strategies for maintaining or transforming the status quo (at the social level).

Drawing on Mann's work, he argues that social hierarchies are a constitutive element of all complex societies and insists that no statement on the micro-macro issue that ignores hierarchies can be fully satisfactory. He goes on to specify the connections between that concept and the ideas of duality/dualism of structure and social/system integration. Incumbents of subordinate positions relate to games played at higher organizational levels in terms of syntagmatic dualism while relating to rules initiated from above predominantly in terms of paradigmatic duality. The opposite combination—that is, syntagmatic duality and paradigmatic dualism—obtains for incumbents of superordinate positions when they “relate to games and rules respectively at lower organizational levels” (p. 103). He links hierarchies and system/social integration by suggesting that social hierarchies be viewed both as vertically organized positions (system integration) and as a series of hierarchically related social games (social integration). Moreover, it is useful to envision social systems themselves as hierarchically arranged.

The utility of Mouzelis's approach is evident in two ways. First, he highlights the empirical fertility of his argument by regularly referring to substantive literatures, particularly studies of development and formal

organizations. Second, he identifies the analytic foibles of competing formulations that have ignored the concept of social hierarchies, failed to observe the rule of system/social integration balance, or disregarded the internal logic of subordinate systems or the emergent dimensions of superordinate systems. In this regard, his chapters on reification and reductionism are particularly compelling.

My evaluation of *Back to Sociological Theory* is two-fold. First, the polemic against social theory strikes this reviewer as overstated. Furthermore, the relations between social and sociological theorizing are more complex than the sort of contrast conception presented in the introduction and first chapter—a point Mouzelis appears to acknowledge in qualifying statements throughout the text. On the other hand, this book is a welcome and incisive addition to the growing literature on the micro-macro problem. Mouzelis's criticisms of alternative approaches to this issue are penetrating and enlightening. Finally, the notion of social hierarchies and the reformulations of the duality/dualism of structure and social/system integration represent thoughtful, useful, and significant contributions and should engage every scholar involved in this important debate.

Modernity and Self-Identity: Self and Society in the Late Modern Age.
By Anthony Giddens. Stanford, Calif.: Stanford University Press, 1991.
Pp. 256. \$35.00.

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One of the dominant perspectives of contemporary sociology was formalized by Erving Goffman as the "presentation of self," the central idea of which is that people "negotiate" or adjust and accommodate to social circumstances, often with minimal conscious awareness. Reading Goffmanesque analyses is like looking in a mirror with running commentary. Implicit in such analyses is the notion that contemporary persons are less secure in the knowledge of who they are and thus more inclined to try to be the persons called for by the situation. Less secure and more inclined, that is to say, than persons in "traditional" society.

In *Modernity and Self-Identity*, Anthony Giddens uses the above proposition to ask just what it is about modernity that evokes this protean propensity in us. Wanting to distance himself from "post" modernists (with whom he has here a gentle, intermittent debate), Giddens labels the present age as "late" or "high" modernity, preferring to see current formative forces not as qualitatively different from modern forces but as modern forces intensified. He also parts company with the prevailing view that as modernity replaces small community with large bureaucracy, it leaves individuals alone and in need of "therapies" that in some sense replace the confessional. Giddens calls this view "substantially inadequate"—valid to a point but failing to see that the modern self is

a reflexive self. People are not just “alone” in the modern world but also have vast, and vastly new, resources for dealing with their situation. *Modernity and Self-Identity* analyzes these resources.

A first analysis is of the development and maintenance of trust, what Giddens calls “ontological security.” Failure on this front diminishes one’s creativity in engaging ever-changing life circumstances and leads to shame (which replaces guilt, the reaction to failure to carry out *un*-changing expectations). A second analysis, therefore, is of what persons do creatively to avoid shame, and the high point of this book for me is Giddens’s identification of the ten sequential “elements” whereby this is done: One moves from the recognition that a “self” is not a “thing” but a “reflexive project” (step 1), that is trying to “self-actualize,” which means balancing “opportunity and risk,” to reach “authenticity,” via ad hoc “passages,” to a personal integrity that allows the self to be “internally referential” (step 10). Success in this endeavor means the capacity to enter into “pure relationships” of mutual trust—the best late modernity can offer to replace traditional, ascribed relationships.

Bad things still happen, of course, but now they are not seen as “fate” since we understand so well not only how they happen but also our own part in why. An illustration offered is that of the sufferer from back pain who, faced with multiple possible therapies, must make a choice knowing that success is not guaranteed and thus responsibility for failure will be partly his or her own. In response to such situations, we “colonize the future,” that is, try to manipulate the future by minimizing risk (as in life insurance plans), even as we recognize its unpredictability.

Yet another strategy is what Giddens calls the “sequestration of experience”: late modern people conceal from themselves a number of experiences that might otherwise paralyze the choice-making process. Such concealment is purchased at a price, however, and the final chapters identify some of these costs and then—in probably the most innovative section of this book—show how “life politics” has significantly replaced “emancipatory politics.” That is, with many late modern people already “free” of traditional constraints, the task now is to live a morally justifiable life without any obvious substitutes for those constraints.

The modern self is therefore not a “minimal self” but the “experience of large arenas of security intersects,” constantly challenging us with the task of “remoralizing” in what we know to be a world without “transhistorical ethical principle.”

Sociology contributed to the destruction of those transhistorical ethical principles, so it is not inappropriate for sociology to assist in the analysis of subsequent developments. *Modernity and Self-Identity* does just that. Giddens’s book is not an easy read, in part because of the profusion of neologisms, but the effort is worthwhile.

Language and Symbolic Power. By Pierre Bourdieu. Edited with an introduction by John B. Thompson. Translated by Gino Raymond and Matthew Adamson. Cambridge, Mass.: Harvard University Press, 1991. Pp. 302. \$34.95.

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Language and Symbolic Power presents, in English translation, most of the essays in a 1982 collection that Pierre Bourdieu entitled *Ce que parler veut dire* (What language means to say), along with four more Bourdieu essays on symbolic power in political representation. While one wonders why any essays from the original book were deleted, the connection of the essays on politics with the essays on language and discourse is valuable, throwing into bold relief Bourdieu's fascination with the politics of naming. The power of instituted authorities to make words into truths and laws is the volume's recurring theme, and Bourdieu's purpose is to write against this power. Bourdieu mistrusts "the origivative capacity" of language, "no doubt the principal support of the dream of absolute power" (p. 42), and seeks to free sociology from the domination of linguistics and its concepts (p. 37). Sociology should instead begin with "a critique of political reason—a reason which is inclined to commit abuses of language which are abuses of power" (p. 250).

His argument is that elites depend upon symbolic power for their position and privileges. Dominated people acquiesce because language imposes schemes of classification that produce a form of recognition of social order that is also misrecognition, hiding the arbitrariness of its foundations. Political activity leads to power and profit only when an apparatus is created that institutes and delegates authority to represent a group. Thereafter the people represented are captive to the interests of the apparatus within the autonomous field of political professionals. If "political science" is complicit in this and fails to unmask the misrepresentations, linguistics as described by Saussure, Chomsky, and Austin also fails to reveal the real workings of power in language codification and use. In their conceptions of *langue*, competence, and felicity, linguists treat the social basis of authority in language practice as formal intrinsic aspects of language itself. In short, to Bourdieu the origivative capacity of language is sinister, because it is used to hide and disguise power relations. "Discourses are always to some extent euphemisms" (p. 78).

This insightful, ponderous book will reward readers interested in language and discourse theory and those interested in Bourdieu. For the former it makes a strong case against formalism, idealism, and structuralism. For the latter it clarifies strengths and weaknesses, powers and limits of Bourdieu's theories, and in particular, the politics of his intellectual vision. For Bourdieu, the mission of social science is to unmask hidden operations of power, and his premise is that other approaches reproduce structures of domination by failing to unmask them. This tight moral

compass leads him to dismiss some vast and burning questions as irrelevant (e.g., the scope of genetic structuring of language skills, [p. 259, n. 19]), and to pronounce casual, virtually a priori verdicts on others (notably, "Cultural difference is probably the product of a historical dialectic of cumulative differentiation," [p. 286, n. 4]). This moral vision is also the buttress for the central pillar of his theorizing: his universalization of capitalist economics into a general theory of social spaces, positions, and tactics. Only his theory of power, he argues, can keep power from dissolving and can track symbolic power not only through transformations but also back to substantial foundations in various sorts of capital (esp. pp. 163, 170). One might ask whether it is not Bourdieu himself who recasts the history of power into the language of the victors.

Bourdieu's generalized idea of capitalism sometimes verges on incoherence. For example, capital can be "shared" from a text (p. 156). If we grant it coherence, this science of social fields operates more akin to Newton's astrophysics than Einstein's. For all of Bourdieu's efforts to be dialectical, his spaces are not only defined by the relations of their contents, but also by the generalized capitalist scheme that, like Newton's space and time, is absolute and unaffected by the histories of the contents and relations. Thus the discourse must always exist within a "given" market (p. 76), beliefs and institutions precede rites (a startling reversal of Durkheim [p. 126]) and, crucially, discourses made by people are never really as real as the underlying invariants (Newton invoked God; to Bourdieu objectivity and power are natural, a startling reversal of Marx and Foucault). All this makes less charming Bourdieu's Franco-centrism (his tendency to pass seamlessly between general rules and French illustrations) and the shallowness of his historical and cultural imagination. History's effect on structure is mainly the growth of increasingly autonomous spaces. Spaces come into being but they never die, and nothing can really bend them. Accept this absolute, and the contents of discourse and culture are valueless except as vehicles for social profit.

Myth as Foundation for Society and Values: A Sociological Analysis. By Pierre Hegy. Lewiston, N.Y.: Edwin Mellen Press, 1991. Pp. iii + 217. \$69.95.

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Has America lost its soul? Are its citizens indifferent to the collective good? Are its religious and republican traditions dead? Drawing on the pessimistic sides of Tocqueville and Bellah, Pierre Hegy says yes. But Hegy's reasoning, which relates the state of society to the content of its central myth, may be more valuable than his conclusion.

Focusing on the social functions of myth, Hegy provides an alternative to postindustrial and postmodern models of contemporary society.

Myths, he explains, are more than "idle rhapsodies," more than an "aimless outpouring of vain imaginings" (p. 1). Myths are cultural forces, symbolic charters for the political, economic, educational, religious, and domestic aspects of social life. Yet, myths change constantly. "The old gods," as Émile Durkheim put it, "are growing old or already dead, and others are not yet born" (p. 42).

Adopting Durkheim's statement as his motto, Hegy devotes the first part of his three-part book to the Enlightenment's politicization of nature. The Enlightenment, he shows, conceived a mythical "state of nature," with its "natural laws" and "natural rights," to counter the divine laws and divine rights claimed by tyrannical monarchs. The myth of nature, however, only supported new masters. Hobbes's and Locke's versions of the myth legitimated the tyranny of the successful and wealthy; Rousseau's legitimated the tyranny of the masses. Hegy believes that the French Revolution, more than any other event, transformed these myths into objects of scrutiny rather than devotion. The Revolution proved that all rights derive from religious and political traditions, not from enlightened intellectuals' philosophical abstractions. Natural laws, as Joseph de Maistre observed, are mere words, easily replaced as one majority replaces another. Only the nation's "higher will"—its transcendental moral values—can create fundamental laws that no majority can abolish.

The myth of the state of nature failed because it ignored tradition and collective consciousness. The second great myth, the myth of the nation, failed not as a theory of life but as a way of life. The foremost sociological interpreter of the nation, as Hegy asserts in the concluding chapter of part 2, was Émile Durkheim. For Durkheim, as for his predecessor Ernest Renan, nationhood embodied the fullest development of society. Renan had defined the nation as a soul, a spiritual principle resulting from a long past of sacrifice and devotion. Durkheim conceived society as an ideal, a sacred object of commitment rather than a mere organization of individuals and institutions. If Durkheim defined society idealistically, however, he treated it mythically. Hegy's distinction is subtle and important. As an ideal, the nation suggests cultural and institutional models that individuals may accept or reject; as a myth the nation is dogmatic, an absolute requiring unquestioning and universal dedication. This is what Durkheim meant when he declared god and society to be one.

By the start of World War I, the myth of the nation had peaked; after the war's end, in Hegy's view, disillusionment set in and the myth deteriorated. By the end of World War II, it was dying. How, then, are we to live in a world without great myths? Such is the question Hegy poses in the book's last part. The myth of the nation began to die during an era of intellectual and moral relativism, a time when pragmatic problem-solving procedures ("paradigms") replaced the search for absolute truth, when respect for moral diversity replaced the search for moral absolutes and moral consensus. In this context, the United States is "be-

coming the first amoral society of modern history" (p. 135). Hegy considers his claim to be pivotal and generalizable. Once any nation's morality is relativized, it must formulate a new myth to recover its integrity.

As Hegy begins the last chapter, his undocumented argument loses coherence. He discusses the uses of language among humans and advanced primates, relates the origin of language to toolmaking, and sets forth the invariant stages of linguistic and moral development, placing special emphasis on that "higher level of [reflexive] consciousness" which is the "driving force of human evolution" (p. 183). Since cognitive growth and communication define the human condition, Hegy regards them as the ultimate "natural rights," their universal achievement the final myth. He also believes this myth will flourish under a new global consciousness—one that leads to redistribution of the world's resources, demise of the old myth of progress, and emergence of a new world of real affluence.

Pierre Hegy succeeds admirably, in my opinion, as an intellectual historian but fails as observer and prophet. I cannot believe, as he does, that previous American generations were morally superior to the present generation. Nor can I accept his claim that the industrial and postindustrial revolutions' achievements—the loosening of mankind from nature's grip—have made the struggles of life more rather than less ferocious. Above all, I cannot conceive of cognitive growth and communication as a myth. Myths are narratives that illustrate the things society holds sacred. With the possible exception of the university, one is at a loss to imagine circumstances under which growth and communication become sacred objects of contemplation and ritual.

Pierre Hegy's *Myth as Foundation for Society and Values* is thoughtful, informative, and engaging, but some parts of it, notably the last parts, are casually argued. The book, indeed, carries its own myth—the myth of the Evil Present—which tells of the industrial democracies, led by the United States, passing through a final stage of moral decadence. Hegy articulates his utopia better than his golden age, but a new emphasis would not change his book's intellectual cast. That the present is at once a morally diminished version of the past and a morally stunted version of the future is the myth that Hegy must transcend.

Virtual Reality. By Howard Rheingold. New York: Summit Books, 1991. Pp. 415. \$22.95.

J. Timmons Roberts
Tulane University

Howard Rheingold is not a sociologist, but, in his book *Virtual Reality*, he does look at some of the potential social effects of the little-discussed revolutionary technology that is the book's subject. In spite of virtual reality technology's potentially enormous impact on how humans work,

live, and interact, it has yet to be included in the sociological literature.

Virtual Reality (VR or "cyberspace") is a rapidly developing field of computer science that is creating its own fast-growing lexicon. In its current embodiment, the user attaches a "head-mounted display" of stereoscopic computer monitors and a "data-glove," while walking on a treadmill or other motion-sensing device. As the user turns his or her head or moves his hand or walks, the computer detects the changes and provides the correspondingly changing view. The effect is of being inside a computer-generated world, and imagination is all that limits the forms that can be taken there by humans and objects. Rheingold reports that "the first cybernauts realized very early that the power to create experience is also the power to redefine such basic concepts as identity, community, and reality. VR represents a kind of new contract between humans and computers" (p. 386).

Rheingold provides plentiful details on some uses of VR that are practical and already in place. For example, architects already use AutoDesk, a system of hardware and software that allows them to take clients through a building before it is built, making changes with keystrokes rather than with a wrecking ball or sledge. Soon surgeons will be able to plan and even rehearse delicate operations with the use of sonograms and virtual reality technology. The Air Force and NASA have been using increasingly complex VR technology in flight simulators for years, and army units around the world already practice warfare in a virtual world called SIMNET. Other, more mundane virtual worlds already exist in global information networks used heavily by academics and businesspersons. Those who have used Bitnet, Internet, the WELL and other computer mail services, bulletin boards, and information networks know already the experience of participating in a "virtual community" on line.

Other uses of VR are fanciful and far from being realized. They include the creation of imaginary worlds in which two people can interact but can take any physical form they choose. As Rheingold admits, "the Sex Question" comes up repeatedly, as do questions of virtual reality being merely "electronic LSD." Rheingold states that computer sex (called "teledildonics") lies far in the future but that "Privacy and identity and intimacy will become tightly coupled into something we don't have a name for yet." The rapid proliferation of phone-sex services has proven once again the potential of human interactions to be further abstracted, mechanically mediated, and commodified.

The effects VR technology is likely to have on the future of work are uncertain but loom large and surprisingly close. To his credit, Rheingold raises what should be a central question in discussing the effect of these new technologies on workers. "Computers can be wonderful amplifiers of human capacity, and computerization can be a soul-deadening juggernaut of alienation. Is virtual reality the ultimate form of alienation, or can it be used as a tool to diminish some of the alienation that has already occurred as a result of computerization?" (p. 363). Rheingold is clearly an optimist, claiming that such interactive interfaces with computers

might return some mobility and intuitiveness to some factory jobs recently automated to computer-screen control.

Rheingold's book is probably best described as a Sears catalog of detailed information about each step in the development of VR technology, and his travel-writing style on his world-wide voyages to investigate the new medium causes the book to drag in many places. Rheingold raises many critical and interesting points but is often not rigorous in exploring them. The book suffers badly from the lack of a tough and careful editor. However, because so little has been written so far on the subject (a recent computer search unearthed a dozen short articles in popular magazines and one British collection edited by Sandra K. Helsel and Judith Paris Roth), Rheingold's book is timely and will serve an important role to bring neophytes up to speed and to document the early years of the technology.

Toward the end of the book Rheingold implicitly compares his role to that of Marshall McLuhan in his 1964 *Understanding Media* (New York: Signet) because he is looking forward with only a slight indication of what lies ahead. Like television, which McLuhan then called "the Timid Giant," virtual reality technology is still all potential: for good and bad. In the best of scenarios VR becomes the great physical equalizer of endowed characteristics—the users can choose their virtual forms, switching gender, race, or even species. Rheingold also intimates that without physical labels to direct social interaction, communication will develop differently. More realistically, it seems to me, VR will be a force for the increasing fragmentation of society for two reasons. First, as with computers, VR is likely to have few applications that require more than one person, thus reinforcing social isolation. Second, because of its sheer expense, virtual reality will exclude the "have-nots" for a long time to come.

Therein lies the importance of sociological scrutiny of the potential effects of the new virtual reality technology. As Peggy Orenstein, editor of *Mother Jones* magazine, said last year, "People who care about media, art, and education shouldn't just pass this off as the latest trend among techno-weenies with weird hair. This time, we have the chance to enter the debate about the direction of a revolutionary technology, before that debate has been decided for us." Rheingold, however, figures that "given the rate of development of VR technologies, we don't have a great deal of time to tackle questions of morality, privacy, personal identity, and even the prospect of a fundamental change in human nature" (p. 350). We had better get going.

The Future of the Mass Audience. By W. Russell Neuman. New York: Cambridge University Press, 1991. Pp. xiv + 202. \$44.50 (cloth); \$12.95 (paper).

Roger Silverstone
University of Sussex

As I wrote this review Los Angeles was still burning. In Great Britain, we saw not only the video of Rodney King's beating but extended extracts from the televised trial of the four policemen. We have watched murders, looting, and fires—in real time. Live. Dead. Disentangling the role of the media is not an easy matter: home video, and the real-life drama of the courtroom—the soap opera of the inner city—and now the news: “real” action that is only distinguishable from the fake in the quality of the images—more real because it is less real—less real because it is more so.

The reference to these contemporary events is not gratuitous. They provide a vivid contrast, in tone and content, to the arguments in *The Future of the Mass Audience*. This is a book—wide-ranging, intelligent, and well-informed but curiously limited—which offers an account of the dynamics of technological and media change in a context of the political economies of the so-called liberal democracies of the Western world. W. Russell Newman's central question is how significant changes in the mass media and information technology may affect the day-to-day functioning of the democratic process.

The new media and information technologies seem to have a *double life*. They can be seen, as Orwell paradigmatically saw them, as the potential or actual instruments of political domination, providing in their interconnectedness, the speed and efficiency of their transfer of information, and their relative cheapness, a powerful tool for those who wish to control a society and its culture. Or they can be seen as potential or actual liberators. Those very same qualities of speed, cheapness, power, and flexibility and interactivity (as well as the vulnerability of overcomplex systems to interference and sabotage) could provide the basis for a diversified, user-based pattern of different tastes and interests and individualized networks of communication.

Quite properly eschewing a technological determinist approach, but nevertheless recognizing that these technologies, and especially these technological systems, have potentials which in a sense “demand” to be realized, Neuman traces the various implications of their increasing presence in contemporary cultures. Technological forces do not determine social structure or cultural values, but rather interact with them. And it is no simple matter to read the signs. There are long-recognized pressures toward uniformity and homogeneity in mass culture, pressures that are both technological and politico-economic: economies of scale, the high cost of entry, the domination of multinational industries, and the declining significance of regional cultural economies as well as the pressures toward

homogenization that intense competition brings in its wake. These all suggest a more uniform, less sensitive media culture in which the rapidly increasing quantity and intensity of our media environment disguises the limited basis on which choices between content can be made.

But at the same time, the audience—the consumers and users—of these media, are part of the equation. Neuman reviews the literature on audience effects and media effectiveness. He discusses the passive and the active audiences. He suggests that we are limited in our capacity to absorb the explosion in information that is a principal character of the modern age. He sees our political ambitions as modest and our capacity to take advantage of the potential in the new interactive technologies, to engage in a heightened form of participatory democracy, as being limited by our own interests and aptitudes. The crucial variable between these technologies and their effects is indeed that of interest. We are conservative in our media use, and our capacity to see, let alone take advantage of, their radical possibilities is limited by that conservatism. Changes toward greater diversity and flexibility in media use will be modest. Audiences will fragment and will only modestly express their own tastes and identities in media use. The power of the centralized institutions, particularly of broadcasting, to define a media agenda and media culture will be sustained, but the consequences for our everyday lives will be slight. The liberal democracies of the world, and our lives within those liberal democracies, are neither threatened nor significantly enhanced by these technologies. The argument is both sensible and plausible.

But it is, at the same time, problematic. Neuman's model of the political process, of the cultural system that supports it and the social system in which it is expressed, is Parsonian. Power is seen as a generalized resource, and in the subtleties of its exercise, the systemic pressure for equilibrium is the bottom line. Such a model of contemporary society could be said to ignore many of those factors that it actually purports to represent. In this case it not only ignores the media's role in structuring and defining interest and motivation; it ignores the role of politics as such, and power as such. And it does not provide any route toward understanding the kind of crisis that was played out in Los Angeles and the media's crucial role in that crisis. Messengers have always been blamed for the messages they carry. McLuhan argued that this was, indeed, a correct perception. But no theory of the role of the media and information technology in contemporary society can safely ignore both the long-term and the short-term power of the image and the text both to transcend and to shatter the evolutionary destiny of techno-democracy. The complacency implied in a model of a self-equilibrating system does very little service to the conflicts and contradictions that lie at the heart of our contemporary media world.

Images of Postmodern Society: Social Theory and Contemporary Cinema.
By Norman K. Denzin. Newbury Park, Calif.: Sage Publications, 1991.
Pp. xii + 180. \$49.95 (cloth); \$19.95 (paper).

Emanuel Levy
Arizona State University West

Despite the fact that the sociology of culture and art has received a tremendous boost over the past decade, few social scientists have devoted their attention to the study of film as an institution or an art form. Norman K. Denzin's *Images of Postmodern Society* is a most welcome addition to the growing body of sociological literature on film.

Denzin claims most persuasively that an essential part of the postmodern American scene can be found in the images and meanings flowing from cinema and television, two ideological systems that have radically transformed American society into one dominated by a visual culture. Assuming that the postmodern self is defined by three crucial identities, class, gender, and race, his chief concern is: "How are these three identities enacted and dramaturgically staged in the postmodern culture?" (p. x).

The book is divided into two parts. Part 1, titled "The Postmodern," is a useful survey of postmodern social theory, focusing on the writings of three major thinkers: Jean Baudrillard, Jean-François Lyotard, and Fredric Jameson. American readers are, of course, familiar with the work of Jameson in cultural studies. But the section on the two French scholars is especially useful because some scholars are still unaware of their writings. Part 1 concludes with an insightful chapter on C. Wright Mills, with the author labeling C. Wright Mills's seminal book, *The Sociological Imagination* (New York: Oxford, 1959), as the first systematic treatment of the postmodern condition.

As expected, Denzin's postmodern perspective is eclectic. Following some basic definitions of postmodernism, the author states three postulates that serve as a framework for his book. First, reality is a staged, social production. Second, the real is now judged against its staged, cinematic-video counterpart. And third, the postmodern society is a dramaturgical society.

The book's second part, "Learning from Cinema," consists of a textual analysis of six movies, all made in the 1980s: *Blue Velvet*, *Wall Street*, *Crimes and Misdemeanors*, *sex, lies and videotape*, *Do the Right Thing*, and *Paris, Texas*. With the exception of the 1984 *Paris, Texas*, which was directed by German filmmaker Wim Wenders, the movies were directed by Americans.

Though the analysis of these films is not guided by a consistent critical orientation, each entry provides some illuminating comments. However, one of the book's main problems is the absence of a strong link between the theoretical perspective and the illustrative materials. Each part of the book can stand on its own. Moreover, because of the way the book

is written, it is often hard to distinguish Denzin's views from those of other critics; the author relies too heavily on film critics.

The book also fails to discuss why these six (and not others) movies were selected for scrutiny. Since all the chosen films are from the 1980s, one might conclude that the American cinema of the 1970s, arguably the best decade of American film since the Depression, does not lend itself to postmodern analysis. But postmodernism, as social theory and cultural perspective, has informed scholarship for at least two decades. Moreover, major European filmmakers (French Jean-Luc Godard, Italian Michelangelo Antonioni, to name only a few) have used a postmodern sensibility in their work, both thematically and stylistically. It would have been most useful if Denzin included at least one or two European films. In this respect, the book's subtitle, *Social Theory and Contemporary Cinema*, is a bit misleading, because its scope is limited to the American cinema and only that of the past few years. Moreover, the analysis of the six films is mostly restricted to their contents (ideas, values, messages), but it lacks an in-depth dissection of their visual styles.

Some of Denzin's claims are controversial because they are not sufficiently substantiated. For example, the author claims that the major theories of postmodernism fail to live up to their promise, because they still linger in the presence of the sociological classics. However, judging by the intellectual developments within culture studies, one may conclude that the entire field has been revolutionized (in subject matter and method) as a result of the new critical, postmodern paradigms.

Denzin's claim, that contemporary representations of postmodernism fail to offer anything more than superficial solutions to the present conditions, is also debatable. Is it the function of such a popular, and highly commercial, medium as American film to offer solutions to social problems? Oliver Stone (*Wall Street*, *JFK*) and Spike Lee (*Do the Right Thing*, *Jungle Fever*) make controversial films whose aim is to provoke and challenge their viewers, but they are first and foremost working directors concerned with the entertainment values and box-office profits of their movies. Besides, in their impact on values, reconstruction of American history, and perception of reality, films like *Platoon* or *JFK* have been extremely influential and anything but superficial solutions to relevant social issues. Can film (and art in general) aspire higher than that?

Denzin's book also suffers from technical deficiencies and a writing style that is not always clear. The book contains such vague and tautological statements as "Postmodern theory is a product of the postmodern decades" (p. 149), or "The postmodern is everywhere and nowhere" (p. 151).

Images of Postmodern Society is intellectually ambitious and always stimulating, but because of the book's structure and style it may not be easy to use in either social theory or film classes.

Microsociology: Discourse, Emotion, and Social Structure. By Thomas J. Scheff. Chicago: University of Chicago Press, 1990. Pp. xvi+214. \$29.95.

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Thomas Scheff's newest book is a collection of 10 essays summarizing his thinking on the sociology of emotions since *Catharsis in Healing, Ritual, and Drama* (Berkeley and Los Angeles: University of California Press, 1979). Anthony Giddens, in the book's foreword, asserts this work is "by any token a major contribution to some central debates in social theory." Indeed, it has some very important things to say about how people think, feel, talk to one another, and form the social bonds that are society's building blocks.

The central theme connecting Scheff's essays is the sociological importance of shame (and its counterpart, pride) as a signal to self about the state of one's social bonds. Scheff contends that when a valued bond (real or imaginary, past, present, or future) is at risk, the actor feels shame (broadly defined here as any of a class of negative self-emotions such as humiliation, mortification, and garden-variety embarrassment). If shame is not discharged through catharsis, he argues, it not only infiltrates the self-concept and prompts self-destructive behavior, it can also spiral into anger and motivate the actor to lash out at others. Thus shame both reflects threatened bonds and impedes further bonding. Modern societies generate shame (e.g., through arrangements based on class, gender, race, nationality, etc.) and at the same time discourage people from recognizing it. The modern social actor pushes shame below awareness. In Scheff's view, this "bypassed" shame is a steady, destructive force that inflames conflict between spouses, between community members, and between heads of state. In short, shame links the macrostructure with the everyday microworld where people form self-concepts and create, test, reaffirm, and shatter social bonds.

Scheff sets his discussion of shame within a theory of subjectivity and intersubjectivity. Incorporating concepts from Cooley, Peirce, Goffman, Schutz, Garfinkel, Goethe, and a number of theorists in psychiatry, linguistics, and artificial intelligence, it is a theory of implicit messages and hidden interpretive processes. According to Scheff, beyond the world of appearances (the "explicate order") lies an immense "implicate order" of unsaid, implied, suggested, or vaguely hinted at messages that make even a two-sentence interchange extraordinarily intricate. Moreover, both the "explicature" and the "implicature" have an improvised character. It is almost a miracle that human beings can make sense of the massively complex welter of information and innuendo that confronts us. We do so (albeit with a fair amount of misinterpretation and self-deception) through a process called "abduction," a lightning-quick shuttling back and forth between "outer search" and "inner search."

Outer search involves reading available cues from interaction. In inner search, also highly improvisational, the actor brings to bear on these cues memory, emotion, cognition, imagination, and years of experience with a society and its culture. The actor also imagines the other's inner search processes and how they relate to his or her own. Emotions, especially shame and pride, play a vital part in inner search, orienting self to others and self to self.

Further, abduction allows people to contextualize events and encounters both in an immediate and an extended sense. The "extended context" of an encounter includes all that has transpired between the actors in the past and the present. Thus Scheff concludes, "Each interpretation of meaning presupposes both the history of the whole relationship and the history of the whole society insofar as it is known to the interactants" (p. 115). Equally important, the extended context also includes "counterfactuals"—what might have happened but did not and possible futures that might unfold.

Microsociology also includes a proposal to use abduction as a sociological research method, a consideration of how emotions generate conformity, a discussion of turn taking and deference in interaction, a theory of genius *cum* critique of educational institutions, and more. It contains so many ideas that not all are addressed with data as fully as one would like and not all are carried to their logical limits. For example, Scheff could clarify his conception of the link between social structure and shame with a fuller explanation of when, how, and why people *intentionally* and *unintentionally* shame or instill pride in others. That is, he could outline the techniques or at least the general strategies people in privileged and nonprivileged positions use to put others in shame or keep them from it. He could also expand his theory by examining other emotions that create, threaten, or comment on social bonds. He begins to move in this direction with his concept of actors' "emotional attunement," but he could also consider some specific other-emotions (e.g., gratitude, liking, sympathy, and disdain) as well as other self-emotions (e.g., loneliness, loss, and hope). Nevertheless, Scheff provides a rich theory that can easily generate further exploration. And he drives home the message that sociological work on interaction, social bonds, and society cannot ignore human emotionality.

Mental Health, Race and Culture. By Suman Fernando. New York: St. Martin's Press, 1991. Pp. x + 243. \$39.95.

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The role played by culture in psychiatric diagnosis and psychiatry itself has been discussed and analyzed by anthropologists, psychologists, psychiatrists, and other social scientists. In *Mental Health, Race and Cul-*

ture, Suman Fernando, a psychiatrist, provides a thorough analysis of the role played by culture in Western psychiatry and goes a step further by incorporating the influence of race and racism into his analysis. He harshly criticizes the lack of attention given to culture and race in the Western practice of psychiatry. He argues that psychiatry is more of a social entity than the scientific specialty, free of cultural or racist bias, it claims to be.

The first part of the book is devoted to a historical overview of the emergence of racism and how the influence of a racist ideology in Western societies has permeated the development of psychiatry. In looking at the evolution of racism in Western societies, the author provides an enlightening discussion and analysis of the interconnectedness and differences between race, culture, and ethnicity. He writes that "race, culture, and ethnicity are interrelated in complex ways depending on historical, political, and social factors" (p. 11). After examining the findings from biological studies that have tried to establish differences across races, Fernando contends that race serves as a social marker rather than as a demarcator of biological differences.

According to the author, since Western psychiatry emerged within the realm of racism, the latter is an inherent part of the discipline. However, since Western psychiatry is governed by the scientific model, individuals are erroneously viewed independently from culture, race, society, and their environment. Fernando addresses the need to change this view, particularly when the maxims of Western psychiatry are applied to people in non-Western societies. For example, focusing on the problem of cultural imposition and the reductionist approach of Western psychiatry, the author illustrates that psychology, religion, and mental health are closely interrelated in many non-Western societies. Fernando concludes that the secular and "objective" approach of Western psychiatry is inadequate to study or treat mental health problems in those societies.

The second part of the book begins with an examination of the practice of psychiatry and its analogous forms in Western and non-Western societies. The author provides suggestions to implement a more universal mental health care system and makes an interesting comparison between British and American psychiatric practices among minority groups. He discusses issues concerning how diagnosis and therapeutic interventions are affected by the type of interaction established by the therapist and the client in order to shed light on the recurrent finding that minorities tend to (1) be overrepresented in various diagnostic categories, (2) show high treatment drop-out rates, and (3) receive treatments that are different from those given to members of the majority group.

In discussing the psychiatric treatment minorities receive in Britain and the United States, Fernando recognizes improvements accomplished in both societies, particularly in the United States. Still, the impression is given that very little has been done and that little is still being done. In this respect, the author fails to note that the National Institute of Mental Health and the American Psychiatric Association are playing an

active role in incorporating culture within the practice of psychiatry in the United States. A group of social scientists and culture experts are currently working to devise ways in which culture can be included in the upcoming *Diagnostic Statistical Manual* (DSM-IV). Thus, significant changes in the practice of psychiatry may be under way.

In providing suggestions as to how to implement mental health care across cultures, Fernando discusses various techniques used to treat mental health problems in different societies. Some of those techniques, he suggests, can be transplanted from one society to another. This appears to be contradictory after he has stressed the importance of understanding mental health problems and treatment within the particular sociocultural setting in which both emerge.

The first part of this book constitutes the most important contribution to the study of culture and mental health. The discussion and historical analysis of the evolution of racism in Western societies and how it pervades the development of psychiatry is revelatory and well researched. The second part of the book, however, is more fragmented. The focus of attention is diverted in many different directions and the reader is left with an unclear sense of what ought to be done to develop a more universal psychiatry. Much has been said about the problems Western psychiatry faces in dealing with culture; the most urgent need is to provide realistic methods for solving them.

The Imprint of Time: Childhood, History, and Adult Life. By M. E. J. Wadsworth. New York: Clarendon Press, 1991. Pp. xiv + 251. \$52.00.

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This book summarizes the results of research on a cohort of over 5,000 English, Welsh, and Scottish children who were born in 1946 and are interviewed periodically. The latest data used in this book come from 1989 when these individuals were 43 years old. The directors and personnel involved in the study have changed over time, and the institutional location of the headquarters for the study has changed a few times. But, throughout all of these changes in the study's leadership and in British society in general, a high percentage of the individuals who were in the original sample (approximately 61% in 1989) have continued to participate in the study. Since it is known that 18% of the sample have been lost to death or immigration, approximately 21% have withdrawn or cannot be found.

The book is divided into chapters that largely coincide with critical ages in the life cycle. Chapter 2 is entitled "Birth," chapter 3 is "The Pre-School Years," chapter 4 is "From 5 to 11 Years," chapter 5 is "From 11 to 18 Years," and chapter 6 is "Adulthood." Chapter 7 explores generational differences by comparing the characteristics of sample

members to those of their parents using data collected from the parents in the early years of the project. Although the themes of these chapters vary somewhat depending on what critical events are occurring during these particular ages, the topics of health, education, and employment recur in most of the chapters.

It would be impossible to summarize the key findings and insights that have emerged from this lengthy research project in a short review. The major shortcoming of the book itself is that it attempts to summarize years of research in a few short chapters. The book should be viewed as an introduction to this research, rather than a comprehensive summary. Those who are interested in further details and information about particular ages or in particular areas will find that the author has done a good job of citing previous research using these data, so the book does serve as a good introduction.

My own favorite insight to come out of this work is summarized neatly on page 198: "Many of the things which will in the long run affect the life of the child or adolescent do not have an immediately apparent impact, either because they are only the first link in a long-term chain or because the vulnerability they bring will be added to gradually as age increases, and they will not in this respect be vulnerable to challenges until an accumulation of problems has developed." This points out the value of longitudinal data on the same individuals that is a clear strength of this study.

This book is a worthwhile investment for those who are interested in child development and research on the life cycle. The book has little technical detail and is accessible to those at the advanced undergraduate level. Some of the research papers that are cited present more technical challenges to readers.

Language, Society and the Elderly: Discourse, Identity and Ageing. By Nikolas Coupland, Justine Coupland, and Howard Giles. Cambridge, Mass.: Blackwell, 1991. Pp. xi + 220. \$44.95.

Allen Grimshaw
Indiana University

Language, Society, and the Elderly is an interesting and important book that I wish would be read by more sociologists (among others) than are likely to read it. It should be read, minimally, by students of aging and the elderly and by researchers on language in use in social contexts. I suspect that the former may ignore the book because of its focus on language and the latter because of its focus on the elderly. Both these constituencies will err if they do not read the book—as will students of, inter alia, (1) identity formation and management, (2) intergroup prejudice, discrimination, and interaction (generational, in this instance), (3) multimethod research approaches, (4) socialization, and (5) stress and

coping. Finally, this work, in contrast to many scholarly books, can be read with profit by members of the general public—both those who are already “Wrinklies” (p. 15) and curious as to why they are sometimes given “no respect”—and younger people who complain about the garrulousness and self-centeredness of the elderly in general and dependent elders in particular.

The authors have used a variety of methods to collect and analyze talk of, to, and about elderly women to identify and describe such phenomena as the use of baby talk *to* the elderly, and convergent and competitive painful self-disclosure, age-telling, and trouble talk *by* elderly women, in order to investigate such larger social processes as the construction of interactional roles of the old by the young, the construction/acceptance/resistance to imposition of identities by the old, and discourse management and interpersonal control through the employment of the resources of language in use in social contexts. Justine Coupland was a participant-observer in interaction with elderly female kin. The researchers collected 40 videotaped, first-acquaintance interactions between peer-peer and cross-generational dyads and analyzed tape and slide programs prepared to train “home-helps” in delivery of services to clients living in their own homes. The team also conducted group discussions about the topic of their research with younger women and with older women living in residential facilities. Their methods of analysis are eclectic and pragmatic and include variants of conversational analysis, simple quantitative analyses, techniques of discourse analysis, and ethnographic interpretation. The authors acknowledge the female bias (p. 169) and urge its redress. My hunch is that inclusion of instances of talk of younger males to older women will reveal a compounding of ageism with sexism.

The investigators employ Communication Accommodation Theory (CAT) as a heuristic frame for their research. The basic notion of CAT is that people adjust their interaction activity, including talk, to either accommodate or not to interaction partners, thereby decreasing or increasing social distance and concomitant good or bad feelings, and so on. A fundamental fact of CAT is that it may generate a predicament “for those wishing to speak most ‘effectively’ to older people; [i.e.] that speech styles that convey support and nurturance are also liable to suggest an expectation of diminished competence” (p. 18; the finding is attributed to other researchers by the Couplands and Giles). This conundrum, and the fact that some older people are not hesitant to exploit beliefs about reduced competence to pursue goals otherwise unobtainable, can make the interaction complex indeed. Students of interethnic interaction will recall that similar stereotypes and similar exploitation exist in *that* arena. Students of socialization might argue that students of interaction of and with the old should attend to possibilities that, at the same time the young are constructing roles for the elderly, the elderly may well be constructing and building their own special culture. The reasons why cultures of children and of the elderly are ultimately very different may turn out to be profoundly instructive.

I disagree with some of the authors' methods, findings, and interpretations. My only substantial complaint about this book, however, is that the authors have ignored or neglected a number of studies and authors (e.g., nonexhaustively, Corsaro, Gumperz, Labov and Fanshel, O'Barr, Tannen) that deal productively with closely related methodological, theoretical, substantive, and applied issues and considerations. This last complaint notwithstanding, I think this is a valuable contribution and I hope that the several constituencies I mentioned at the beginning of this comment will read and attend to it.

The Other Half: Wives of Alcoholics and Their Social-Psychological Situation. By Jacqueline P. Wiseman. Hawthorne, N.Y.: Aldine de Gruyter, 1991. Pp. 290. \$39.95 (cloth); \$19.95 (paper).

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Seasoned veterans of the knowledge explosion inevitably develop shortcuts and rules of thumb to manage the vast amounts of information available today. One of my shortcuts directs attention to works by veterans such as Jacqueline P. Wiseman, who, for over two decades, has produced numerous books and articles that explore issues related to alcoholism and family relationships.

The Other Half reports the results of a cross-cultural research project that uses U.S. and Finnish data to compare the impact of alcoholism on the family in the two countries. Its specific concern is with the alcoholic's spouse, who is usually the woman.

The book begins with an analysis of how wives diagnose their husbands' alcoholism, which the author conceives as "the hidden drama on the home front," an indication of the life-long influence of Wiseman's mentor, the late Erving Goffman. The "data" available to an alcoholic's wife is typically ambiguous, uncertain, and fuzzy. In addition, the cultural standards and expectations about drinking behavior are varied and supported normatively by others to a greater or lesser extent. In Finland, for example, cultural standards direct males to engage in extremely heavy drinking over a short period of time for the explicit purpose of getting drunk. Wives and employers recognize that two- and three-day drinking bouts are common in Finnish culture. By contrast, in the United States, cultural norms are more varied, but generally involve drinking smaller amounts of alcohol over longer periods of time. For these and other complicated reasons explored by the author, the wife's deliberations and decisions about her husband's alcoholism are made with great anguish and reticence. In many cases, one or more events may "tip the scales," and this may lead to an escalation of such incidents. Once alcoholism has been suspected, wives then routinely monitor the environment for "clues" and "evidence" to either confirm or disconfirm their suspicions.

In most marriages, whether in Finland or the United States, the process commonly takes many years.

A singularly unique contribution of *The Other Half* lies in its presentation of data about "the home treatment" of alcoholism. This refers to the varied actions taken by a wife to try to stop her husband from drinking. These include discussions, accusations, protestations, persuasion, emotional pleading, nagging, and threats to leave. Some of these approaches are very direct. Most spouses at one time or another have destroyed their husbands' supply of alcohol. Other approaches are indirect. Some wives even try to diminish or control their husbands' drinking by joining with them and trying to match them glass for glass. While pervasive and universal, these home-treatment remedies inevitably and invariably fail, leaving the wife to ponder what to do next.

The process of seeking professional help for the husband usually involves a learning process for the wife, as she begins the process of reinterpreting past behaviors which were normatively supportive as now being "sick." The disease concept of alcoholism, the author argues, is more complex than the parameters of more transitory illnesses, as previously analyzed by Talcott Parsons. The process also brings the wife in touch with a wide range of community agencies and treatment facilities, and as the wife begins to interact more and more with these treatment specialists and professionals, we have the alcoholism version of "the betrayal funnel," originally analyzed in Erving Goffman's *Asylums* (Garden City, N.Y.: Anchor Books, 1961), where the wife and counselor or therapist now collaborate to gain political advantage over the erstwhile "patient." Many husbands use the therapeutic process cynically, says Wiseman, and this in turn leads many wives to lose faith in the alcoholism-as-disease idea.

The middle chapters of the book analyze the effect of alcoholism on the marital relationship. In even the most chronic alcoholism, the husband is sober much of the time, so the marital relationship vacillates between these moments of sobriety and the moments of alcoholism. When sober, alcoholic husbands direct their attentions to "being a good (sober) husband," and wives commonly encourage this pattern, which paradoxically produces considerable stress and pressure that pave the way for yet another "relapse." Wiseman is at her best in describing the impact of these chronic vacillations on social interaction at mealtimes, family recreational activities, and sexuality. She reports a surprising similarity between Finnish and American wives on these issues. While all of these interactional dynamics can be found in some nonalcoholic marriages, they were found to be more frequent in alcoholic marriages from both cultures.

Jacqueline Wiseman comments on the ironic features of alcoholic consumption: "It is perhaps one of the great ironies of social life that the ingestion of alcohol in large amounts defeats the very behavior it intends to enhance. People serve alcohol at parties to encourage sociability and conversation, yet continued overindulgence appears to curb these desirable social behaviors. . . . Shakespeare noted that alcohol increased sex-

ual desire, but decreased ability to perform. More tragic is the apparently almost permanent effect of long-term heavy alcohol ingestion on both the interest in sex and the potency of men. Wives of alcoholics are victims of the personal wreckage that alcoholism leaves in its wake" (p. 157).

The long-term effect of chronic alcoholism on the family and marital relationship can be and often is devastating. Nevertheless, few women readily or easily leave their alcoholic partners, not only because of the many well-known barriers and complications of getting a divorce, but also because of the typical sociopsychological adaptations of wives, which inhibit the steps to a divorce. Wiseman articulates an interesting version of the self-fulfilling prophecy as applied to alcoholics' wives. For both Finnish and American populations, one group of wives refocused their efforts on outside activities, employment, or other interests, whereas others did not refocus their efforts. This distinction turns out to be a meaningful one, separating wives who are relatively successful and happy in their adaptations from those who sink further into the abyss of depression and despair. Put differently, Wiseman found that nothing breeds success like success, just as despair breeds further despair.

Jacqueline Wiseman is a symbolic interactionist, and this book reflects the great advantages of this position. The behaviors and adaptations of wives are inevitably and invariably linked to their conceptions of self and how they define their present circumstances, which of course includes societal and cultural conventions. Such an approach cannot be dismissed as one that only studies "micro" phenomena, because all larger social and cultural structures must ultimately be found or embedded in the consciousness of social actors. This is a work of major importance and includes the comments and perceptions of many research subjects, common to this tradition, which allows the reader to access how well Jacqueline Wiseman has done her work. Seasoned veterans will conclude that she has done it very well indeed.

The Sphinx in the City: Urban Life, the Control of Disorder, and Women. By Elizabeth Wilson. Berkeley and Los Angeles: University of California Press, 1992. Pp. 190. \$35.00.

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University of Pennsylvania

Women experience cities; men design them. The tension between emotion and reason, feeling and doing, that characterizes feminine and masculine interaction with cities provides the thematic scaffolding for Elizabeth Wilson's *The Sphinx in the City*, an extended, and sometimes lyrical, essay on the nature of the city. Urban space is an arena where gender conflict plays itself out beside economic development and pleasure, danger, and chaos lurk behind ordered urban facades.

The city of architectural texts and planning documents is an aesthetic

object whose meaning is fixed. In contrast, Wilson argues that spectacle is the “urban-ness of urban life” (p. 158). Spectacle allows “face to face contact [in public space] of amazing variety and richness” (p. 158) that imbues the city with multiple meanings and creates the kaleidoscopic quality that fascinates and liberates. The image of the *sphinx* encapsulates Wilson’s thesis. According to Greek mythology, the sphinx was the female monster who would strangle male wanderers who found their way to the end of the labyrinth and could not answer her riddle. For Wilson, the labyrinth represents the spectacle city. The sphinx, the hidden core of the city, is unbridled sexuality and danger. Men fear the ambiguity of the sphinx and they order urban space to destroy it; women embrace this ambiguity and find power in it.

In six intertwined essays that range from 18th-century London to post-modern Los Angeles, Wilson reworks standard arguments regarding the split between nature and culture. She departs from conventional feminist wisdom and claims that cities are a source of danger to men and freedom for women. Wilson’s assertion that culture liberates women and nature constrains them challenges socialist visions of antiurbanism. The history of the modern city is the history of a masculine assault on feminine disorder. All cities have planned public and civic spaces, but the excitement and beauty of a city exists in the interstices inhabited by disorderly groups—the human components of spectacle. Urban planners and architects rationalize pleasure by walling off spontaneity. Prostitutes and criminals are jailed or exiled to red-light districts and ghettos. The market takes over where the planners fail. The bohemian Soho of artists becomes the yuppified Soho of investment bankers.

Romantic optimism transferred to an urban locale is only one feature of Wilson’s account. She realizes that the sphinx was myth and that part of the excitement and potential for opportunity that the city offers women is *chimera*. The women who populate Wilson’s account are secretaries, salesclerks, unsuccessful artists, and assorted bohemians existing at the margins of economic viability. The educated and “successful” women in Wilson’s book are as eager as their male counterparts to impose order on women’s lives. Mass consumption permits the salesclerk to copy the dress of her bourgeois customers, but the saleswoman inhabits a room in a boardinghouse, and the customer owns her home. The feeling of exhilaration and possibility that the city offers the unattached woman has a dark side. If the urban planners are the central forces of order in this narrative, the prostitute is the central force of disorder. Prostitution is pleasure and possibility gone awry as women who are unable to earn living wages turn to the peddling of desire to support themselves.

Wilson marshals novels, memoirs, architectural treatises, government documents, and a vast array of secondary sources to buttress her arguments. In a brief 150 pages, she presents a vivid, if idiosyncratic, portrait of urban life in London, Paris, New York, Chicago, Vienna, Brazil, and Los Angeles. Wilson juxtaposes; she does not argue. She describes her book as a “collage” that aims to recreate the “experience of city life

itself" (p. 10). The book's absence of linearity works because Wilson is a clever stylist who chooses her examples well. We learn how the term "ragpicker" originated in 19th-century Paris and how boardinghouses developed in the United States, about the bohemian community that existed near Chicago's Water Tower in the 1920s, and that Le Corbusier aspired to become Mussolini's principal architect.

The book's evidence is not always persuasive. Nowhere does a woman's voice, besides the author's own, state that the city was a source of freedom, and much of the narrative suggests the opposite. Urban sociologists and students of gender will probably argue with Wilson; the nonspecialist reader will find her engaging. The book is a cavalcade of Wilson's learned eclecticism and displays a formal resemblance to its subject. Reading this book is as much of an experience as wandering through the urban venues that it describes. In short, Wilson is provocative and fun to read. *The Sphinx in the City* is scholarship as spectacle and, to reverse Gertrude Stein's comment on Oakland, there is a there there.

Crime, Policing and Place: Essays in Environmental Criminology. Edited by David J. Evans, Nicholas R. Fyfe, and David T. Herbert. New York: Routledge, 1992. Pp. xii + 332. \$87.50.

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Crime, Policing and Place is the latest in a string of very useful edited books (e.g., *The Geography of Crime* [New York: Routledge, 1989]) and journal special issues in which David Herbert, David Evans, and associates have advanced the development of environmental criminology, a field that explicitly attempts to merge the social and spatial analysis of events into a broader understanding of fear, crime, and criminality. This particular volume develops the importance of place and policing in 16 essays by 22 different authors from Britain, Canada, Hungary, and the United States.

Although there is an emergent consensus among many social scientists on the desirability of studying the social and spatial dimensions of phenomena simultaneously, the book illustrates the difficulty of doing so. Only two of the essays manage to consider both dimensions empirically at the same time. John Lowman's study of the evolution of the character and location of street prostitution in Vancouver demonstrates a strong interplay between the places of prostitution and the social relations in those places at particular times. Lowman also shows how the history of social interactions concerning the street prostitution problem changed the geography of prostitution over time. Anthony Bottoms, Ann Claytor, and Paul Wiles examine the criminal careers of two adjacent housing estates in Sheffield, "Stonewall" and "Gardenia." They show that, al-

though the two started from essentially similar demographic and social profiles, differences in chance events, the application of housing authority rules, and physical geography reinforced ecological labels that made it hard to induce "respectable" families to move to Gardenia, which in turn reinforced both poor child socialization patterns and development of a mild criminal subculture within that housing estate. By the mid-1970s Gardenia had developed into both a high-crime and a high-offender area, while Stonewall had developed into a low-crime community. They then trace the social effects of further changes in housing policy and physical plan that resulted, by 1988, in an equalization of the two estates' victimization rates. Stonewall's had risen to become similar to Gardenia's, though Gardenia appeared to still house a higher concentration of offenders.

Three essays focus on development of theoretical structures for expanding the range of environmental criminology. Anthony Bottoms and Paul Wiles adapt Giddens's general *structuration* theory to the task of tracing the criminal careers of communities, then use their theoretical model to inform the research reported in the essay on Stonewall and Gardenia. David Evans examines the theoretical and heuristic utility of the doctrines of Left realism for further development in environmental criminology and finds that they support existing trends without providing much that is new. In an extension of prior work on crime displacement, Robert Barr and Ken Pease extend the important idea of *crime flux* as an alternative way of conceptualizing the volume and position of crime in time and space.

Essays on the development of a geographic information system (GIS) for a particular police department and the use of small area crime profiling techniques point to the need for additional research tools to explore the role of place in the patterning of crime. Michael Levi's essay on the policing of fraud points up a series of conceptual legal problems centered on the *place* of the crime that make dealing with transnational frauds particularly difficult both for researchers and for prosecutors. George Rengert provides a lucid short summary of the current state of the literature on the journey to crime.

A number of essays represent more straightforward studies in the geography of crime and provide interesting results: the residential distribution of convicted offenders in Budapest in the late 1970s looks quite similar to that of Sheffield in the same period. Perceived crime levels are positively associated with actual victimization levels in a number of communities in Wales. Women who are vulnerable to violence in private are also at high risk of violence in public space.

Four concluding essays dealing with the evolution of English crime-prevention policy, the evaluation of neighborhood-watch programs, and the implementation of mandatory police-community consultation programs are intrinsically interesting but difficult to connect with the socio-geographic dimension or with the book's other essays.

The usefulness of a synthesis of sociological and geographical perspec-

tives to development of a more general social science seems well developed by this book. I believe that most environmental criminologists, human ecologists, and social geographers will want to add it to their collections.

The Cycle of Juvenile Justice. By Thomas J. Bernard. New York: Oxford University Press, 1992. Pp. vii + 195. \$35.00.

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A great virtue of *The Cycle of Juvenile Justice* is its accessibility. Thomas J. Bernard's generalizations about "the cycle of juvenile justice" and other "lessons of history" provide a useful organizing device for his narrative about axial developments in U.S. juvenile justice, highlighting the interrelation of legal and social history. He concludes by formulating sensible and humane prescriptions for juvenile justice today. Detracting from these achievements, however, are problems of conceptualization and evidence that render the work as a whole disappointingly less enlightening than the sum of its better parts.

In Bernard's narrative, the primary motivation for the two axial reforms represented by the founding of the New York House of Refuge in 1825 and the Chicago Juvenile Court in 1899 was the dilemma of having to treat juvenile offenders harshly—by consigning them to adult institutions—or not at all. Both sets of reformers "sold" their ideas (precisely to whom, Bernard does not specify) by drawing on contemporary ideas of delinquency (delinquents as potential paupers in 1825 and as dependent or neglected youth in 1899). These ideas also appeased the economic interests of the rich and powerful by suggesting the moral and intellectual superiority of the reformers themselves and by posing "unfair comparisons" between idealistic assessments of their own good intentions and cynical assessments of actual past performance. The effects of both reforms were to expand state power.

Generalizing from these two cases, and reasoning deductively from Samuel Walker's "law of criminal justice thermodynamics" (that the certainty and severity of penalties vary inversely), Bernard discovers the "cycle of delinquent justice." "Many people" mistakenly believe delinquency rates to be higher than in the "good old days," blaming current justice policies rather than the chronicity of the problem. "Because only a limited number of policies are possible to begin with, the result is that the juvenile justice system tends to cycle back and forth between harshness and leniency" (p. 39).

Along with the conditions enabling reforms to sell, this cycle of juvenile justice constitutes for Bernard the "lessons of history." He applies these "lessons" to explain both the failure of "due process" reforms promulgated in a series of Supreme Court decisions from 1966 to 1975 and the

ascendancy of "get tough" policies (based on notions of delinquents as "hardened criminals") in their stead. Chapter 8, in which Bernard employs a format loosely modeled on the legal brief to compare and contrast these Supreme Court cases with each other and with previous axial cases, represents to my mind the best and most original chapter of the book.

Bernard's narrative, abstracted from familiar secondary sources, is of greater pedagogical than scholarly value. Bernard accomplishes the teacher's task enjoined by Max Weber, to confront students with "inconvenient facts." Moreover, Bernard's organization is clear, his presentation concise, and his prose style refreshingly unpretentious, making selected chapters especially appropriate for inclusion in undergraduate syllabi. However, Bernard violates Weber's injunction against the ready-made presentation of the value-laden answers impatiently craved by students. Although he clearly labels as personal opinion his prescriptions for juvenile justice—and even explains why he does not expect them to sell—Bernard offers these prescriptions in a chapter misleadingly entitled "The Lessons of History." Without using the term, he argues in effect for the restoration of the rehabilitative ideal, based on "coerced treatment" and the right to treatment as an alternative to "get-tough" policies. Whatever the wisdom of Bernard's prescriptions, they do not follow from his historical narrative.

Bernard's analysis of the present state of juvenile justice rests in part on poorly reasoned, poorly documented, or empirically questionable claims. The vast preponderance of "not innocent" findings does not necessarily indicate a failure of due process. That relatively few juvenile arrests eventuate in probation supervision, institutionalization, or trial as an adult—or that delinquency rates peak among 16-year-olds—does not indicate that the juvenile court (p. 177) "ought to be considered an outstanding success." Bernard neglects to clarify the distinction between coerced treatment and punishment. At any rate, it is unfair to assert, on the basis of reference to a scant pair of essays in an edited anthology—but against the preponderance of empirical evidence—that "recidivism among the most violent delinquents can be reduced up to 70% in small, secure, treatment-oriented juvenile facilities" (p. 163).

Even Bernard's historical narrative is problematic, since in seeking generalization he overlooks the complexity, contingency, and human agency of historical and cultural process. The history of juvenile justice charts linear and spiral patterns as well as cyclical ones. Not only demographic and ecological transformations, but the gradual extension of formal citizenship rights and welfare entitlements, changing relations of federalism, new technologies of control, and indeed even reflexive awareness of previous reforms are just some of the factors that disturb cyclical patterns in ways Bernard overlooks. History does not yield its "lessons" as readily, nor are historical cycles as inevitably self-sustaining, nor do "ideas drive the cycle" as hermetically as Bernard suggests.

The Gang as an American Enterprise. By Felix M. Padilla. New Brunswick, N.J.: Rutgers University Press, 1992. Pp. viii + 198. \$40.00 (cloth); \$14.00 (paper).

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New books on urban gangs are an expected response to the dramatic proliferation of gangs over the past decade. These new works range in breadth from Huff's collection of new papers covering a broad range of topics to descriptions of the gang situation in several cities to descriptions of gangs in one area to case studies of particular gangs. Each approach contributes to knowledge in different ways, and together they emphasize our need to develop a generic sense of urban gangs. Felix M. Padilla's study of one small Puerto Rican drug-dealing gang in Chicago falls clearly within the single case study category but contributes to the generic issue by providing enough data on a drug gang to help us distinguish drug gangs from the larger category of street gangs.

Case studies of this sort carry an extra burden if they are to contribute to generalizations. My approach to reading Padilla's work was to establish some questions to which I hoped he would respond: Does he establish the parameters of the drug gang? Does he place this gang within an array of drug gangs, using the same parameters? Does he place this gang within the larger population of street gangs? Does he deal with the selection problem, that is, how some neighborhood youth come to be involved and others do not?

Other questions—adequacy of methods, use of scholarly materials, clarity of writing, relevance of data—are also appropriate, as they are to all empirical works. Padilla's methodology is familiar but narrow, being limited principally to a cautious form of nonparticipant relationships and biographical interviews of the gang members. Street observations were limited, standardized questionnaires eschewed, and use of official records or practitioners' experiences with the gang members discarded as invalid. More methodological triangulation, especially in view of the "hands-off" ethnography, would have broadened our understanding of this gang's situation.

On a more positive note, the book is easy to read, with gang member quotes judiciously used to illustrate rather than to simultaneously derive from and prove the author's points. Reference to other scholarly work on gangs, while not extensive, is appropriately employed, especially in regard to more recent works.

But what of my case study questions? In an era when policymakers and justice-system officials are touting a close relationship between street gangs and drug dealing, those with pertinent observations have much to contribute, if they would. Drug gangs can evolve *de novo*, they can develop in response to franchising efforts by drug dealers, or they can evolve and split off from larger street-gang organizations. Padilla's gang,

the "Diamonds," is of this third variety. Thus their description is particularly pertinent to distinguishing drug gangs from street gangs. Although the author never mentions the size of his group, I count 24 named members appearing in the manuscript. Most active dealing members are said to be 15–18 years old with associated ex-members and distributors mostly in their twenties. All are male—the author chose not to study the female companions—and the focus is heavily on their roles in dealing marijuana and cocaine.

Padilla fails to compare his group to other drug-dealing groups in the general area—there are reportedly a dozen of them—and does not explicitly compare them to garden-variety street gangs. My impression, however, is that the Diamonds are smaller, younger, and less criminally versatile than traditional street gangs. Because of the "business" of drug dealing, I sense they are more cohesive, more hierarchically organized, and far more focused on the roles, skills, and attitudes required of successful drug dealing. I infer these differences from the data presented; the author does not attempt these comparisons.

The selection problem is more adequately handled by reference to labeling theory. Padilla's young men contribute a counter-culture that is reacting to early deviance labeling by teachers, police, and others not sympathetic to Puerto Rican culture. And while gang affiliation clearly provides them with the expected status and group esteem they seek, it also traps them into a set of blockages that the author describes as limiting severely their later adoption of satisfying adult roles. Drug-gang members share the fate of their street-gang counterparts.

The topical sequence of the book contributes to reader retention. The author moves from a general introduction to a description of the local Puerto Rican community with its cultural advantages and economic disadvantages. There is a description of joining the gang, followed by a description of the business side of the gang, which only benefits the midlevel distributors. As one dealer noted, "I'm only making enough to starve" (p. 151). The last chapters are the downside, what belonging to the gang does for its members in the long run. "It is quite likely that at the end of the journey they are even more disadvantaged than when they first began" (p. 182).

Blacks in the White Establishment? A Study of Race and Class in America. By Richard L. Zweigenhaft and G. William Domhoff. New Haven, Conn.: Yale University Press, 1991. Pp. ix + 198. \$27.50.

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University of Pennsylvania

Blacks in the White Establishment? A Study of Race and Class in America by Richard L. Zweigenhaft and G. William Domhoff is a valuable work. It fills an important gap in the race relations and social-

stratification literature by providing an illuminating account of the journey of young blacks from the impoverished ghettos to the elite halls of corporate America via the country's most prestigious preparatory schools and colleges. This is a qualitative study of the actual experiences of the early participants in A Better Chance (ABC), a government program that identified promising junior high and early high school youngsters in some of the nation's most problem-ridden inner cities and sent them to exclusive prep schools or public schools located in suburbs like Swarthmore, Princeton, and Stanford. Under close supervision in these settings, they were carefully monitored and taught. The premise of ABC was that, to enter and succeed in the upper-middle and upper classes, lower-class people need not only a vastly better education than their neighborhood schools provide but also a whole different style of being and the right connections.

The results of the program are presented in a way that allows us to appreciate the significance of class and cultural issues for the black middle class, that class is consequential, and that education, behavior, and propriety—not to mention self-esteem—can make an important difference in terms of life chances. With dreams of success, the students worked hard to divest themselves of their own cultural particularities while actively pursuing universalism. In the context, through their pursuits, the universalism became transformed into a peculiar particularism favoring the WASPish majority. But given the opportunity to acquire upper-middle-class social, cultural, and educational skills, the participants have been remarkably successful. To be sure, there were casualties, but the vast majority “not only endured a very difficult experience, they flourished.” Almost all those who graduated from preparatory school went on to college, many to Ivy League institutions, and a large percentage went on further to earn graduate degrees. Then they were hired by top corporations and became part of the elite network that sustains the management of those corporations.

The material for the book was gathered through in-depth interviews with the program's early participants, and the study's strength owes much to the authors' obvious skill and sensitivity in conducting the interviews and presenting them. They were able to elicit candid and often self-evaluating comments on difficult and painful issues and throughout the book maintain a deep sense of the subjects' humanity. The reader gains a real understanding of these young people and their experiences.

The tracing of the “ABCers” paths out of the ghetto and into the upper-middle class fills a gap in our understanding, and as such this book is an excellent resource for courses in race relations. It has important policy implications as well in that it shows that social intervention can be successful but that it must be begun early and with a certain commitment. In the early years of the program, candidates were carefully screened to identify those who were open to mainstream culture, and few of those who were chosen were unable to make the transition to their new environment. Later, funding cuts forced a more perfunctory screening

process, and the success rate fell. This corroborates my own findings that to be efficacious, intervention must occur before kids become academically turned off and seriously invested in the oppositional culture, which disparages education, the use of standard English, and even, at times, being civil to whites as "acting white."

An important point which the book touches on but does not fully address is that of co-optation. A cynic might say that much of the response to the riots of the sixties, including this program, was an attempt to buy off frustrated elements of the black community. At first glance, it would appear that the ABC program proves him wrong. Not only have many of these young people been absorbed by mainstream culture, but they have taken root in it in that, by building and accumulating capital, they have established opportunities for future generations. Yet some of them are now beginning to seriously question the true extent of their success. Very few seem destined to reach the very top, the inner circle of corporate power. There seems to be a consensus among the respondents that there is a ceiling above which they cannot rise and that often they are sidetracked into dead-end jobs before they even approach the ceiling. At the same time, despite the many close friendships, very few have intermarried with upper-class whites; in fact, a significant number (40%) have not married at all. This has led a number of the book's subjects to question whether they really belong in this world for which they were socialized and educated. Yet they no longer feel they belong in the world into which they were born, either. A fuller discussion of this tension and its implications would have been helpful.

The question mark in the book's title hints at the authors' conclusion: for blacks, even those who have been granted the upper-class privileges which the right schools and connections provide, race is still "the most salient part of their personal and social identity." Despite their qualifications, they are not yet full members of the white establishment.

Education and the Social Construction of "Race." By Peter Figueroa. New York: Routledge, 1991. Pp. xi + 216.

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Peter Figueroa argues in *Education and the Social Construction of "Race"* that race has no biological or scientific validity. Despite this lack of scientific validity, "races" are actually "constructed" through social interaction. As social constructions, they constitute "important structural features of a stratified (or divisive) social system, and are characterized by differential access to power" (p. 39). Thus, racism is not just the result of individual acts of prejudice but rather is embedded in, and legitimated through, processes of social relations, social interactions, and social interpretations. There is a circular and mutually reinforcing rela-

tionship between a (de facto) racist power structure and the evolution of racist frames of references.

Furthermore, Figueroa argues that since "race," racism, and ethnocentrism can be sustained and constructed through education, they can also be deconstructed. Schools can and should challenge racism and ethnocentrism. They should actively develop antiracist education and stress goals of equality, freedom, solidarity, pluralism, and openness (pp. 5-7).

In developing his thesis, Figueroa reviews research on how British schools educate students from the Caribbean and Asia. He maintains that, within this research, the larger educational and societal contexts are ignored, while the contexts that are examined are those of student homes and ethnic minority cultures. He argues that when the larger contexts are ignored, the implicit assumption is that all students are treated equally. Therefore, one can conclude that the differences in achievement must be due to individual or cultural/racial differences. This leads to analyses of educational achievement that tend to emphasize pathologies and failures. This, in turn, contributes to the construction of a context that influences inequality.

This work is worthwhile reading, particularly for researchers in the field of race and ethnicity. There are, for example, careful, thoughtful, and constructive critiques made of major reports on multicultural education in Britain and on recent sociological research into racial and ethnic relations. (Of particular interest is his critique of rational choice theory in chap. 1.) Also quite valuable is his discussion of the case studies he has conducted, for they illuminate how British schools reinforce racist assumptions and behavior. The case studies also illustrate how a more multicultural and antiracist education could promote constructive cross-cultural relations.

The book is also useful because it forces the reader to think more comparatively with regard to race, ethnicity, and education. In reading the book one thinks of many parallels concerning Latinos, African-Americans, and Asians in the United States. Also immediately evident are the similarities in the deficit approaches taken, in Britain and the United States, to explain "what might be wrong" with minority students' educational experiences.

For example, some approaches cite problems in motivation, cultural factors, migration shock, culture shock, degree of identification with the "host" community, parents' and pupils' attitudes toward education and problems of adjustment to British educational practices, differences between home and school (especially with respect to discipline), parents' educational background, pupils' early education, length of education in Britain, length of stay in Britain, supposed "language problems," and "linguistic deprivation," social disadvantages derived from class, the quality of accommodations, child-minding patterns, "female dominance," and other aspects of family life and family organization. Also familiar is the tendency to view and treat these factors as functioning independently and not interactively. (It should be noted that Figueroa

does not argue that issues such as motivation, self-esteem, and parental support should be eliminated, but rather that they should be seen within a wider context.)

The book also raises a central question for U.S. researchers. This is whether it is fair to compare African-American and Latino educational achievement with that of whites—given that these groups may each receive different treatment in the larger educational and social contexts. The book makes clear that, at the very least, it is important to examine the influence of these larger contexts when studying the education and achievement of minority students.

Commenting on research deficiencies in this regard, he notes a few areas that merit more research—and one is again reminded of the U.S. situation. He notes, for example, the lack of research on overt and covert racism and how this relates to educational performance, how policies influence the opportunity of minority children, and how minority student achievement is affected by school practices, teacher training, ethnocentric curriculums, teaching materials, assessment, and placement, and teachers' and white students' attitudes, stereotypes, expectations, and ignorance.

Although the book is at times cumbersome to read and needs greater explication, it is important and insightful and should be read by all researchers interested in issues affecting people of color in the United States.

Giving Up on School: Student Dropouts and Teacher Burnouts. By Margaret Diane LeCompte and Anthony Gary Dworkin. Newbury Park, Calif.: Corwin Press, 1991. Pp. xii + 300. \$42.00.

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The contemporary educational reform movement in the United States identifies the loss of academic talent—capable students and teachers—from American schools as a serious problem. The rhetoric of reform claims that the large number of school dropouts and teacher burnouts is evidence of the failure of the American educational system. Yet in the minds of many reformers, the “system” is only the sum of its parts—the individual students and teachers who carry out the business of education on a daily basis in American schools. In this view, the problem of dropouts and burnouts is defined as an individual phenomenon and the solution to this problem requires the manipulation of the skills, attitudes, and motivations of individual students and teachers. The system as an entity in and of itself is invisible, and its effects are not considered.

Margaret Diane LeCompte and Anthony Gary Dworkin challenge the view that student dropout and teacher burnout are solely the consequences of the characteristics of individual actors. They argue that,

among both students and teachers, giving up on school is rooted in a long process of alienation from school that stems from poor coupling between schools and society. Most of *Giving Up on School* is devoted to documenting the nature of this alienation process and its sources in society at large and the inner life of U.S. elementary and secondary schools.

LeCompte and Dworkin argue that teacher burnout—defined as alienation from teaching characterized by feelings of meaninglessness and powerlessness—and student dropout are manifestations of a process of alienation and strain that is common to both teachers and students. In their view, the inability of schools to adapt to a new socioeconomic order results in cultural lag, whereby schools and the values that support them do not keep pace with social changes such as deindustrialization, the declining value of educational credentials, increased child poverty, and new forms of communication and knowledge. This cultural lag—between societal change and institutional stasis—produces gaps between individuals' expectations (rooted in an idealized image of schooling and society) and their attainments (moderated by institutional stasis). Thus, neither students nor teachers are assured of successful adult lives. Recognition of this gap leads both students and teachers to give up on schools and drop out or burn out, respectively.

The book relies heavily on secondary data analysis and benefits especially from Dworkin's previous work on teacher burnout (e.g., *Teacher Burnout in the Public Schools: Structural Causes and Consequences for Children* [Albany: State University of New York Press, 1987]), which is the source of many of the key sociological ideas presented. There is not, however, as much substantiation for the student-dropout side of the argument. The distinctive feature of this work is the claim that the tuning out and dropping out of students is a manifestation of the same process of role-specific alienation that leads teachers to burn out and quit the teaching profession. While the thought is intriguing, I found it incomplete and somewhat forced. In discussing student dropout, the authors consider social and cultural conditions broadly affecting the lives of youth—that is, beyond the student role. However, they present the circumstances affecting teachers as confined more narrowly to the teacher role. Thus, improving the internal workings of schools might substantially reduce teacher burnout, but would be unlikely to have much of an impact on the poor articulation between schooling and the economy, and hence on student dropout. The need for different strategies to remedy student dropout and teacher burnout suggests a lack of parallelism between the two constructs, an issue the authors do not address.

If the central problem of this book is why students drop out and teachers burn out from school, the analysis is fairly persuasive. LeCompte and Dworkin muster an array of evidence regarding structural conditions that might produce feelings of meaninglessness and powerlessness on the part of students and teachers. Such feelings of alienation could easily result in students and teachers giving up on school.

On the other hand, if the central problem of the book is why *particular*

teachers and students give up on school while others do not, the book must be judged less successful. The mechanisms by which macroshifts in economic and social life affect individuals are vague, and the authors are only partly successful in accounting for why some subgroups of teachers and students are more alienated from school than others. In addition, the macrolevel political, economic, and social changes in U.S. society over the past half-century are presumed to affect all individuals, but the authors disregard the fact that not all teachers and students become alienated from school. If schooling is so boring and meaningless, why do not *all* students drop out, instead of a relatively small fraction?

Social Paralysis and Social Change: British Working-Class Education in the Nineteenth Century. By Neil J. Smelser. Berkeley and Los Angeles: University of California Press; New York: Russell Sage Foundation, 1991. Pp. xii + 511. \$35.00.

David Levine

Ontario Institute for Studies in Education

England was the first industrial nation, yet its systematic development of human capital lagged behind most other countries. Why was this? Neil J. Smelser suggests that we must consider the creation of a national system of education at the institutional level of state formation, so he draws our attention to the complex balance of forces contending for influence in a polity that was peculiarly susceptible to lobbying by diverse interest groups. At the heart of the retardation of English education was an "equilibrium-in-tension" or "balance of forces." Educational change, then, resulted from "cumulative imbalances" that created "feelings of dissatisfaction, relative deprivation, or threat [which] induce[d] actors to mobilize for political action on the basis of . . . [ideological] . . . definitions, and thus to engender group conflict." In this set of circumstances each new, incremental "resolution yields a new point of equilibrium-in-tension, which constitutes, in its turn, a new starting point for the next phase of change" (pp. 23–24). The very forwardness of English social development—its pluralism and its complex class structure—interacted with the "primordial imagery" of hierarchy, privilege, and regionalism to create a mode of halting progress that persisted for the first three-quarters of the 19th century. Thus, it was only in 1870 that the logjam was broken and state-sponsored, secular education was mandated by the parliament that had hitherto been paralyzed by denominational factions. But even this development was not conclusive since as late as 1891, compulsory elementary education (legislated in 1876 and made operative in 1882) was still based on the payment of fees so that absenteeism and truancy were rife, especially among the very poor who were reckoned to be the main objects of this missionary project. The demand that the poor pay for their education was directly related to

prescription—education was considered to be the “sacred function” of the parent (i.e., father) whose authority was a “moral force” so that interference with this relationship was most troublesome because it would undermine the “independence” of working-class families. This argument existed at the level of ideology—and at this level it was powerful for as long as there was no paradigm shift to replace it with a child-centered or human capital vision of schooling. Yet, one wonders about its resonance—Was it only paid lipservice or was it representative of strongly held beliefs? Smelser never asks these questions.

Some books are ahead of their time; this one appears after its time has passed. Not only is the author's bibliography out-of-date, but he also seems to be unaware of recent developments concerning schooling, popular culture, social discipline, and state formation. In addition, his vision of social structure and ideological crosscurrents is rather static and, again, outdated. Smelser is aware of the class-based character of Victorian education, but he never probes below the surface to illuminate its deeper mainsprings; similarly, he is alive to the “primordial imagery” of educational discourse, but he never deconstructs it. His repeated references to “public opinion” are in this regard most revealing because he refers to official promulgations of organized interest groups, which is a far cry from what Toqueville implied. In these three ways, then, *Social Paralysis and Social Change: British Working-Class Education in the Nineteenth Century* exemplifies the traits of an older school of educational history from above—the story of political mobilization and lobbying leading to institutional organization and reorganization. In effect, this is a narrow study of schooling, not a broad study of education. There is not a hint here that the “discourse on poverty” formulated by Malthus was able to create a framework in which the poor could be blamed for their poverty on account of their prolific powers. This was the parson's greatest achievement and his most lasting contribution to the debate on education that reverberated throughout these years; according to Gertrude Himmelfarb, Malthus “defined that problem, gave it a centrality it had not had before, made it dramatically, urgently, insistently problematic.” Victorian notions of education were founded on the basic premise that the value of schooling would be tested in its ability to create a working class that was morally educated—both words (moral *and* education) are revealing since what the educational promoters had in mind was a working class that not only learned its place but also learned to accept that place. Smelser touches on these issues, but he does not situate the question of moral education in the context of a pullulating proletariat. There is no trace here of the *Annalist* search for deep currents below the surface waves; there is not much evidence of agency on the part of those who were to be educated. Indeed, there is almost complete silence on working-class self-activity of the sort that David Vincent and John Burnett unearthed in their autobiographical researches while the opinions and experiences of plebeian informants before the parliamentary commissions are largely neglected. In fact, Smelser's dependence on the views

of school inspectors is rather like asking the police to comment on the social origins of crime.

This should have been a much better book. Indeed, many of its parts are better than the whole—I found the discussions of teachers and inspectors to be most valuable while Smelser's decision to look at Britain (i.e., Wales, Scotland, and Ireland) was refreshing and gives a valuable sense of perspective to his account of English schooling. The best part of Smelser's argument concerns the "gyroscopic" consequences that resulted from the English state's inability to extricate itself from its historic relationship with the Anglican Church. In the name of resisting secularization, the church establishment and its cadres continuously checked reforms in provided, working-class schooling; it also doggedly fought a rearguard action to protect its disintegrating monopoly powers and in so doing created extraordinary complications in the organization of universal, free, compulsory, and nonsectarian schools for the working class. Jealous of the Anglican's privileged position, religious denominations were in turn unwilling to compromise until there was evidence that the state church would be effectively disestablished in the realm of provided schools for the working classes. The intricate balance between these different religiopolitical groupings was refracted through the prism of institutionalized political life. *Social Paralysis and Social Change*, then, is the kind of institutional study that was the staple of educational history before E. P. Thompson demanded that we rescue the poor from the condescension of posterity. The voices of the poor are not only unheeded; they are also unheard in this account. These silences could have been probed and charted in order to complement the top-down accounts of administrators and bureaucrats. Without doing so, Smelser has given us a valuable account of only one-half of the story. This is doubly unfortunate: first, because there is much of value in this study; and second, because Smelser's talents are underemployed. In sum, then, I can only lend this work qualified support because, like the curate's egg, it is good in parts.

Vancouver's Chinatown: Racial Discourse in Canada, 1875–1980. By Kay J. Anderson. Montreal: McGill–Queen's University Press, 1991. Pp. 323. \$34.95.

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State University of New York

Kay Anderson's *Vancouver's Chinatown: Racial Discourse in Canada, 1875–1980* is not really about Chinatown, but rather about white Canadians' mentality about Chinatown. What white Canadians think of Chinatown, according to Anderson, reflects what they think of themselves as Europeans. She argues that Europeans, in order to maintain their dominance over Canadian society, need to see Chinatown as a place

with particular people, alien and distinct from themselves. They have structured a selected image of the Chinese to separate "them" from "us." Chinatown is partly a Western creation, and it says as much about the frame of mind of the West as it does about the ethnic attributes of the East.

Anderson joins Alexander Saxton, who wrote *The Indispensable Enemy: Labor and the Anti-Chinese Movement* (Los Angeles: University of California Press, 1971) and more recently *The Rise and Fall of the White Republic: Class Politics and Mass Culture in Nineteenth-Century America* (New York: Verso, 1990), in focusing attention on whites as discriminators. Both have adopted Antonio Gramsci's concept of "cultural hegemony" to their analyses. According to their formulation, either prejudice or rationalization of economic exploitation is an inadequate explanation for racism. To them, racial expression against the Chinese is a "collectively constructed" ideology, by and for the Europeans, in support of the regime that bestowed on them identity, status, power, and economic advancement. Here "cultural" and "political" dimensions form an organic whole—the Europeans not only have the power to discriminate; they are the ones who set the societal "values" to justify their actions.

Anderson and Saxton add to the understanding of Chinese experience in North America a vital perspective that is lacking in the current literature. Present research tends to focus on the Chinese and on the internal workings of their long-isolated and stereotyped communities. The unintended result is to compound the already distinctive image of these communities as peripheral to the development of the host society. This is a common problem confronted by all ethnic studies, which have long been marginalized within the mainstream social-science disciplines.

Gramsci's hegemonic approach gives an ideal theoretical framework to integrate the microlevel research on Chinatown with the macronational developments, and even developments on an international level. Anderson asserts that white Canadians' dominance is part of the process of Europeans' struggle to gain global hegemony.

The concept of cultural hegemony is a powerful analytical tool, but it requires a giant canvas to accommodate the myriad details needed to map out the concrete situation. For what it attempts to explain is not just the dominance of whites, but also the process in which a single ideology is formulated out of a host of competing positions within the white community.

Anderson attempts to demonstrate European dominance by studying the actions taken by the three different levels of the Canadian government—national, provincial, and local—in dealing with the Chinese. The governing sector plays the part of the executioner and legitimizer of the hegemony. While records of legislative decisions and parliamentary debates are revealing, they are secondary to the critical struggles among the owners of capital, labor unions, and politicians representing various regional interests within Canadian society.

For instance, on the issue of the exclusion of the Chinese, the capitalists who benefited from exploiting the Chinese labor were opposed to it, while the laborers who felt threatened by the competition of cheap labor were for it. After long deliberation, the Canadian government eventually placed restrictions on Chinese immigration. The important question to ask is, How was labor, the less powerful of the two sectors in the dominant society (in fact, only a small fraction of that society), able to make its interests triumph? We need to know how labor articulated its interests, how it formed alliances, forged compromises with other interest groups, took advantage of regional divisions, and manipulated the Canadian national psyche to the point of having its position prevail as the dominant ideology.

Anderson is not able to elucidate this type of ideological construct. Instead, she provides graphic details of white assaults against the Chinese through the 105-year span of Canadian history of discrimination. The cumulative impression is that the dominant European sector has more or less always been united, able to maintain control, and devise ideological justifications for its actions. The distinction between dominance and prejudice is no longer clear—an outcome exactly contrary to Anderson's intent.

The problem may lie in Anderson's interpretation of governmental power. This can be seen in the last paragraph of the book, in which she states: "it . . . suggests the need for today's policy-makers to rethink potentially divisive assumptions about 'differences' . . ." (p. 252) and later, "so policy-makers might take some direction from shifts that are possible at the street and personal levels" (p. 252). If the "policy-makers" can indeed lift themselves above the dominant ideological environment, acting and thinking independently, then it may be that the whole discussion on cultural hegemony is pointless.

Student Protests in Twentieth-Century China: The View from Shanghai. By Jeffrey N. Wasserstrom. Stanford, Calif.: Stanford University Press, 1991. Pp. xiv + 428. \$45.00.

Tony Tam
Harvard University

Jeffrey Wasserstrom's book is an interpretive study of student protests in 20th-century China. Specifically, the study consists of four case studies of protest movements during the republican era in Shanghai: the May 4th Movement of 1919, the May 30th Movement in 1925, the anti-Japanese agitation of 1931, and the Anti-Hunger, Anti-Civil War movement of 1947. In the epilogue, the book also considers the revival of the May 4th tradition in late 1986 that culminated in the bloody military crackdown of June 4, 1989.

Integrating the four cases of student movements into a single study for

the first time, the book provides a distinctive approach to an old subject. Although the May 4th Movement is often considered to include a comprehensive set of events that had singular importance in the transformation of modern Chinese political and cultural history, the present study is restricted to the popular anti-Japanese protests that gave rise to and shaped the course of subsequent youth movements. Given the massive primary and secondary sources required to accomplish the task, *Student Protests in Twentieth-Century China* offers a useful bibliographic essay that reports on the reliability and contents of various sources on the subject.

For the most part, the book is devoted to documenting an empirical puzzle. The empirical puzzle lies in the observation that Chinese rulers and foreign officials viewed and consistently reacted to student protests as extremely serious events. The power of student protests in China became clear when, despite their apparent absence of physical and economic threats, these protests were perceived by officials as great threats to the governance of a city or the state and responded to with great alarm.

The interpretation of the book is explicitly guided by a cultural perspective. The perspective diverges from two conventional approaches in the literature: an intellectual history approach that emphasizes the ideological root of cognitive consensus among students and the collective motivation for protests and a mobilization approach that stresses the manipulative role of political parties in generating or suppressing student movements. Without rejecting the contributions of these extant approaches, Wasserstrom adopts a political culture approach that places much causal significance in the symbolic performances of student protests.

The main thesis of this approach is that student protesters are elite actors engaging in political struggles, and the contest for power is essentially conducted on the stage of a political theater. The key symbolic dynamic in the struggle is grounded in a complex symbiotic relationship between the governing elites and the students. The very symbolic and organizational means from which the governing elites derive their legitimacy are often appropriated, subverted, and turned into weapons of assault on the establishment. Because of the efficacy and power of the theatrical acts of students in their political struggles, student protests became a truly powerful instrument of challenge to the governing elites.

For a sociologist, the book may be viewed as offering a theoretical interpretation of modern Chinese student movements in the midst of a persistent national crisis that was produced by the invasion of foreign powers. However, the study does not succeed in pooling evidence to adjudicate between alternative explanations for the guiding empirical puzzle. The style of reasoning in this book may be true to training in history and consistent with most interpretive works in anthropology, but it falls short of the requirement of empirical and theoretical rigor in sociology.

In fact, much of the book consists of assertive interpretations of various events from the author's perspective. Little effort is devoted to the construction of alternative interpretations or the formulation of implications from contending viewpoints so that different interpretations of a given event may be empirically tested. The reader may get the impression that the author often thinks his interpretations are unproblematic. One may wonder whether the author has wrongly assumed that, when the interpretive focus is on the symbolic meanings of different acts and events, one may take the participants and/or the researcher's subjective interpretations and insights for granted.

Notwithstanding its theoretical weakness, the theoretical perspective of the book is interesting. Protest movements indeed provide fertile ground for the study of how history, tradition, symbols, strategies, power, mobilization, and efficacy interact in transforming collective problems into collective possibilities, and collective actions into collective outcomes. The theoretical mechanism adumbrated in the context of the student protests in Shanghai is potentially applicable to other settings and should be testable with historical data. Future work that explicitly redresses the theoretical weakness of the current study will be a promising contribution to our understanding of the interplay between culture and strategic action.

Offspring of Empire: The Koch'ang Kims and the Colonial Origins of Korean Capitalism, 1876–1945. By Carter J. Eckert. Seattle: University of Washington Press, 1991. Pp. xv + 388. \$40.00.

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University of Iowa

The interplay of the state and the world system in development of Third World countries has received a great deal of attention from development specialists and political sociologists. "Dependent development," "export-oriented regimes," or "developmentalist state" are only a few of the terms used that suggest the importance of that dynamic. Its influence particularly characterizes South Korean development, often depicted as "Asia's next giant," or as one of the "small tigers" or "four gangs." Yet most previous studies of Korean development primarily focus on state policy and participation in the world economy since the 1960s. *Offspring of Empire* corrects such an ahistorical view, tracing the origins of Korean capitalism back to the Japanese colonial period with a persuasive case study of a representative Korean capitalist family, the Koch'ang Kims (Kim Söngsu and Yönsu). The book shows that the postwar interplay of the state and the world system was a pattern already developed during the colonial period, especially in the later years (1931–45).

Avoiding the chimerical debate about whether Japanese imperialism was exploitative, since such is "the nature of imperialism," Eckert dis-

putes the Korean nationalist scholarship that claims that "Korean modernization under colonial rule was no more than an empty word" (p. 5). Instead, he argues that, despite colonial status, industrial development did occur under the guidance of the colonial state, and by 1945 Korea became "an integral part of an imperial economic nexus that stretched from Japan across Korea to the Asian continent" (p. 67). Drawing on a wide variety of personal interviews and published and unpublished colonial materials including private company records and confidential correspondence (much of which had never before been disclosed), Eckert shows that, for both political and economic reasons, the Japanese authorities permitted and even promoted (especially after the 1931 invasion into Manchuria) the rise of a Korean capitalist class of which the Kim family is an example. The successful story of the Kim family illustrates a distinctive type of capitalist growth in colonial Korea that is in contrast to early capitalist growth in the West. This type of growth, which depends on the massive economic support of both the state and a more advanced capitalist system and the perpetuation of an authoritarian form of government, also characterizes patterns of postwar development.

The book is well written and its argument provocative. It provides new insights about the workings of Korean development under Japanese colonial administration, and requires rethinking the nature of "national capitalism." Although *Offspring of Empire* deals with a Korean capitalist family, the Kims' experience is consistently placed in much broader, macro, socioeconomic, and political contexts. In fact, the portrait spans a wide range of themes, both historical and sociological, including the origins of Korean capitalism and of the modern business elite, the changing nature of Japanese colonial policy, the role of the colonial state, state/business relations, the nature of labor control and conflict in Korean enterprises, and the issue of national capitalism and Korean collaboration. Also, Eckert's argument regarding the colonial legacy of Korean development (i.e., a strong state role, concentration of private economic power among a small number of large business groups or *chaebol*, and repressive labor policy) not only demonstrates the importance of historical perspective in studies of social change and development; it gainsays the prevailing ideology in Korea that the period of Japanese colonialism was entirely destructive and contributed nothing to subsequent events.

While this view of the colonial legacy is well taken, the historical continuity in Korean development should not be exaggerated. Still to be explained is how the historical legacy interacted with postwar changes such as division of the nation, land reforms of 1949–50, and the civil war of 1950–53 to produce patterns of development since the 1960s. Also, more attention to some sociological factors in the transition of land to commercial/industrial capital is needed. For instance, rural unrest in the 1920s and 1930s may have made land investment less attractive, inducing some landlords to convert to industrial capital. Finally, does this pro-Japanese capitalist case generalize to the whole bourgeois class, espe-

cially to smaller and/or nationalist entrepreneurs? Still debatable is how to define and understand "national capital" or "national capitalism."

Offspring of Empire essentially takes a courageous stand that will be controversial in Korea, but it is surely one of the best works on Korean development at present. It offers fascinating, enlightening reading not only to Korean specialists but to historical sociologists, political sociologists, and development specialists.

The Territorial Imperative: Pluralism, Corporatism, and Economic Crisis. By Jeffrey J. Anderson. New York: Cambridge University Press, 1992. Pp. xiv + 252. \$49.95.

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University of Bergen

The Territorial Imperative is a study of political responses to problems generated by declining regional economies. Four regions are examined: Northeast and West Midlands in Britain and Saarland and North Rhine-Westphalia in Germany. The time frame is the end of the 1920s to the mid-1980s. Focus is on the patterns and content of relationships among actors—public and private, within and between the national, regional, and subregional levels. Who becomes active? How do they combine into pluralist, corporative pluralist, and corporative patterns of interaction and to what effect? Key dimensions are whether the center formulates and implements policies in isolation or with the participation of affected interests in the regions and whether local actors behave in a cooperative or noncooperative way.

The principal theoretical aim is to identify the relative impact of economic and political factors, or the explanatory power of a problem logic and a political logic. A problem logic implies that the dictates of economic problems determine political responses. A political logic (subdivided into a partisan-electoral and a governmental logic) implies that responses are influenced by political and institutional characteristics. Similar economic problems evoke different responses in different political institutional contexts. The research design keeps regional problems constant by cross-national comparisons and political institutional context constant by intra-national comparisons.

The study combines a power-dependence perspective with its language of strategic actors, objectives, resources, resource dependencies, and coalitions, and a perspective where constitutional orders are viewed as institutions. The focus of the latter perspective is on how institutions connecting central and regional levels shape the territorial distribution of resources and capabilities, as well as interests, identities, values, incentives, and rules. Institutions are here seen as affecting both the alterna-

tives available and the frameworks for competition and cooperation among actors.

In general, Anderson finds empirical support for his theoretical ideas. The exception is that the model, due to "immutable structural factors" (p. 189), performs miserably in predicting the emergence of sectoral corporatism. The primary conclusion is that responses in crisis regions are best understood as "political logic interpreting problem logic" (p. 193). There are significant effects of a common problem logic. Regional economic decline, essentially, activates the same territorial clienteles, presents national and subnational actors with similar challenges, and elicits comparable interpretations and policy responses. Simultaneously, the constitutional order matters. For instance, federalism in Germany allows for dominance of state government at the regional level and permits political parties to play a key role. The presence of one resourceful actor who can legitimately act in the name of the region and a highly institutionalized, multilateral policy-making framework make it possible to concentrate on policy formation and implementation. In comparison, the institutional vacuum at the regional level in Britain necessitates focusing on political organization. A regional actor has to be manufactured and the relations between center and provinces constructed ad hoc. This gives central policymakers more flexibility. Mobilization from below and the development of region-wide cooperative strategies is difficult. Resources reside in local governments, and local organizations defend their identities and control of resources. Cooperative strategies demand that divisive issues must be avoided and thereby create powerful constraints on a partisan logic.

Regions that have achieved robust variants of corporatist pluralism were found to be more effective in confronting economic decline than regions where such structures were weak or absent. Yet nowhere has the regional economy rebounded completely. After first arguing that political institutions and processes are important, Anderson seems to conclude that the market is actually the overriding institution.

The Territorial Imperative is interesting reading. The book reminds me of the importance of the spatial dimension of political life and the need to analyze carefully the interaction between levels of government and their respective configurations of organized interests. There is, however, room for improvement. First, the data for the study could have been described and evaluated in more detail. Readers learn little about the number and nature of the interviews made, the access to different archives, and so forth. Second, the book aspires to refine our understanding of institutions and their impact on political phenomena, yet some important theoretical assumptions are not commented upon. For example, it is assumed that institutions affect actors' identities, resources, and incentives, even though they are not assumed to affect the logic of action. Political actors are assumed to be strategic and means-end oriented, never driven by norms of appropriateness. Third, since institutions matter, it is important to understand how they change. Anderson argues that there

is nothing immutable about institutions or existing rules and distributions of resources. There are periodic and occasionally dramatic changes that create new actors, new interests, and relationships. The somewhat weak (yet realistic) conclusion is that radical institutional changes are difficult to predict. Nevertheless, their impact is undeniable.

The Politics of Irish Social Policy, 1600–1990. By Frederick W. Powell. Lewiston, N.Y.: Edwin Mellen Press, 1992. Pp. xiv + 374. \$79.95.

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Rutgers University

Seeing the “contradictions of the welfare state” as posing a profound challenge to social policy analysts, Frederick Powell aims to redress what he regards as an imbalance in Irish social historiography by focusing on “the poor” as well as power hierarchies in the formation of social policy in Ireland, and in doing so, aims to “challenge traditional notions” that view this process “in terms of a progression from barbarism to enlightenment” (pp. x–xi). Using official government reports, Powell, in the first part of the book, describes the extent of poverty and the related problems of vagrancy, disease, and homelessness that characterized Ireland during colonial rule and the response of the British Poor Law with its important distinction between the deserving and undeserving poor. The remainder of the book, drawing primarily on secondary literature and with recourse to official reports and a smattering of labor/leftist newspapers and journals, describes the relatively ad hoc approach of independent Ireland to its economic and social problems and the way in which nationalist politics and cultural conservatism circumscribed the policy options available.

Taking such a broad and complex period in Irish history as his focus (three centuries of British subjugation followed by seven decades of independence), perhaps it is not surprising that the author has a tendency to resort to simplistic, causal attributions, culminating in the final chapter with dismissive, polemical remarks about present-day Irish society, all of which gives the reader little insight into the politics of Irish social policy. One of the greatest difficulties in reading this book is the absence of clear conceptualization. Who, or what, constitute the “ruling elite,” the “working class,” “the socially deprived underclass,” “Catholic power,” “secular moral responsibility,” and “the sexualisation of Irish society” is not immediately evident and clearly takes on different meanings according to the time period and question under discussion. But despite his heavy reliance on such terminology, the author makes no attempt at explication. Thus, for example, one is left wondering whether “The emergence of social policy in early modern Ireland marked the advent of a secular moral responsibility” (p. 1) is equivalent to “development predicated on modernisation presaged a more secular society” (p. 275).

In discussing the power relations that influenced Irish social policy, Powell tends to frame the issue in terms of a contest between a reified State, Catholic social teaching, and the poor and their representatives. Despite the historical breadth of his study, he does not discuss the structural causes underlying poverty and the ways in which colonial industrial policy maintained subsistence level agriculture for the majority of the Irish at a time when the rest of the Western world was industrializing. What Powell's framework obscures are the important qualitative differences between government under the British as opposed to an indigenous Irish state; the overlapping interests of the Catholic episcopal hierarchy with both the British and, subsequently, the Irish state; the historically grounded close relationship between the Catholic church in Ireland and the Irish people, and the integration of cultural and political identity with Catholicism. And, contrary to what Powell asserts (p. 271), the Catholic church was involved in providing a range of social services to the poor long before Vatican II and his equation of it with the emergence of liberation theology. Here, too, he erroneously labels an association of Irish Catholic educators/administrators as liberation theologians (p. 294).

Missing from Powell's discussion is an account of the role played by employer organizations in influencing social welfare policy, and he also ignores the fragmentation within the Trade Union/labor movement, in particular, the greater political power wielded by unions representing skilled workers in contrast to nonskilled ones, and the overall indifference of the Congress of Trade Unions to the plight of the unemployed and the poor in Irish society. The political influence exerted by the powerful farming associations also goes unnoticed by Powell. Their success in securing favorable taxation status for relatively affluent farmers and exchequer money for agriculture and rural development has largely been at the expense of urban welfare programs and has contributed to exacerbating the inequities in access to education and health care. To have included the farmers as a central part of the analysis would also have provided the author with a much needed bridge linking British colonial policy making with postindependence policy making. As tenant farmers became small landowning farmers and others consolidated their holdings, the farming "class," while always a stratified one, provided the personnel and ideology critical to the hegemony of a conservative order in independent Ireland. Failure to appreciate the complex interweaving of Irish economic, political, and cultural interests ultimately leads the author to interpret the economic and moral debates of the 1980s in terms of "New Right" ideology and to the equally simplistic conclusion that this was suddenly transformed in 1990 (with the election of Mary Robinson as president) into "a major reversal upon the recrudescence of traditionalism" (p. 328). Overall, unfortunately, this book is a most disappointing treatment of a very interesting topic.

Economic Rationalism in Canberra: A Nation-building State Changes Its Mind. By Michael Pusey. New York: Cambridge University Press, 1992. Pp. 310. \$59.50.

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University of Chicago

Much in this book is not clear to me, but one thing definitely is: the author is totally unsympathetic to recent political and economic developments in Australia, and he blames most of these developments on what he calls economic rationalism. His concluding sentence is about as gloomy a forecast as you can get (p. 242): "The next generation of Australians may yet be sentenced to stand with the Aborigines whose land was declared a *terra nullius* and to then say, with them, 'Poor Fellow My Country.'" So, if you are looking for a reasonably unbiased evaluation of recent Australian national policies, this is not the place to find it.

The blame for the sad state of affairs seems to have been due to the introduction of foreign ideas (p. 227): "in the late 1980s and with the benefit of a little hindsight, no one can doubt the tremendous success that the 'New Right' American and British policy organizations and think-tanks have had first in cloning themselves in Australia, and then in reorganizing the public policy agenda along Anglo-American 'free' market lines—continental European social democratic experience is excluded almost to the point of invisibility."

The author does not give us any hint that would help us understand why the Australian political and economic actors might have found such ideas attractive. He devotes less than a page to a description of the unfortunate performance of the economy under a conservative government for a decade prior to the Hawke rise to power in 1983 with its glowing record of disciplined wage restraint, budget surpluses, reduced rates of taxation, a decline in the rate of inflation, increased profits, higher investment, and near-elimination of the public debt. Yet in this idyllic situation, the current account deficit worsened, net external debt rose sharply, and "there has been a continuing fall in the real income of most Australians" (p. 33). It is not explained why, if the performance by the Hawke government was so exemplary, things seemed to turn out so badly.

A major focus of the book is the presentation of the results of interviews with 215 members of the Senior Executive Service of the national government. Material is presented on the origins and backgrounds, on political leanings, evaluations of the power of various organizations (unions, corporations), on job satisfactions and dissatisfactions, and on views of recent shifts in the general directions of national policies. It was concluded that, as of the 1980s, the members of the service were "way to the right of centre."

The author clearly decries the shift from what he called a social democratic flavor to national policies to a right-of-center flavor. He places

a considerable responsibility for the shift on the policy front to the changes in the political orientations of the members of the Senior Executive Service that occurred over the past two decades.

I quote at some length a quite remarkable explanation of why this shift occurred:

It is through the power of a particular university economics curriculum that the recent past has had the strongest hand in casting the nation's future. In the crucial post-war period, the Canberra bureaucracy was largely built on the reputation of a very small and enormously influential group of generously minded economists who served both Labor and Liberal governments . . . until the late 1970s. Their prestige raised the expectation that the second generation of young economists with an entirely different life experience who now dominate the Canberra state apparatus would bring 'more of the same' attitudes and similar intellectual skills and norms to the policy and management process. [P. 5]

Pusey goes on to note that "the new generation national policy (and with it perhaps the fate of the nation) is held in the compass of the restrictive, technologically-oriented, neoclassical economics curriculum that swept through the economics departments of Australian universities from about 1947 onwards." This is a sweeping conclusion about the power of professors of economics that even I as a member of that group never imagined existed.

Besides blaming the economics curriculum of a time past, the author provides us with no explanation for what he believes has been a major (and undesirable) shift in the thrust of Australian economic and social policies. I find it hard to believe that there were not forces other than the change in the composition of the senior members of the bureaucracy that played a major role in the developments that he finds so threatening to the future of Australia.

The Accidental Proletariat: Workers, Politics and Crisis in Gorbachev's Russia. By Walter D. Connor. Princeton, N.J.: Princeton University Press, 1991. Pp. xv + 374. \$39.50.

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Are Soviet workers a class in the Marxian analytical sense? Relying on four criteria developed by Michael Mann (*Consciousness and Action among the Western Working Class* [London: MacMillan, 1973]) to analyze the proletariat of European capitalism, Walter D. Connor assesses the class formation of the Soviet proletariat. But as he acknowledges, this is a "less constant theme than it might be" (p. 13). Instead, his book is a broad and elegant synthesis of popular and scholarly publications

from the USSR and abroad about this proletariat that Connor calls "accidental."

It is accidental because the class developed differently over the Khrushchev-Brezhnev period from the way the political vanguard intended it to. Elites did not want to make a proletariat consciously opposed to the dominant system. Of course, in pre-Soviet Russia, the working class bore qualities of a revolutionary proletariat, but the October Revolution's culmination in Stalin's industrial revolution destroyed that old proletariat with its autonomous identity and enabled the authorities to make an industrial labor force without class in the 1930s.

From these beginnings, Connor presents the making of a new Soviet working class in extraordinary detail. He explains various policies on education, welfare, wages, and work organization from Khrushchev through Gorbachev. Drawing on previously published materials, he documents the effects of these policies on the stratification of Soviet society, with particular attention to effects on workers' lives and attitudes. He explains the power relations and politics of life in the enterprise. While he cites 106 strikes in the Soviet Union between 1956 and 1983, his principal concern is to explain their rarity before Gorbachev. After 1985, collective protest is not rare.

Long-time analysts of the Soviet scene such as Connor face a common problem. Accustomed to the challenges of studying a "stable" society with limited opportunities for independent research, they were obliged to divine from secondary sources the meaning of politically sensitive issues. This is an exemplary book in that tradition. But because Connor was putting final pen to paper in May 1991, three months before the failed coup d'état, he was faced with his own crisis: how to finish a book about a proletariat that was increasingly becoming organized, was militant but confused, and which could be studied on site. While he apparently did not go to the Donbass or Kuzbass areas when miners were on strike in 1989 or 1991, he has drawn on secondary materials to write a fitting concluding account focusing on this most militant sector of the accidental proletariat. While the drama of historical change prohibits any kind of finality to a narrative on the class created by Soviet industrialism, certain analytical conclusions can be reached.

Connor argues that before Gorbachev we could only understand the making of the new Soviet working class in terms of its "potential," and not its action. Oppositional class consciousness was contained by the state's prohibition on any self-organization, and by workers' dependency through employment security and other ingredients of the workers' "social contract" with the state. This social control allowed two basic forms of resistance to emerge: a relatively atomized veto power like absenteeism on the shopfloor and more collective manifestations of "populist legitimism," wherein organized appeals to the center for redress of local injustice were made. But these were not autonomous actions seeking a systemic alternative. It was not class action in the full sense or even in comparison with the strikes of 1989–91.

Connor finds in the 1991 strikes the culmination of this new working-class identity. Here strikers finally acquired the awareness of a systemic alternative to the Soviet order. But what is that alternative? In the Soviet Union's condition of economic crisis and political confusion in May 1991, Connor makes a crucial point: "*being* a class cannot determine how to *act* as a class" (p. 319). I agree. But where, then, does that leave a study of the proletariat, accidental or otherwise?

I found Connor's book to be profoundly ironic. Connor acknowledges his own "unabashedly pro-market, capitalist and democratic" sympathies (p. 13), but the coherence of the work depends on a deep Marxist assumption: the formation of the proletariat shapes history. Connor often reminds us that Marx's predictions about the proletariat have failed, but his work requires old-time Marxism's linking of class formation with the making of history. Why else would we study this class in and of itself, without systematic comparison to other classes or without systematic comparison of fractions within it?

Marxism links deep structure with historical change better than other theoretical traditions and that may be why Marxist themes can appeal to avowed non-Marxists like Connor. But Marxism faces a more profound crisis today than when "the crisis of Marxism" was a common title for articles over 20 years ago. Marxism depends on a vision of systemic determinations and alternatives, and this is a vision that Connor shares with most Marxists. The transformations of Soviet-type societies and their attendant consciousnesses suggest, however, that "fundamental" alternatives may be a legacy of the old Cold War and old social science and that conjunctures and multiple structures shape history more than the evolution of classes.

Part of the appeal of class analysis is the hint that it does more than say who gets what and why and that it also is the key to the question "So what?" The tragedy of the accidental proletariat may be that it shows us that without the moral persuasion of socialism, the working class loses its power to shape history. The proletariat that Soviet industrialism made does not know, cannot know, what it wants. But on the other hand, the world remains full of convictions about what is right, and these certainties are constructed in much more complicated ways than those formed in class relations alone. Most Marxists acknowledge this, and Connor does too.

Connor has written two books in one: the original book he set out to write to describe the evolving capacity of a category called the proletariat and the new book forced on him by history, which finds it is not the proletariat but various associations of workers in different enterprises that are making history even if it is not exactly as they please. The latter demands systematic comparisons so that the multiple determinations of different kinds of "class action" can be laid bare. Among those determinations is the information divined in Connor's original book. For those many dissertations and publications that will appear about local actions of Soviet workers in the unmaking of Communist power, Connor's book

will be foundational, if only for being the last of its kind that can talk about the Soviet proletariat in general. Local power relations, more than systemic class relations, are the source for finding the logic in accidents, including the one I find most profound. It is not just the proletariat of Soviet-type societies, but also the peasantry and intelligentsia who are destroying the foundations for their class existence. Why is this so?

What Is to Be Done? Proposals for the Soviet Transition to the Market. Edited by Merton J. Peck and Thomas J. Richardson. New Haven, Conn.: Yale University Press, 1992. Pp. 220. \$28.00 (cloth); \$12.00 (paper).

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Wellesley College

The essays in *What Is to Be Done?* are the result of a conference of Western and Soviet economists convened in the waning days of the Soviet Union to address the worsening economic crisis in the country and to develop policies for the resolution of this crisis. The policy recommendations in the volume were designed for the economic reform of the Soviet Union. The dissolution of the Union might lead one to think that such recommendations are obsolete. Yet the breakup of the Union has accelerated the process of economic reform. In this respect, this volume is still highly relevant for understanding the process of market reform in the post-Soviet social context.

The second chapter describes the economic crisis of the Soviet Union and presents five measures that should be taken to resolve it: the liberalization of prices, the corporatization of enterprises, the stabilization of government spending and restrictions on credit, the moderation of the social costs of unemployment, and the opening of the economy to internal and international competition. These issues are the topics of the volume's subsequent essays. According to the authors of chapter 2, the solution to the economic crisis entails adopting these measures swiftly and decisively, without compromise. For the authors there is no middle ground: "the solution [to the economic crisis] lies in abandoning the search for halfway houses, in abandoning the dream of a regulated market economy" (p. 20). Uncompromising attitudes such as this have led some critics to refer to proposals such as those offered in this volume as capitalist "shock therapy."

The essays in this volume are grounded in the ideology of the supremacy of the market. In the introduction, editor Merton J. Peck cites (p. 7) the Shatalin report, the so-called Five-Hundred-Day Plan, as the ideological inspiration for the structural transformation of Soviet society: "Mankind has not created anything more efficient than the market system. It gives strong incentives to materialize man's abilities, to activate labor and business and to expedite greatly the progress of science and technol-

ogy. . . ." There is little question that Western economists believe this to be the case, and no doubt a number of sociologists would heartily concur. Yet upon reading the volume, many sociologists will find themselves asking a different set of less policy oriented, but nonetheless significant sociological questions. Let us presume for a moment that many of the plans put forth in the volume have now been implemented. What comes next?

The authors of this volume present grand plans for economic reform with little discussion of the social consequences of such reform. In one chapter, Dutch economist Wil Albeda addresses the social costs of unemployment and how these can be moderated. Albeda notes that unemployment is a "necessary cost" in a market economy and notes that "the social consequences of frictional unemployment are tolerable and the policy measures required [to alleviate unemployment] relatively simple." One might ask, For whom are they tolerable? Certainly they are tolerable for Albeda who does not have to live with the consequences of such "tolerable necessities." This attitude is reflected in all of the essays and is the most troubling aspect of this volume. As advocates of economic rationality, the authors are unable to offer any concrete appraisal of how such tolerable costs are likely to affect the people who will be the "losers" in the shock-therapy game. From the start of *perestroika*, it was clear that a major problem involved in the transition to the market economy would be rising unemployment. Yet seldom did economists ask themselves how a population of workers who were quite unaccustomed to unemployment would deal with the prospect of losing their jobs.

Another problem that has accelerated since the dissolution of the Soviet Union is inflation. Since price controls in Russia were lifted in January 1992, the inflation rate has been about 14% per month. As with unemployment, economists presumed that inflation was another "necessary" consequence of capitalist economic reform. Yet an important sociological question, unasked by economists, is, How is this necessity met by the population and with what consequence for the future of post-Soviet society? One can only wonder how long it is before the tolerable necessity of inflation produces social outcomes that might undermine the successful transition to a market economy or a democratic society. Spiraling inflation has already generated a great deal of support for nationalist movements in the republics, movements that promise to replace anarchy with order, economic uncertainty with certainty, and so forth. Perhaps this volume is not the place for such discussions. Yet the policies outlined in the volume are implicated in shaping the fate of the republics of the former Soviet Union and, as such, ought to be interrogated by sociologists and economists alike.

If it is the task of Western economists to devise and implement economic shock therapies for post-Communist societies, it is the task of sociologists to explore and understand the effects and outcomes of the application of such therapies. The proposals outlined in this volume presume that a successful transition to a market economy will occur simply

by implementing macrolevel changes in economic structure. Yet how are these macrolevel changes likely to intersect with the microlevel social and cultural processes already in place? These questions are seldom addressed in policy-oriented volumes of this sort. Yet, current theoretical debates in sociology, in particular theoretical debates about the micro-macro link, are highly relevant to understanding the prospects for the Soviet transition to a market economy. Economists generally assume a rational choice model in their writings: give the people a supply of goods and services, and they will rationally choose from among a variety of means to achieve these ends. But is this the case in this social context? What about the influence of the cultural traditions of the Soviet welfare state with its ethos of patrimonialism, social leveling, and collectivism? It is rather enticing to think that the "micro-macro debate," usually thought of as one of the most metatheoretical areas of sociological theory, might play an extremely important role in understanding the future of post-Communist societies. Indeed, the success of economic reform in this context depends a great deal on understanding the interrelations between macrostructural changes and the microprocesses of everyday life in the post-Soviet social context. While this is a task for future sociological work, the essays in this volume are an integral part of the process of economic reform and ought to be understood by anyone interested in the fate of post-Soviet society.

Professions and the State: Expertise and Autonomy in the Soviet Union and Eastern Europe. Edited by Anthony Jones. Philadelphia: Temple University Press, 1991. Pp. x + 256.

Victor V. Golubchikov
Yale University

In August 1991 eight white males with unremarkable faces took over the top office of the Soviet Union, claiming that the only way to save the country from the irreversible slide into economic and social chaos was to revive the genuine values of "socialism." A few days later the men were arrested and the Communist party was outlawed. The same year *Professions and the State* came out in print.

The articles in this publication describe and discuss the existence of doctors, lawyers, engineers, teachers, university professors, sociologists, and journalists in a state dominated by Communist ideology. Is this essay collection of anything other than historical interest today? The answer is yes, and there are three substantiating reasons for such an answer: continuity, continuity, and continuity.

The volume's contributors attempt to place the observed processes within professional communities into a broader sociohistorical context, in some instances reaching as far back as the pre-Soviet period in Russian history (articles by E. Krause, M. Field, E. Gloeckner, and A. Jones and

E. Krause). The state, although only one of many factors relevant to the status of professions, undoubtedly leaves a permanent imprint on the society. The state has lost its omnipresent significance in Russia and Eastern Europe, but to overlook the continuity means to distort the picture.

In spite of the fact that "in the sociology of professions there is no consensus on what constitutes the core of professional group power" (p. 234) (as well as, one might add, on what constitutes it in a set of related issues) the book represents a consonant attempt to apply the Western tradition of reasoning about professions to a non-Western society like Russia. The Western tradition is based by and large on an *ex post facto* reflection on the actual historical development of occupations in the United States and Western Europe. The forms of professionalism, observed in the West, are nothing but specific outcomes of collision between practitioners and broader economic, social, and ideological arrangements in their historical development. The most common indicator of Western-type professionalism—power and autonomy, embodied usually in professional associations—is just another of those specific outcomes.

Scholars of professionalism face the following paradox. Russian and Eastern European practitioners often share with their Western counterparts the functional component of a profession (i.e., the body of skills and knowledge), while working under utterly different political and societal conditions. As a result, professions as macrosocial phenomena could not be found in Soviet-type societies. In the absence or token character of professional associations, power, and autonomy, what does constitute a profession there?

Each of the authors of the volume has had to find a solution to this methodological problem. The closing chapter by A. Jones and E. Krause (pp. 233–53) mentions some of the promising efforts on this path.

My own list of what is relevant to the Soviet-type society professionalism would highlight the ideas of microposition, intelligentsia, and counterculture—all of which notions are touched upon with varying degrees of thoroughness in *Professions and the State*.

Microposition refers to the set of conditions and arrangements within the immediate work setting of a practitioner. For example, physicians in Czechoslovakia, while lacking the guild power of their American counterparts, "have been left with substantial choice and work autonomy to decide how to handle their patients and how to practice their particular medical specialties" (p. 208). The coupling of "powerless profession" and "powerful professionals," noted by M. Field, L. Shelley, and A. Heitlinger, is fostered by the broader tendency toward "privatization of the society" (see Vladimir Shlapentokh, *Public and Private Life of the Soviet People: Changing Values in Post-Stalin Russia* [New York: Oxford University Press, 1989]).

The term intelligentsia connotes a social stratum in Russia and Eastern Europe that embraces practitioners known in the West as professionals (engineers, lawyers, etc.). At the same time, intelligentsia is "a form of

social identity that does not exist in the West" (p. 237). It is characterized by a distinct ideology based on humane values and historical skepticism regarding, if not openly dissenting from, the self-serving state bureaucracy. M. Kennedy and K. Sadkowsky observantly discuss the juxtaposition of the broader mission of intelligentsia and less societally ambitious professional ethos (pp. 173–76). Obviously, the "dual consciousness" of an Eastern European professional shapes specific ways in which practitioners in that part of the world organize themselves and their relations with the environment.

The roots of the counter-cultural drive might be traced to its locus in the intelligentsia. It still deserves to be discussed separately as the single most important reason why practitioners to the east of the Oder River never really developed macrosocial forms that became a synonym to professionalism. The rejection of the Communist state establishment inevitably implied distrust toward stillborn corporatist associations that were set up and run under the close supervision of the state. Those professional corporations were in no sense a field for advancement of any outstanding cause but rather an arena for self-promotion of practitioners with a mediocre record of accomplishments, who lacked general human decency. The notion of professionalism as craftsmanship of high standards was alien to them. The discord between substantive professional aspirations and politicized organizational forms resulted in the unique (as well as understudied and underdiscussed) phenomenon of unofficial professional activity that brought about outstanding achievements in medicine, teaching, and engineering.

The scholars of professions in Soviet-type societies are confronted with a choice. Their first option is to continue measuring Russia with a Western ruler and therefore keep coming to the conclusion that it has no professions. The alternative is to follow the way of the Western tradition of professionalism studies; that is, to investigate the process in which specific social groups commanding specific bodies of knowledge and skills interact with specific economic, social, and political arrangements. *Professions and the State* demonstrates the results of both opportunities, which makes it even more engaging.

Labor Parties in Postindustrial Societies. Edited by Frances Fox Piven. New York: Oxford University Press, 1992. Pp. 290. \$39.95 (cloth); \$16.95 (paper).

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Oberlin College

Labor Parties in Postindustrial Societies is an edited collection of essays based on the papers presented at a conference in 1988 on the fate of labor parties in Europe and North America during the transition from industrial to postindustrial economies. The collection could not hope to

be exhaustive—there are no chapters on labor parties in Spain, Italy, or Greece, which leaves a big hole in Mediterranean socialism, and no explanation is given for ignoring European Communist parties, which are more Labor-based than either the French Socialist party or the U.S. Democratic party, both of which are included—but this remains a very fine collection of essays that coheres well, and adds to our understanding of the Left in important ways.

The central theme, laid out by Frances Fox Piven in an excellent introductory chapter, is the divergent fates of Labor parties and the inability of purely economic explanations to account for the different paths taken, and different degrees of electoral success achieved, by such parties. All the chapters play with this theme, and most are relatively successful in keeping the two balls of convergence and divergence in the air, acknowledging the pressures and constraints emanating from economic change while demonstrating the importance of policies, politics, and political structures.

Several of the chapters, Neil Bradford and Jane Jenson on the Canadian New Democratic Party (NDP), Frances Fox Piven on the American Democratic party, and George Ross on the French Socialist party, highlight the importance of party structure and organization and emphasize the difficulty which uneasy coalitions of party factions have in formulating coherent policy. Bradford and Jenson give a particularly vivid account of the way the regional, populist, and laborite wings of the NDP fractured in the face of free trade and constitutional issues in the 1980s.

Three central chapters, by Göran Therborn, Claus Offe, and Gösta Esping-Andersen, differ from the others in the collection in that they are not really about leftist parties, but rather about the role of welfare and labor market policies in structuring economic change. They all demonstrate the ways in which policies that are essentially social democratic can lead to a distinctive postindustrial profile. Thus Therborn argues that the dislocation associated with postindustrial transition was mitigated in Sweden by the expansion of public-sector service employment. Both Offe and Esping-Andersen emphasize the role of welfare policies in shaping the kinds of jobs (and the security associated with those jobs) being created. These three chapters are the best in the collection because of the sophistication with which they trace the interaction between political and economic factors. Furthermore, as Esping-Andersen points out, the form that the postindustrial transition itself takes has implications for political conflict. He illustrates this in his discussion of the centrality of an emerging gender-based public and private sectors cleavage to the future of the Swedish Social Democrats. In a similar vein, Alan DiGaetano's chapter on Democratic party city politics in the United States is a marvelously subtle account of how progrowth coalitions displaced local party organizations in the 1950s and 1960s, thus shaping the form of urban development and the peculiar postindustrial profile of major American cities. This in turn has brought forth, in reaction, new "progressive-populist" Democratic coalitions in several cities.

The emphasis in this volume on the divergent fates of labor parties, and the admonition that "simple sociological reductionism does not work" (p. 21) in explaining those fates, is important, though perhaps not startling. There are differences between the authors on the degree of contingency in their explanations. Some chapters give most prominence to political structures—the state, party organization, welfare institutions—while the two chapters on the British Labour party, by Ivor Crewe and Joel Krieger, probably go furthest in blaming party behavior or strategy, rather than political or economic structures, for decline, and Piven argues in her introduction that "the great battle of postindustrial politics is being fought on the terrain of ideology, and it may be here that the future prospects of labor parties are decided" (p. 17).

Nevertheless, one can question just how divergent the fate of labor parties has been. Arguably, the distinctiveness of the ideologies and programs of labor parties is a better measure of their continued viability than simple electoral success. As Ross demonstrates, the French Socialist party's electoral dominance in the 1980s went hand in hand with policies which were practically indistinguishable from those of the Right and with an almost complete capitulation to market ideology. The collapse of Social Democratic party hegemony in Sweden in 1991 also removes the great exception to the rightward tide of the 1980s. In this respect the greatest contribution of this collection lies in the tentative and scattered indications that the essays provide a set of left-wing policies, centered upon training and an expanded conception of welfare, which are better suited to the emerging postindustrial economies than the widespread infatuation, of Right *and* Left, with the market.

Hidden Technocrats: The New Class and New Capitalism. Edited by Hansfried Kellner and Frank W. Heuberger. New Brunswick, N.J.: Transaction Publishers, 1992. Pp. xi + 246. \$29.95.

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University of Virginia

In the 1970s advocates of the new-class thesis argued that the shift toward a postindustrial economy spawned a new class of knowledge workers in competition with the capitalist class to secure power and status. Whereas the business community's interests are served through a competitive market economy, the new class benefits from a change in the locus of production to nonmarket spheres such as the government, public education, communication industries, and the public sector in general. Subsequent empirical tests primarily involved the analysis of survey data in an effort to determine the social-structural location of knowledge elites antagonistic to the traditional business class. The debate stalled, however, as the research produced mixed results, due in part to ambiguities surrounding the central concept.

Hansfried Kellner and Frank W. Heuberger's edited volume, *Hidden Technocrats*, attempts to renew the debate, but with a modified theoretical emphasis rooted in the Weberian tradition. Peter Berger argues that, while political and cultural conflicts persist between industrial and knowledge elites in advanced industrial capitalism, a "historic compromise" between these sectors is emerging that combines elements of bourgeois culture with values and behavioral patterns associated with the new class (p. ix). Several contributors to the volume delve into the thesis further and collectively propose the following: the postmodern world bears witness to the latest transformation of capitalism and the resilience of the system to coopt even its staunchest critics.

In the first article, Kellner and Berger characterize the knowledge class as "life-style engineers" attempting to diffuse functional rationality throughout the "life-world." In their view the educated elites of an expanded welfare state struggle to secure different client bases and assume responsibilities historically associated with the family, church, and community. But rather than fulminating against the excesses of modern capitalism, the new class legitimates the market by translating "quality of life" issues into highly profitable products and services. The authors conclude that the new-class culture has revitalized capitalism while concurrently transforming the business culture.

Heuberger echoes a similar theme in describing the new class as a cultural phenomenon wherein certain knowledgeable elites market the themes of fulfillment and the elimination of social injustice. While not quite prepared to proclaim a convergence, Heuberger speculates that the possible absorption of the "culture of critical discourse" by the market economy may serve an integrative function for knowledge elites and the business community. Indeed, James Hunter and Tracy Fessenden view the new class as a distinct branch of the capitalist class crucial to the evolution of American capitalism. The authors describe one such subgroup as "moral entrepreneurs" engaged in the production and distribution of new moral codes. Their analysis highlights the importance of applying business strategies to market these "moral products" and to reap the standard rewards of wealth, fame, and power.

Other papers stress the convergence in terms of a melding of elite symbols and sentiments, corporate culture, and popular consumerism. Kellner and Heuberger discuss such a convergence in their article concerning methods of business consulting in West Germany, as does Bernice Martin in a paper on qualitative market research in Britain. Kellner and Heuberger argue that, whereas classical consulting involved the rationalization of production processes and organizational structures, modern consultants more often deal with the corporate culture and help firms remain competitive by facilitating the development of a more cohesive enterprise. Martin discusses the importance of the new class of market researchers in promoting a consumer culture and influencing consumption patterns. Both papers stress the theme that postmodern experts promote new forms of rationality that penetrate every aspect of the life-

world, virtually eliminating the distinction between the public and private spheres.

These developments, however, have not proceeded at precisely the same pace across cultures. For example, Anton Bevers and Anton Zijderveld apply the thesis to explain the apparent convergence between cultural specialists in Holland's government and technocratic managers of the art world in the market sector, where a standardized professionalism has blurred the boundaries between economics, politics, and culture. In contrast, Paulo Jedlowski's research in southern Italy demonstrates that certain groups of professionals have influenced interaction modes in opposition to established, patronage-based ("clientelistic") public bureaucracies. Although groups such as advertisers and psychologists have guided the processes of rationalization and secularization, for Jedlowski these groups do not constitute a class. He acknowledges "a vertical separation between two *types* of middle classes, but not, however, between a *symbolic knowledge* class and a *business* class. Thanks to the area's weak industrialization, an autonomous business class never had the chance to develop" (p. 212).

The "new capitalism" thesis seems to be an apt description of what the authors have described almost unanimously in their cross-national research. The evidence and the arguments presented should revitalize a debate that has barely simmered in the past decade. The volume can be criticized, though, both in terms of a narrow Weberian interpretation of modern capitalism and through the selective use of empirical evidence. For example, the theory of a new capitalism as formulated takes for granted that the "immense productivity of industrial capitalism" (p. 12) has provided the necessary conditions—such as surplus wealth—for the emergence of the welfare state. The new class of cultural elites then encourages the competition for entitlements and benefits through an expanded state. Yet one could argue that the weakening of informal welfare systems (the family, church, etc.) may have been necessary for industrial capitalism to proceed. The new class and the state may be intervening in the private lives of the citizenry because the logic of *industrial* capitalist development has strangled the capacity of informal welfare mechanisms to perform.

As regards the methodology, the use of particular examples may be an effective device to demonstrate the viability of a thesis, but encourages the charge of selection bias. For instance, Hunter and Fessenden categorize several new-class, left-leaning organizations as dominated by highly educated, middle-class, suburban whites, but do not mention the fact that an enormous inventory of right-wing organizations (e.g., National Conservative Foundation, Liberty Lobby, Family Research Council, Boy Scouts of America) are dominated by members with similar backgrounds. Their thesis would be strengthened if they considered not only the "quasi-terrorist Animal Liberation Front" (p. 168), but the "quasi-terrorist Operation Rescue" as examples of moral entrepreneurs competing for resources in contemporary America. In either case, the eternal

sociological conundrum remains, Why do individuals of similar social locations, even members of the same family, construct such radically different life-worlds? While the new capitalism thesis has not answered that question, these articles should certainly stimulate a fresh round of debates about capitalism's future.

Post-Military Society: Militarism, Demilitarization and War at the End of the Twentieth Century. By Martin Shaw. Philadelphia: Temple University Press, 1991. Pp. vii + 229. \$44.95 (cloth); \$19.95 (paper).

Gregory McLauchlan
University of Oregon

This is the latest in a series of books written or edited by Martin Shaw that deal with sociological efforts to understand the relationships between war, militarism, and social change in modern society. The current book summarizes much of this earlier work and also engages in a lively dialogue with recent works by Giddens, Mann, Moscos, Mary Kaldor, Robin Luckham, and others who have sought to theorize the military dimensions of social power or the ways social forces affect military institutions. The result is a useful and accessible overview of much of the recent literature, as well as a provocative treatise suggesting the need for new conceptualizations if we are to understand military-society relations in the emerging post-Cold War world.

Shaw's central thesis is that in the second half of the 20th century we are seeing the emergence of a "post-military" society in the advanced capitalist states, a pattern that may, in time, be extended elsewhere. Postmilitary society is a nuanced concept—it does not indicate a society "beyond war" or one where military institutions are insignificant. Instead, it denotes tendencies in society where, for example, the mass military participation characteristic of the total wars of the first half of the century may be replaced by volunteer forces and more insular, technology-intensive military bureaucracies where martial values and militaristic ideologies will now compete with other ideologies such as consumerism to be the basis of social identity. In this kind of society, the primacy of military power in enforcing relations in the international state system may be eclipsed by economic and political institutions, such as the EEC.

While encouraging in many respects, these tendencies do not in themselves presage a pacific society and international system. Indeed, Shaw emphasizes that insular military bureaucracies possess weapons of unprecedented destructive capacity (nuclear and conventional), and the autonomy provided by technology and the nonreliance on conscript troops may even provide greater freedom to use military force, as in the Gulf War of 1991. Similarly, while masses are no longer called upon to participate directly in war making, there has emerged a potent "armament culture" in which citizens participate in distant wars through a

mass-mediated, high-tech militarism, what Mann has referred to as "spectator-sport militarism." But at the same time that images of weapons become pervasive—for example, in films such as "Star Wars," "Top Gun," and the like, or in children's television—Shaw argues that these "often become detached from a specific military context [leaving] armament culture open, not just to new military definitions, and to market forces, but also to anti-military interpretations" (p. 83). The strength of Shaw's analysis is his ability to identify what are often contradictory processes of militarism/demilitarization, thus specifying the complex nature of the growing gulf between classical modes of militarism and post-military society.

Shaw develops his arguments along three lines of comparison. He contrasts developments in the post-World War II era with the industrialized militarism of the 19th- and first half of the 20th-century, producing macroscopic, historical comparisons that give depth to his arguments. Second, he engages in a running dialogue with proponents of the "militarization" thesis, including E. P. Thompson and other peace movement theorists of the 1980s, who have argued the Cold War—especially the nuclear arms race—represented a new and deeper embedding of militarism in society. Shaw argues, for example, that, in the United States, even as the state mobilized the scientific assets necessary to modernize nuclear arsenals, aggregate levels of military spending did not dominate the economy and masses remained frozen in a kind of political stasis by the abstract and yet total threat of nuclear war. Finally, Shaw takes on proponents of the "obsolescence of war" thesis who have argued recently (and periodically since the days of classical sociology), that war and militarism are social institutions in decline. Here he locates the elements of truth in this thesis among other, contrary social processes, showing the question to be one of historical specificity that is far from having been settled definitively.

Technology, especially that of nuclear weapons, figures prominently in bringing about postmilitary society. For Shaw nuclear weapons created an almost unbridgeable gap between military force and geopolitical aims and rendered global, total wars unthinkable. Citizens could hardly rally around the threat of nuclear annihilation. But there is an element of technological determinism in Shaw's analysis, as he seems to consider the superpower's pursuit of a nuclear arms race and reliance on nuclear deterrence to have been, in the circumstances, inevitable. A related weakness is his failure to consider adequately the role of elite decision making and grand strategy in the analysis of changing modes of military-society relations. We are left with an account that, despite its historical grounding, is overly evolutionary and does not come to grips adequately with the role of agency. Yet it should be stressed that these are weaknesses of degree, rather than fatal flaws, in what is otherwise perhaps the best road map for contemporary debates on the changing nature of militarism, war, and peace.

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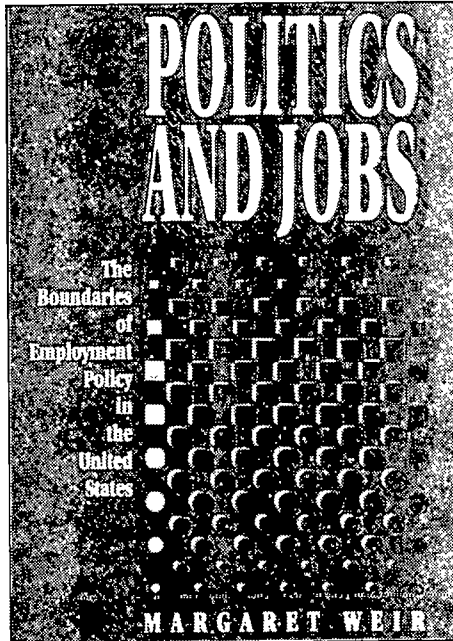
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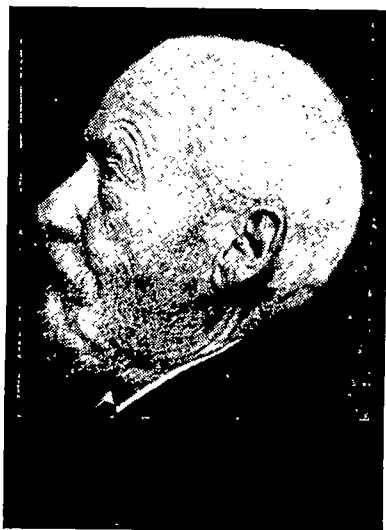
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Sommaire

- | | | |
|-----|--|--|
| 5 | Martine Bungener | Avant-Propos : Prise en charge des personnes âgées : les enjeux actuels |
| 9 | F. Cayla, C. Grouchka, A. Grand-Filaire, M. Duchange, J. Pous | Soins infirmiers à domicile aux personnes âgées : une approche comparative services de soins - infirmiers libéraux |
| 27 | Alain Berger, Michel Nègre | Comportement et prise en charge collective des populations âgées dépendantes |
| 45 | Jean-Claude Henrard | Soins et aides aux personnes âgées dans cinq pays de la C.E.E. |
| 63 | François Béland | Les hommes et les femmes âgés et leurs sources d'aide |
| 79 | D. Mayeux, A. Patris, P. Métais, J.-M. Dumay, C. Jeandel, F. Kohler | Le PMSI en gériatrie de court séjour : adaptation et adéquation |
| 111 | Résumés | <i>(anglais, espagnol)</i> |
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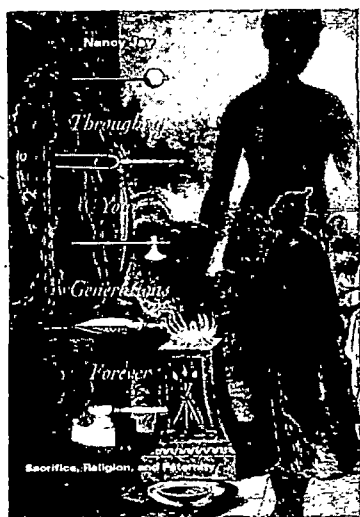
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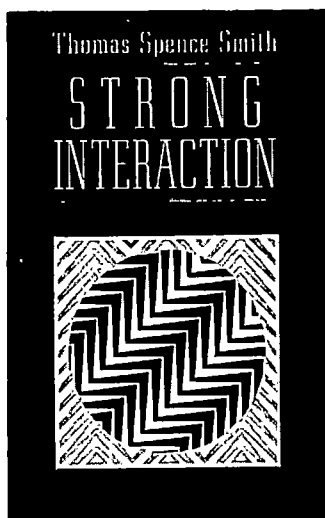
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Robust Action and the Rise of the Medici——Padgett and Ansell
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Link, Lennon, and Dohrenwend
Minority Proximity to Whites in Suburbs——Alba and Logan
Intergenerational Exchange in American Families——
Hogan, Eggebeen, and Clogg

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CONTENTS

- 1259 Robust Action and the Rise of the Medici, 1400–1434
JOHN F. PADGETT AND CHRISTOPHER K. ANSELL
- 1320 Embeddedness and Immigration: Notes on the Social Determinants of Economic Action
ALEJANDRO PORTES AND JULIA SENSENBRENNER
- 1351 Socioeconomic Status and Depression: The Role of Occupations Involving Direction, Control, and Planning
BRUCE G. LINK, MARY CLARE LENNON,
AND BRUCE P. DOHRENWEND
- 1388 Minority Proximity to Whites in Suburbs: An Individual-Level Analysis of Segregation
RICHARD D. ALBA AND JOHN R. LOGAN
- 1428 The Structure of Intergenerational Exchanges in American Families
DENNIS P. HOGAN, DAVID J. EGGEBEEN,
AND CLIFFORD C. CLOGG

Book Reviews

- 1459 *Is Anyone Responsible? How Television Frames Political Issues*
by Shanto Iyengar
RICHARD V. ERICSON
- 1462 *Make Room for Television: Television and the Family Ideal in Postwar America* by Lynn Spigel
CHRISTINE LAFIA
- 1464 *Shared Pleasures: A History of Movie Presentation in the United States* by Douglas Gomery
ERIC SCHAEFER

- 1466 *Salsiology: Afro-Cuban Music and the Evolution of Salsa in New York City*, edited by Vernon W. Boggs
PAUL D. LOPES
- 1468 *Men, Masculinity, and the Media*, edited by Steve Craig
LAUREN J. PIVNICK
- 1470 *Women's Two Roles: A Contemporary Dilemma* by Phyllis Moen
LINDA D. MOLM
- 1471 *Women, Family, and Utopia: Communal Experiments of the Shakers, the Oneida Community, and the Mormons* by Lawrence Foster
JOHN R. HALL
- 1473 *How Fascism Ruled Women: Italy, 1922-1945* by Victoria De Grazia
TED PERLMUTTER
- 1475 *Crafting Selves: Power, Gender, and Discourses of Identity in a Japanese Workplace* by Dorinne K. Kondo

Crested Kimono: Power and Love in the Japanese Business Family by Matthews Masayuki Hamabata
KEIKO IKEDA
- 1478 *Political Life in Japan: Democracy in a Reversible World* by Takako Kishima
WILLIAM W. KELLY
- 1480 *Rhetoric and Ritual in Colonial India: The Shaping of a Public Culture in Surat City, 1852-1928* by Douglas E. Haynes
JOSEPH W. ELDER
- 1482 *Thinking Through Cultures: Expeditions in Cultural Psychology* by Richard A. Shweder
STEVE DERNÉ
- 1483 *Science as Practice and Culture*, edited by Andrew Pickering
SUSAN A. FARRELL
- 1485 *Questions about Questions: Inquiries into the Cognitive Bases of Surveys*, edited by Judith M. Tanur
STANLEY PRESSER
- 1487 *The Professional Quest for Truth: A Social Theory of Science and Knowledge* by Stephan Fuchs
ANDREW PICKERING

- 1489 *Milestones and Millstones: Social Science at the National Science Foundation, 1945–1991* by Otto N. Larsen
STEPHEN COLE
- 1491 *Organizational Communication* by Peter K. Manning
PETER M. HALL AND RON BUSSELMAN
- 1493 *The New Institutionalism in Organizational Analysis*, edited by Walter W. Powell and Paul J. DiMaggio
DAVID H. KAMENS
- 1495 *Human Services as Complex Organizations*, edited by Yeheskel Hasenfeld
HARRY PERLSTADT
- 1497 *Choosing Crime: The Criminal Calculus of Property Offenders* by Kenneth D. Tunnell
MARCUS FELSON
- 1499 *The Rebirth of Private Policing* by Les Johnston
PAMELA IRVING JACKSON
- 1501 *Western Times and Water Wars: State, Culture, and Rebellion in California* by John Walton
STEPHEN P. MUMME
- 1503 *Community versus Commodity: Tenants and the American City* by Stella M. Čapek and John I. Gilderbloom
TERRY J. ROSENBERG
- 1505 *Toxic Work: Women Workers at GTE Lenkurt* by Steve Fox

Life and Death at Work: Industrial Accidents as a Case of Socially Produced Error by Tom Dwyer
MARC LENDLER
- 1508 *Work and Democracy in Socialist Cuba* by Linda Fuller
RICHARD A. DELLO BUONO
- 1510 *Managing Modern Capitalism: Industrial Renewal and Workplace Democracy in the United States and Western Europe*, edited by M. Donald Hancock, John Logue, and Bernt Schiller
MICHAEL RUSTIN
- 1512 *Emptying Their Nets: Small Capital and Rural Industrialization in the Nova Scotia Fishing Industry* by Richard Apostle and Gene Barrett
TERRY AMBURGEY

- 1513 *Rural Poverty in America*, edited by Cynthia M. Duncan
THOMAS A. HIRSCHL
- 1515 *Separate Societies: Poverty and Inequality in U.S. Cities* by William W. Goldsmith and Edward J. Blakely
KIRK E. HARRIS
- 1517 *Education and Society in Hong Kong: Toward One Country and Two Systems*, edited by Gerard A. Postiglione
STEPHEN LUNG WAI TANG
- 1519 *The Crisis of Socialism in Europe*, edited by Christiane Lemke and Gary Marks
PAUL HOLLANDER
- 1521 *Schumpeter: A Biography* by Richard Swedberg
ROBERT HOLTON
- 1523 *The End of History and the Last Man* by Francis Fukuyama
JOHN A. HALL
- 1527 Acknowledgments to Referees
- 1531 Contents of Volume 98
- 1548 Book Reviewers for Volume 98

IN THIS ISSUE

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(Rev. 1/93)

Robust Action and the Rise of the Medici, 1400–1434¹

John F. Padgett and Christopher K. Ansell
University of Chicago

We analyze the centralization of political parties and elite networks that underlay the birth of the Renaissance state in Florence. Class revolt and fiscal crisis were the ultimate causes of elite consolidation, but Medicean political control was produced by means of network disjunctures within the elite, which the Medici alone spanned. Cosimo de' Medici's multivocal identity as sphinx harnessed the power available in these network holes and resolved the contradiction between judge and boss inherent in all organizations. Methodologically, we argue that to understand state formation one must penetrate beneath the veneer of formal institutions, groups, and goals down to the relational substrata of peoples' actual lives. Ambiguity and heterogeneity, not planning and self-interest, are the raw materials of which powerful states and persons are constructed.

INTRODUCTION

Regardless of time or place, political centralization lies at the heart of state building. Less widely appreciated is the fact that the process of centralization is contradictory: its agents are forced to seek both reproduction and control. Centralization occurs, often abruptly, when founders emerge out of the soup of contending actors to establish (perhaps unintentionally) new rules for others' interaction. Reproduction ensues when rules induce roles, which induce interests, which induce strategic exchanges, which lock in patterns of collective action that depend on the

¹ Our colleague Paul McLean is a full joint participant in the larger project out of which this paper has been drawn. His help has been invaluable. We would also like to thank Wayne Baker, Ronald Breiger, Gene Brucker, Michael Cohen, Samuel Cohn, Walter Fontana, Mark Granovetter, the late David Greenstone, Wendy Griswold, the late David Herlihy, Alex Hicks, Ian Lustick, Charles Perrow, Tony Tam, Charles Tilly, and participants in the University of Chicago's Organizations and State-Building Workshop, the New School's "think and drink" seminar, and the Santa Fe Institute's Adaptive Organizations Conference for their many helpful comments. This article is dedicated to the memory of David Herlihy, whose quantitative research on Renaissance Florence made work like this possible.

rules.² Control is when others' locked-in interactions generate a flow of collective behavior that just happens to serve one's interests.

The contradiction, in state building or in any organization, is between judge and boss: founders cannot be both at once. Stable self-regulating maintenance of rules (i.e., legitimacy) hinges on contending actors' conviction that judges and rules are not motivated by self-interest (Elster 1983; Padgett 1986; Douglas 1986). At the same time, the nightmare of all founders is that their organizational creation will walk away from them. As Weber recognized long ago, in crisis (sooner or later inevitable), direct intervention in or overt domination of locked-in interactions is a sure sign of control's absence, not of its presence. Tactical tinkering to maintain fleeting control sucks in founders to locked-in role frames, thereby inducing attributions of self-interest and undermining their judicial perch above the fray.

This article analyzes one historical resolution of this state-building contradiction: the early 15th-century rise of Cosimo de' Medici in Renaissance Florence. We focus in particular on analyzing the structure and the sequential emergence of the marriage, economic, and patronage networks that constituted the Medicean political party, used by Cosimo in 1434 to take over the budding Florentine Renaissance state.

The historical case is exemplary in numerous ways. From a state centralization perspective, the period marks the abrupt transition from the late medieval pattern of fluid urban factionalism to the birth of a regionally consolidated Renaissance state (Baron 1966).³ Before the advent of the Medici, two centuries of late medieval Florentine politics could be characterized by a cyclic alternation between guild corporatism and warring urban feudal factions, as is implied by figure 1. Originally, the Medici partook of this ancient rhythm, which became puzzlingly muted thereby. After the rise of the Medici, the periodic explosion of the system, under the pressure of "new men" families surging from below, abruptly stopped, never to be renewed.⁴

² Feedback dependence is not necessarily of the form of everyone's obeying the rules. More common is when rules structure the patterned process of subverting themselves, thereby sustaining a mutual symbiosis between subversion and rules. See Padgett (1990) for an example in the domain of courts.

³ The oligarchic regime of Maso degli Albizzi and Niccolò da Uzzano (1382–1433) was significant in effecting this transition, as well as Cosimo's regime (1434–1464). The oligarchic regime spawned the formally democratic institutions that newly constituted "the consensual state" (Najemy 1982). Through political party networks, the Medicean regime learned how to use these institutions for purposes of control (Rubinstein 1966).

⁴ "New men" (*novi cives*) refers to families only "recently" admitted to legal participation in the state. See fig. 1.

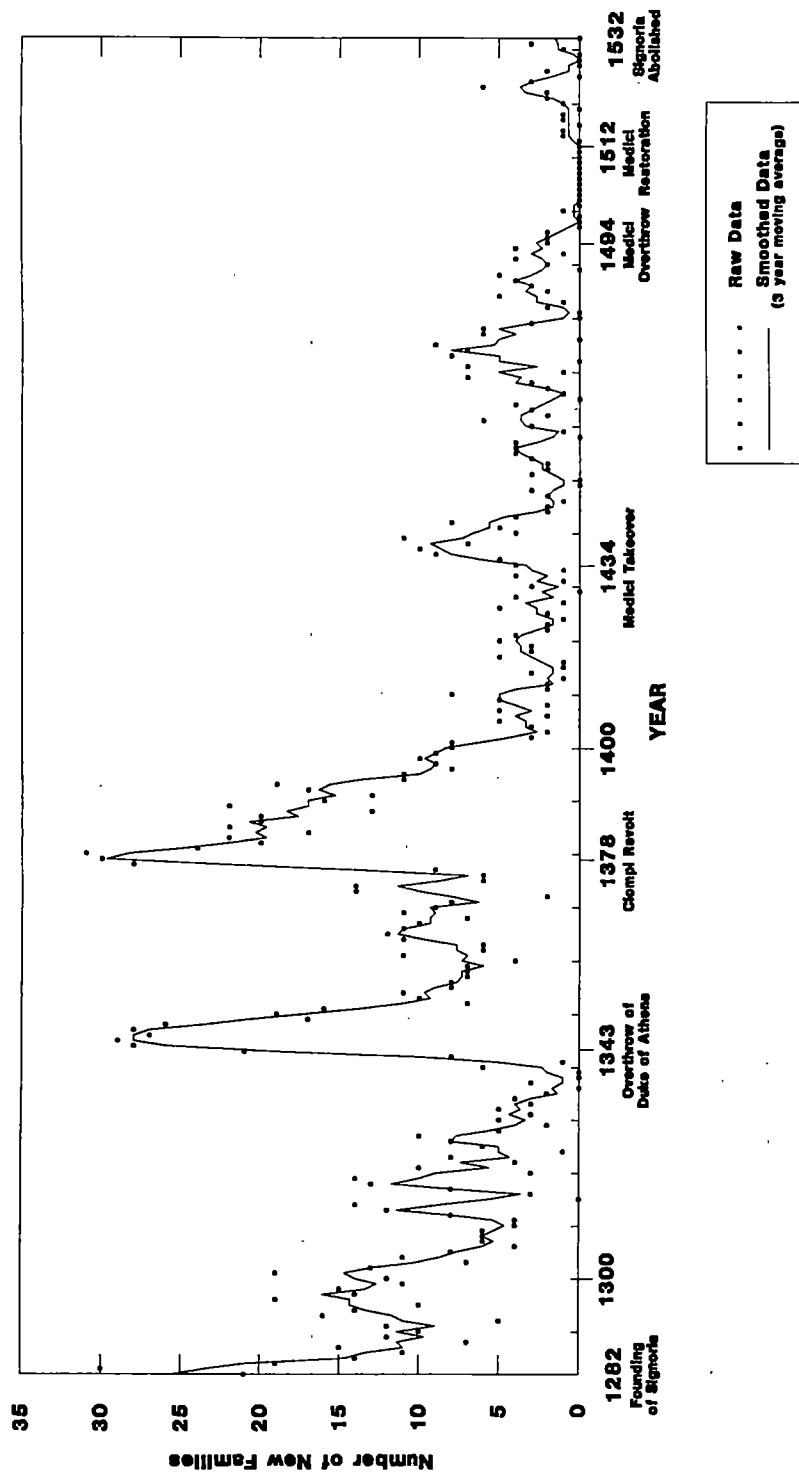


FIG. 1.—New families admitted to the Signoria, 1282–1532 (Source: Najemy 1982, pp. 320–22).

The dynamic underlying Florentine state centralization, we shall show, was this: unsuccessful class revolt (1378–82) and fiscal catastrophe due to wars (1424–33) were the ultimate causes, but these shocks were transmitted through the ratchet mechanism of elite network transformation. A citywide oligarchy, cemented through marriage, first emerged from a quasi-feudal federation of patrician neighborhood hierarchies. The very process of oligarchic consolidation, however, also produced the agent of its own destruction: the Medici party. The Medici party was a heterogeneous mixture of contradictory interests and crosscutting networks. In stark contrast to this fact, contemporaries perceived the Medici categorically as “heroes of the new men.” The Medici’s contradictory agglomeration exhibited great cohesion and capacity for sustained collective action. But what the Medici stood for is unclear to this day.

On the surface, it seems obvious that Cosimo de’ Medici (1389–1464) did it all. Cosimo de’ Medici was multiply embedded in complicated and sprawling Florentine marriage, economic, and patronage elite networks. And he was riding herd on vast macropolitical and macroeconomic forces far beyond his control. Yet he founded a dynasty that dominated Florence for three centuries. He consolidated a Europe-wide banking network that helped induce both international trade and state making elsewhere (de Roover 1966). And he oversaw and sponsored the Florentine intellectual and artistic efflorescence that we now call “the Renaissance.”

Contemporaries deeply appreciated Cosimo’s power. Foreign princes after 1434 flocked to Cosimo’s private palazzo to work out international relations, much to the consternation of bypassed Florentine officials. Cosimo was legally enshrined on his death as the father of his country—no mean recognition from citizens as cynical and suspicious as the Florentines. Machiavelli ([1525] 1988), almost a full century later, still held Cosimo and his family in awe—attributing both all good and all evil in recent Florentine history to Cosimo’s deep and ruthless machinations.⁵

Yet the puzzle about Cosimo’s control is this: totally contrary to Machiavelli’s portrait in *The Prince* of effective leaders as decisive and goal oriented, eyewitness accounts describe Cosimo de’ Medici as an indecipherable sphinx (Brown 1961, p. 186). “Cosimo was anxious to remain in the background, hiding his great influence, and acting, when need arose, through a deputy. As a result, very little is known of the measures for which he was directly responsible” (Gutkind 1938, p. 124). Despite almost complete domination of the state, Cosimo never assumed lasting

⁵ This is not entirely surprising, since Niccolo Machiavelli enjoyed the freedom to write in the first place because he had been outmaneuvered into exile by his boyhood friends, the Medici. Only their memory of his childhood saved Machiavelli from summary execution, a fact that probably focused his gaze.

public office.⁶ And he hardly ever gave a public speech.⁷ Lest one conclude that this implies only savvy back-room dealing, extant accounts of private meetings with Cosimo emphasize the same odd passivity.⁸ After passionate pleas by supplicants for action of some sort, Cosimo typically would terminate a meeting graciously but icily, with little more commitment than “Yes my son, I shall look into that” (cf. Vespasiano 1963, pp. 223, 226).

Moreover, especially after 1434, all action by Cosimo (never explained or rationalized) appeared extraordinarily reactive in character. Everything was done in response to a flow of requests that, somehow or other, “just so happened” to serve Cosimo’s extremely multiple interests.

We use the term “robust action” to refer to Cosimo’s style of control. The key to understanding Cosimo’s sphinxlike character, and the judge/boss contradiction thereby, we argue, is multivocality—the fact that single actions can be interpreted coherently from multiple perspectives simultaneously, the fact that single actions can be moves in many games at once, and the fact that public and private motivations cannot be parsed. Multivocal action leads to Rorschach blot identities, with all alters constructing their own distinctive attribution of the identity of ego. The “only” point of this, from the perspective of ego, is flexible opportunism—maintaining discretionary options across unforeseeable futures in the face of hostile attempts by others to narrow those options.

⁶ The only state offices Cosimo ever held were short-term: three two-month terms as Gonfalonier of Justice (Gutkind 1938, p. 123), a one-year stint as one of the *Ufficiali del Banco* in 1428 (Molho 1971, p. 218), and a few martial and Monte commissions.

⁷ We know of Cosimo’s reticence because Florentine verbatim documentation of *Consulte e Pratiche* (an informal “inner elite” advisory body) and other speeches is very extensive. Two rare exceptions were a 1446 debate about potential electoral reforms (Rubinstein 1966, p. 24) and a vigorous public debate with Neri Capponi in 1450 about whether Florence should realign from Venice to Milan (Gutkind 1938, p. 110).

⁸ Contemporaneous reports of Cosimo’s personal style are as follows: “He acted privately with the greatest discretion in order to safeguard himself, and whenever he sought to attain an object he contrived to let it appear that the matter had been set in motion by someone other than himself. . . . He replies were brief and sometimes obscure, so that they might be made to bear a double sense” (Vespasiano [ca. 1495] 1963, p. 223). “In 1432, just before his exile and triumphant return, a political opponent, Francesco Filefo, described in a letter how Cosimo, in contrast to his ‘open and lighthearted’ brother, Lorenzo, ‘is, I notice, despite appearing devoted to me, the kind of man who feigns and dissembles everything. He is so taciturn that he can scarcely be understood even by his intimates and servants in his family circle’” (Brown 1992, p. 106). “Said Neri di Gino [Capponi] to Cosimo: I would like for you to say things clearly to me, so that I can understand you. He replied: Learn my language!” (Poliziano [ca. 1478] 1985, p. 57). Cosimo’s speech, when it occurred, was often Delphic in form. “As Gutkind has suggested, in this situation Cosimo’s use of proverbs and fables served a useful political purpose—in delivering messages ‘in such a way that no one noticed,’ as Vespasiano put it” (Brown 1992, p. 106).

Crucial for maintaining discretion is *not* to pursue any specific goals. For in nasty strategic games, like Florence or like chess, positional play is the maneuvering of opponents into the forced clarification of their (but not your) tactical lines of action.⁹ Locked-in commitment to lines of action, and thence to goals, is the product not of individual choice but at least as much of others' successful "ecological control" over you (Padgett 1981). Victory, in Florence, in chess, or in *go* means locking in others, but not yourself, to goal-oriented sequences of strategic play that become predictable thereby.¹⁰

Robust action resolves the contradiction between judge and boss because at the center there are no unequivocal self-interests. Cosimo, after all, "merely" responded graciously to the flow of requests. Because requests had to flow to him, others, not Cosimo himself, struggled to infer and then to serve Cosimo's inscrutable interests. Control was diffused throughout the structure of others' self-fashionings.

Of course, robust action will not work for just anyone. For the flow of requests to be channeled, only some network structures will do. And for the resolution of judge and boss to be credible, coherent interests must remain opaque as far down as it is conceivable to peer.¹¹ Contra Machiavelli, even Cosimo himself did not set out with a grand design to take over the state: this assumption reads history backward. As this article will show, Cosimo's political party first emerged around him. Only later, during the Milan war, did Cosimo suddenly apprehend the political capacity of the social network machine that lay at his fingertips.

⁹ Our original inspiration for the robust action idea was the research of Eric Leifer (1991; this is a revised version of his 1985 Harvard University dissertation), who studied chess. While skill, not identity, was Leifer's main focus, he did point out that experts' moves in chess and in dyadic roles often are directed toward maintaining multiple lines of play, especially in balanced situations. Of course, one difference between Florence and chess is that the multiple networks of Florence constituted an entire linked ecology of games, each game layered on top of another. One single action, therefore, might be a move in multiple games simultaneously.

¹⁰ Harrison C. White (1992) argues along similar lines. John Holland was the one who informed us that locking in others but not yourself to clear lines of play is also the secret to victory in the Japanese game *go*.

¹¹ Of course, Cosimo had goals tied to specific roles—to make money as a banker, to increase family prestige through marriage, to maintain power as leader of Florence—but the points here are three: (1) goals are properties of roles, not of persons, (2) no overarching trade-off or utility function existed for Cosimo that could prioritize these possibly conflicting role-based goals, and (3) once he was in structural position, success in attaining these goals flowed to him without tactical intervention or even effort on his part. In Cosimo's special position, indeed, which role was in play at which time was not transparent. Therefore, whether Cosimo de' Medici was really a person, as conceived by modern liberalism, is undecidable by any means available to us, or to them (cf. Goffman 1974, pp. 293–300; Foucault 1975).

The bulk of this article is an archaeological dig for the structural preconditions of that learning and of that success.

These arguments will be developed in the following stages: after a brief summary of data, we will analyze first the attributional composition and then the social network structure of the Medici party, during the period 1427–34, as compared with those of their opponents, the “oligarchs.” After this cross-sectional anatomy, we will sketch the long-term historical dynamic—the emergence first of the marriage and second of the economic patronage halves of the Medici party as a function of ongoing transformations within the Florentine elite. At the end, we will show how contradictory networks induced both robust action in Cosimo and political legitimacy in the Medicean state.

DATA SOURCES AND SELECTION

This article is empirically possible because of the thorough and impressive work of many historians of Florence. In particular, we build on the work of Dale Kent, whose book, *The Rise of the Medici* (1978), in the tradition of Lewis Namier (1929), is an intimate prosopographical description of the network foundations both of the Medici party, or faction, and of the looser alliance system of their opponents.¹² From the detailed text of this account, we coded a core network data set, which consists of information on the following nine types of relations among early 15th-century Florentine elite families: (a) one type of kinship relation—intermarriage ties,¹³ (b) four types of economic relations—trading or business ties, joint ownerships or partnerships, bank employment, and real estate ties,¹⁴ (c) two types of “political” relations—patronage and per-

¹² Some may question whether the term “party” is apt for such an early time period. If presentist definitions, which include reference to mass electorates, are insisted on, then of course the Medici did not organize a party. But the Medicean organization was mobilized, in part, in order to influence the outcomes of popular elections (called “scrutinies”) for government office, albeit in a restricted electorate. This certainly fits dictionary meanings of the term.

¹³ At the valuable suggestion of Ronald Breiger, we took care in the second round of our coding to distinguish the family that provided the marrying male from the family that provided the marrying female. Hence, unlike the data provided to Breiger and Pattison (1986), the interfamily marriage relations analyzed here are asymmetric. Only marriages occurring in the time period 1394–1434 were coded. The modern reader may need reminding that all of the elite marriages recorded here were arranged by patriarchs (or their equivalents) in the two families. Intraelite marriages were conceived of partially in political alliance terms. Hence, there is little doubt that, in this time and place, marriage relations were interfamily, not interpersonal, relations.

¹⁴ Kent’s sources for all these different types of economic relations (except bank employment) were the 1427 and 1433 *catasti*, which are registers of tax reports. Trading and partnership data were symmetric, by definition, since information on directional-

sonal loans,¹⁵ and (d) two types of personal friendship relations—personal friends and *mallevadori*, or surety, ties.¹⁶ Social network ties were constituents of, as well as backdrops to, Florentine political party formation.

These network data, coded from Kent, were supplemented with attributional data coded from a variety of sources: (a) economic wealth was obtained from the computer tape of the 1427 *catasto*, coded and generously made available for public access by the late David Herlihy and Christiane Klapisch-Zuber (1981);¹⁷ (b) “date of first Prior,” the Florentine measure of family social status, was obtained from Najemy (1982) and Kent (1975);¹⁸ (c) neighborhood residence, at both the ward (*gonfalone*) and the quarter levels, was obtained from Herlihy and Klapisch-Zuber’s tape (1981) and from Kent (1975); and (d) 1403 tax assessments, for the richest 600 households in the city, were obtained from Martines (1963). In addition, for interpretative purposes, systematic data over time on factional memberships, bank employment, city finances, and rates of neighborhood exogamy were obtained from Brucker (1962, 1977), de Roover (1966), Molho (1971), and Cohn (1980), respectively.

All in all, the unusual richness of these data, from such a distant time, bear witness to the impressive creativity and industry both of the original

ity of trade and magnitude of partnership investment was not provided by Kent. Kent’s asymmetric bank employment data were coded mostly from de Roover (1966). Real estate ties were coded as symmetric when they referred to joint ownership of property, and as asymmetric when they referred to rental relations.

¹⁵ “Political” is in quotations here because the motivations underlying these relations may be complicated: a mixture of political aid, economic exchange, personal friendship, and unspecified “building up of credits.” Such mixtures of motives are typical of multifaceted patron-client relations.

¹⁶ Personal friends were coded conservatively as such only when Kent seemed to indicate, on the basis of surviving letters, that the relationship had no political content. This coding rule keeps “friendship” from being confounded by our dependent variable, membership in political faction. *Mallevadori* are friends who put up surety, or bond, to guarantee the good behavior of an exile. “Citizens who helped relatives or friends accused of political crimes were motivated by a strong sense of obligation, for they were risking not only money, but their reputations and status” (Brucker 1977, p. 29).

¹⁷ Household economic wealth was aggregated to the clan or “common last name” level of aggregation, to be consistent with other attributional data. Some small amount of error is inevitable in this procedure (Kent 1978, p. 119).

¹⁸ The Priorate (or city council), first created in 1282, was Florence’s governing body. This board was almost sacred in its ritual construction (Trexler 1980): membership, on a randomly rotating (within a select circle) two-month-term basis, was public confirmation of one’s family’s and one’s own highest status and honor, in peers’ eyes (Martines 1963). Hence, the date of first Prior measures how old and dignified one’s family was. Consciousness of their family’s date of entry into the elite, relative to other families’ dates of entry, was acute among Florentines.

Florentine scribes and of the modern historians who labor in the Florentine field.

Two matters are important to clarify at the outset, in order to frame the universe of this study—the definition of “family” and the definition of “elite.”

Operationally, “family” here means “people with a common last name.” Hence, it is more equivalent to clan than to household.¹⁹ This level of aggregation is forced on us by the nature of some of our data (particularly, date of first Prior and neighborhood).²⁰ However, F. W. Kent (1977) provides strong substantive justification for this coding. Relations between distant lineages in an elite clan were less solidary during the Renaissance than they had been in medieval *consorteria* times. But, contra Burckhardt (1860) and Goldthwaite (1968), the Renaissance in Florence was not an era of individualism. Relations among households in a clan were typically, although not universally, very strong (see also Brucker 1977, pp. 18–19). The turbulence of the times reinforced defensive cohesion (Brucker 1977, pp. 19–21; Kent and Kent 1981). And communal citizenship and office-holding regulations during the early 15th-century placed more emphasis on the unitary legal character of clans than before (Witt 1976, p. 262; Najemy 1982, chap. 8). In other words, while the clan level of data aggregation is indeed a data convenience, it was also a Florentine social reality.

The definition of “elite” is more complicated. The Florentine political elite (called the *reggimento*) was in no way identical to the Florentine economic elite, in part because of volatility in international markets. In our definition, which emphasizes political practice, we follow the lead of Kent (1975), who in turn follows the practice of Brucker (1977).

For us, a Florentine family is politically elite if it satisfies any of the following criteria: (a) it had two or more members who spoke in the *Consulte e Pratiche* three or more times between January 1429 and December 1434, (b) it had three or more members who qualified in 1433 for scrutiny, or election to the leading public offices in Florence,²¹ or (c) it was a magnate clan.²² Information on the former two criteria is contained

¹⁹ The median number of households in the 92 elite families studied here is nine. One household, in turn, may contain a number of politically active brothers, in addition to the patriarch father. (See Herlihy and Klapisch-Zuber [1985] for more information on elite vs. nonelite household composition.)

²⁰ But also Kent and Brucker, following the practice of Florentine chroniclers, frequently report alliances and ties by family name only.

²¹ In particular, the so-called *Tre Maggiori*: the Signoria (or Priorate) and the *Dodici* and *Sedici* (auxiliary colleges to the Signoria).

²² Magnate clans were old, previously powerful and violent noble families that had misbehaved politically in the past. They and their offspring were punished by the

in Kent (1975); information on the last criterion (added because of the political importance of this legally excluded group) is contained in Becker (1965) and Lansing (1991). Since official Florentine political participation was comparatively broad, this liberal definition of the ruling elite contains more parvenu new men and upper middle classes than one might expect.

In all, 215 Florentine families (i.e., clans) satisfied one or more of these criteria. Attributes of these 215 families are analyzed in the following section. Kent's book (1978) contains information on at least one marriage or economic relation for 92 of these 215 families. These 92 families are the basis for the network analysis contained herein.

By intention, Kent's book is a comparative study of the Medici party and its oligarch opponents. Hence, the 92-family "sample" is skewed, relative to the 215-family elite "universe," toward active participation in factions and away from political neutrals.²³ We are not aware of any sample bias in Kent's selection of network data *among* partisans.²⁴

NETWORK STRUCTURE

Attributional Analyses of Florentine Partisanship

Let us begin our analysis in the traditional way: namely, let us ask, Who exactly were the Mediceans and their oligarch opponents? And what social interests did they represent? Four hypotheses (not necessarily mutually exclusive) have been presented in the literature, all built around a common assumption that politics fundamentally means a struggle between self-interested groups.

victorious *popolani* in 1293, and occasionally thereafter, by being excluded legally from high public office (Becker 1965; Lansing 1991). They were politically defanged, however, only in this formal sense.

²³ This fact means that it is difficult, with these data, to study factional participation—only factional membership, given participation, can be studied. As will be seen in the attributional analyses below, however, "factional participation" included virtually all of the economically and socially important families in Florence. Nonparticipation was more an issue for the politically active, but not factional, middle classes. We are currently working to expand our network data set with primary materials, in part in order to evaluate potential sample selection problems. Padgett has been coding original Carte dell'Ancisa marriage records in the Florentine archives, covering the *longue durée* period 1300–1500. Our colleague, Paul McLean, likewise has been coding original 1427 and 1433 *catasti* tax records in order to assemble a broader cross-section of economic relations in the Florentine elite. Analyses of these primary materials will be reported in future publications.

²⁴ In particular, residential distribution of marriage dyads (possibly relevant for bias in estimated exogamy rates, calculated below) is not seriously skewed. The slight overrepresentation of San Giovanni families if anything works against the Medici neighborhood exogamy finding reported below.

An older economic class hypothesis is that the oligarchs were rich, and the Mediceans were of the middling sort. Prominent recent exponents of the class perspective, albeit not explicitly applied to the Medici, include Martines (1963) and Cohn (1980). An important variant of the economic class hypothesis applies not to volume of wealth but to its change: the Mediceans were rising economic parvenus, while oligarchs were "old money," generally on the decline.²⁵

Tables 1–4 present distributional data on these two economic class hypotheses for the entire 215-family universe of elite. Distributions of wealth and of change in wealth are tabulated for Medici partisan families, for oligarchic partisan families, and for neutral families. Party memberships were taken, here and throughout this paper, from the two lists published as appendices to Kent (1978).²⁶

Both economic class hypotheses are false. While both the Mediceans and the oligarchs were significantly more wealthy than the neutrals, the two parties' wealth distributions were statistically identical to each other. Moreover, the elite was not split into partisan rich and neutral poor; both parties were extremely heterogeneous in their wealth composition. A similar story holds for change in wealth, as measured relative to a 1403 base. A Marxist class struggle this was not.

Correlated with but not identical to the economic class view is the social class (or prestige) argument: the oligarch party was recruited from older patricians, whereas the Mediceans were new men—defined not in terms of wealth, but rather in terms of the political age of their families. Brucker (1962, 1977) and Becker (1962) are the most prominent exponents of the view that broad stretches of Florentine history should be analyzed primarily as a conflict between old and new family political cohorts. Bolstering this interpretation is the fact that numerous contemporaries, such as (pseudo) Niccolo da Uzzano (Kent 1978, pp. 212–14) and the chronicler Cavalcanti, forcefully analyzed the conflict in class terms, without always distinguishing between economic and social versions.

As presented in tables 5 and 6, evidence for the social class hypothesis is mixed. The oligarchs were indeed more skewed toward patricians than were the Mediceans, but this was due to the relative absence of new men

²⁵ Logically, a third variant might be occupational in focus: the Mediceans were bankers, and the oligarchs rentiers. However, no one in the literature has, to our knowledge, seriously proffered this view, for the simple reason that Florentine economic elites are well-known to have been extremely multifaceted and nonspecialized in their money-making activities.

²⁶ Kent identified Medicean partisans primarily from private letters to and from the Medici. She identified oligarch partisans from lists of those exiled when the Medici took over.

TABLE 1
REGGIMENTO FAMILY DISTRIBUTIONS OF WEALTH IN 1427

| Gross Wealth (1,000 florins) | No. of Oligarch Families | No. of Medicean Families | No. of Neutral Families | Total No. of Families | Proportion Partisan |
|---------------------------------|--------------------------------|--------------------------------|-------------------------------|-----------------------------|------------------------|
| More than 100 | 4 | 2 | 0 | 6 | 1.000 |
| 50-100 | 7 | 7 | 6 | 21 | .714 |
| 10-50 | 16 | 23 | 44 | 86 | .488 |
| 0-10 | 14 | 10 | 58 | 83 | .301 |
| Missing | 0 | 0 | 18 | 19 | .053 |
| Total | 41 | 42 | 126 | 215 | .414 |

NOTE.—Median gross wealth (in florins) was, for oligarch families, 21,053; for Medicean families, 20,874; for neutral families, 9,052; for total families, 12,414. The source for the data in this table is Herlihy and Klapisch-Zuber (1981). “Family” is operationally defined by common last name. Hence, the gross family wealth reported here is sums of the prededuction wealth of all households that shared last names in the 1427 *catasto*, according to Herlihy’s computerized coding. Numbers do not add perfectly because of families whose partisan loyalties were split. When split families had a majority on one partisan side or the other, they were allocated to the majority side for purposes of the attributional analyses here. However, six families were tied, and excluded from all tabular breakdowns (but not from proportion partisan calculations).

TABLE 2
KOLMOGOROV-SMIRNOV TESTS FOR DISTRIBUTIONAL DIFFERENCE IN FAMILY WEALTH

| | Kolmogorov- Smirnov Statistic | One-tailed <i>P</i> |
|-------------------------------|-------------------------------------|---------------------|
| Mediceans vs. oligarchs | .1051 | .632 |
| Mediceans vs. neutrals | .3942 | .000 |
| Oligarchs vs. neutrals | .3202 | .002 |

NOTE.—Here and in tables 4 and 6, Kolmogorov-Smirnov statistics were calculated on the basis of underlying continuous distributions, not on the basis of the summary tables 1, 3, and 5, in which the data have been made ordinal through cut points. Stricter one-tailed tests which use the chi-square approximation, not the more usual two-tailed Kolmogorov-Smirnov tests, are applied in tables 2, 4, and 6 since the literature implies the following expectations: Mediceans were on average less wealthy, more recently wealthy, and newer (in terms of first Prior) than oligarchs. See Blalock (1972, p. 264) for this one-tailed chi-square approximation procedure, which makes it harder to reject the hypothesis that parties are different. (I thank Ed Laumann for this suggestion.)

from the oligarch party, not to the absence of patricians from the Medicean side. Mediceans were not more new men than the oligarchs; they simply were more socially heterogeneous. Relative to political neutrals as a control group, the Mediceans were distinctly old-guard patrician in cast.

Finally, both Dale Kent herself (1978; Kent and Kent 1982) and F. W.

TABLE 3

REGGIMENTO FAMILY DISTRIBUTIONS OF RELATIVE WEALTH CHANGE, 1403-27

| Estimated Change in Relative Wealth | No. of Oligarch Families | No. of Medicean Families | No. of Neutral Families | Total No. of Families | Proportion Partisan |
|--|--------------------------------|--------------------------------|-------------------------------|-----------------------------|------------------------|
| More than 100%..... | 2 | 7 | 7 | 17 | .588 |
| 0% to 100%..... | 15 | 11 | 19 | 46 | .587 |
| 0% to -50%..... | 8 | 7 | 17 | 34 | .500 |
| -50% to -100% | 4 | 8 | 17 | 29 | .414 |
| Missing..... | 12 | 9 | 66 | 89 | .258 |
| Total..... | 41 | 42 | 126 | 215 | .414 |

NOTE.—Median tax in 1403 was 24 florins; median wealth in 1427 was 22,500 florins. The sources for the data in this table are Herlihy and Klapisch-Zuber (1981) and Martines (1963). Since systematic data on wealth do not exist before the 1427 *catasto*, this table is based on the tax tables of 1403 *prestanza* (or forced loans) found in Martines (1963, pp. 353–65). These tables report tax data only on the 150 wealthiest households in each of the four quarters—that is, 600 households in all. Thus, the 1403 *prestanza* information used here is a truncated data set. *Prestanza* tax data of course are not comparable to gross wealth data, so both data sets were standardized by their medians before estimated change was calculated—that is, estimated change = [(1427 wealth/wealth median) – (1403 tax/tax median)]/(1403 tax/tax median). Because of this standardization, “estimated change” refers not to absolute change in florins, which is impossible to know, but rather to relative change, in ranked comparison to peers. Data in the above table (and associated medians) include only those families with nonmissing data in both 1403 and 1427. Missing data is a serious problem here because of the truncated nature of Martines’s 1403 “wealthiest” data set.

TABLE 4

KOLMOGOROV-SMIRNOV TESTS FOR DISTRIBUTIONAL DIFFERENCE IN (Estimated)
RELATIVE WEALTH CHANGE

| | Kolmogorov- Smirnov Statistic | One-tailed <i>P</i> |
|-------------------------------|-------------------------------------|---------------------|
| Mediceans vs. oligarchs | .1735 | .395 |
| Mediceans vs. neutrals | .1606 | .333 |
| Oligarchs vs. neutrals..... | .2954 | .033 |

NOTE.—For an explanation of the statistics used, see table 2.

Kent (1977, 1987; Kent and Kent 1982), without directly disagreeing with either class view, distinctively emphasize the importance of neighborhood: The Medici party was rooted in the San Giovanni quarter, particularly the Medici’s home ward of Lion d’oro, whereas their opponents were centered in the Santa Croce quarter.

Tables 7 and 8 show that there was no statistically significant difference between the two parties by neighborhood. This does not mean that

TABLE 5

REGGIMENTO FAMILY DISTRIBUTIONS OF SOCIAL PRESTIGE

| Date of First Prior | No. of Oligarch Families | No. of Medicean Families | No. of Neutral Families | Total No. of families | Proportion Partisan |
|------------------------|--------------------------------|--------------------------------|-------------------------------|-----------------------------|------------------------|
| 1282-1299..... | 24 | 19 | 26 | 72 | .639 |
| 1300-1342..... | 10 | 7 | 30 | 48 | .375 |
| 1343-1377..... | 5 | 9 | 31 | 45 | .311 |
| 1378-1434..... | 2 | 7 | 33 | 44 | .250 |
| Missing | 0 | 0 | 6 | 6 | .000 |
| Total | 41 | 42 | 126 | 215 | .414 |

NOTE.—Median date of first Prior is, for oligarch families, 1289; for Medicean families, 1318.5; for neutral families, 1349; for total families, 1327. The source for the data in this table is Najemy (1982, 323-27). Except for the arbitrary date of 1300, the intervals in dates reported here are defined by major revolutions in the history of Florence: 1282, 1343, and 1378. Since large waves of new families were admitted to eligibility in the priorate during these revolutions, the historical periodization in table 5 corresponds to discrete political cohorts. Old magnates either have no date of first Prior, by virtue of never being legally eligible for this office, or have a misleadingly recent date of first Prior, if they were reinstated as *popolani* somewhere along the line. Therefore, most magnates were coded as having a date of 1284, essentially the oldest possible. There are 21 magnate families in this data set.

TABLE 6

KOLMOGOROV-SMIRNOV TESTS OF DISTRIBUTIONAL DIFFERENCE IN SOCIAL PRESTIGE

| | Kolmogorov- Smirnov Statistic | One-tailed <i>P</i> |
|-------------------------------|-------------------------------------|---------------------|
| Mediceans vs. oligarchs | .2968 | .026 |
| Mediceans vs. neutrals | .2619 | .014 |
| Oligarchs vs. neutrals | .4477 | .000 |

NOTE.—For an explanation of the statistics used, see table 2.

geography was irrelevant: the two parties mirrored each other in geographical concentration, especially relative to neutrals. The Mediceans were indeed overrepresented in San Giovanni, but then again so were the oligarchs. San Giovanni was the most polarized of quarters; Santa Croce was a distant second.

The main theme that comes through these attributional analyses is similarity, not difference. With the important exception of the absence of new men from the oligarch side, the Mediceans and oligarchs were mirror images of each other. The elite as a whole appears to have fractured in two, with no underlying social group basis.

A deep historical enigma remains. Contrary to these heterogeneous

TABLE 7

REGGIMENTO FAMILY DISTRIBUTIONS OF NEIGHBORHOOD RESIDENCE

| Quarter of City | No. of Oligarch Families | No. of Medicean Families | No. of Neutral Families | Total No. of Families | Proportion Partisan |
|--------------------------|--------------------------|--------------------------|-------------------------|-----------------------|---------------------|
| Santo Spirito | 7 | 8 | 36 | 52 | .308 |
| Santa Croce | 14 | 6 | 33 | 55 | .400 |
| Santa Maria Novella..... | 9 | 7 | 29 | 46 | .370 |
| San Giovanni | 11 | 21 | 27 | 61 | .557 |
| Missing..... | 0 | 0 | 1 | 1 | .000 |
| Total..... | 41 | 42 | 126 | 215 | .414 |

NOTE.—The source for these data, both for the four quarters and for the 16 *gonfalon*i (or wards), is Kent (1975, pp. 624–32).

TABLE 8

CHI-SQUARE TESTS OF DISTRIBUTIONAL DIFFERENCE IN RESIDENCE

| | ACROSS QUARTERS (<i>df</i> = 3) | | ACROSS GONFALONI (<i>df</i> = 15) | |
|-------------------------------|-------------------------------------|----------|---------------------------------------|----------|
| | χ^2 | <i>P</i> | χ^2 | <i>P</i> |
| Mediceans vs. oligarchs | 6.629 | .084 | 17.806 | .273 |
| Mediceans vs. neutrals | 12.554 | .006 | 29.567 | .014 |
| Oligarchs vs. neutrals..... | 2.683 | .443 | 19.660 | .185 |

membership statistics, contemporaries had a perfectly clear, almost polemical, understanding of what was at stake. Virtually all recorded participants interpreted the partisan conflict in traditional economic and/or social class terms: the oligarchs were considered to be the conservative party of old, wealthy, and threatened patricians, and the Mediceans were considered to be the heroes of the economically rising new men (Brucker 1977; Kent 1978). This in spite of the fact that it does not objectively appear to be true.²⁷ A puzzling structural mismatch existed between clear cognitive typifications of social groups at the level of culture and extreme heterogeneity and overlap of social groups at the level of behavioral action.

This puzzle about mismatch between cognition and behavior remains

²⁷ Moreover, even if the difference were one of policy, not one of membership, fig. 1 shows clearly that the Medici did not in fact represent the interests of the new men once they assumed office.

to be solved, but even at this point we can conclude that tables 1–8 provide *prima facie* evidence against classical group theories of parties, of either the pluralist or the neo-Marxist varieties. These theories assume that parties represent coalitions of groups. But political “groups” in the sense of sets of attributionally similar individuals who solve collective action problems in order to coordinate action on common (latent) interests simply did not exist in Renaissance Florence. Indeed, as we shall show below, the more homogeneous the attributes, the less coherent the collective action. We do not argue thereby that social attributes and groups are irrelevant to party formation; merely that their role needs to be understood within a deeper relational context. There is no simple mapping of groups or spatial dimensions onto parties; social attributes and group interests are “merely” *cognitive* categories, which party mobilization, networks, and action crosscut.

Social Structure: Blockmodel Analysis

We now look more directly at this mobilization, through an analysis of our nine microstructural networks. Party organization was not reflective of any one of these networks, taken alone, but Kent has already demonstrated persuasively the fact that both parties were constructed from differing concatenations of preexisting social networks. We will return in subsequent sections to examine the consequences of these patterns of micromobilization for the aggregate social characteristics (or “interests”) that they organized.

The essential step in this task is to derive an overall relational picture of Florence’s social structure, within the 92-family ruling élite. For the purposes of this article, we define “social structure” to be marriage and economic networks, which we take to be “strong ties” in Granovetter’s (1973) sense.²⁸ Figure 2a presents, in graphical form, the result of our

²⁸ This distinction between “strong tie” marriage and economic networks and “weak tie” political and friendship networks was to some extent arrived at inductively. We were concerned from the beginning with excluding patronage and friendship networks from this particular analysis, because these might be too close to our dependent variable, political partisanship, to be considered legitimate independent predictors. Marriage and economic networks, on the other hand, are driven primarily by nonpartisan calculations. It goes against the whole thrust of our article to assert that these networks were independent of politics, but the first-order consideration in economic relations was making money, while the first-order consideration in marriage was hierarchical status. Furthermore, partisan politics operated on a higher-frequency temporal pulse than did the more glacially changing marriage and economic structures. Personal loans and *mallevadori* ties, however, were ambiguous in our minds. We ran the blockmodel analyses both including and excluding these networks and examined the robustness of the resulting partitions as well as the goodness of fit (see below). Includ-

analyses: an aggregate blockmodel image of the marriage and economic networks, obtained by methods described in Appendix A. Figure 2b presents the parallel blockmodel image of “weak tie” political and friendship networks, based on the family clusters generated by the analysis of marriage and economic data. Appendix B lists the cluster memberships of those families that are contained in the various structural blocks graphed in figures 2a and 2b, along with those families’ partisan affiliations and social attributes.

Methodological and goodness-of-fit issues are addressed in Appendix A. Suffice it to say here that the structuralist research style embedded in blockmodeling aggregates actors into structurally equivalent sets, or “blocks,” in accordance with their common *external* ties with outsiders, rather than in accordance with dense internal relations with each other (as in cliques). The sets of families observed in Appendix B, in other words, were clustered or “thrust together” by common third-party relations to outside families; the blocks need not (and usually do not) contain any ties within themselves.²⁹

Three graphical points need to be borne in mind in order to interpret the figures: (1) Family labels in figures 2a and 2b do not indicate solo families; they encode the most prominent family in that structurally equivalent block of families (App. B gives details). (2) An image line or “bond” in this global portrait corresponds to at least two underlying ties between families in the linked blocks, of the graphically indicated type. (3) The triangulated circle superimposed onto the blockmodel diagram contains the dependent variable, Medici party membership. The bulk of oligarchic partisans are contained in the rectangular set of intermarrying blocks, directly beneath the Medicean party.

The first thing to observe about figure 2a is that the capacity of marriage and economic blockmodels to predict political partisanship is remarkable, especially given the virtual attributional identity of the two parties. The partisanship of the Medici family itself is impossible to predict from social structure alone, since the Medici family was deeply tied to both sides. But given the Medici split from the lower set of blocks, prediction of Medicean followers is obvious: the enclosed circle of blocks

ing these two networks did not improve goodness of fit but did have the effect of breaking up the oligarch blocks into smaller “globules,” an indicator of ties that cut across rather than reinforce the existing system. Figure 2b illustrates this graphically: Personal loans and *mallevadori* ties were sent not only to structural intimates, but also to families far distant in the social structure. Perhaps these were the “bridges” to future structural change, as elite families attempted to reach to new partners, just as Granovetter’s weak tie image conveys.

²⁹ If they do, this is indicated in the figures by a circle around the block name.

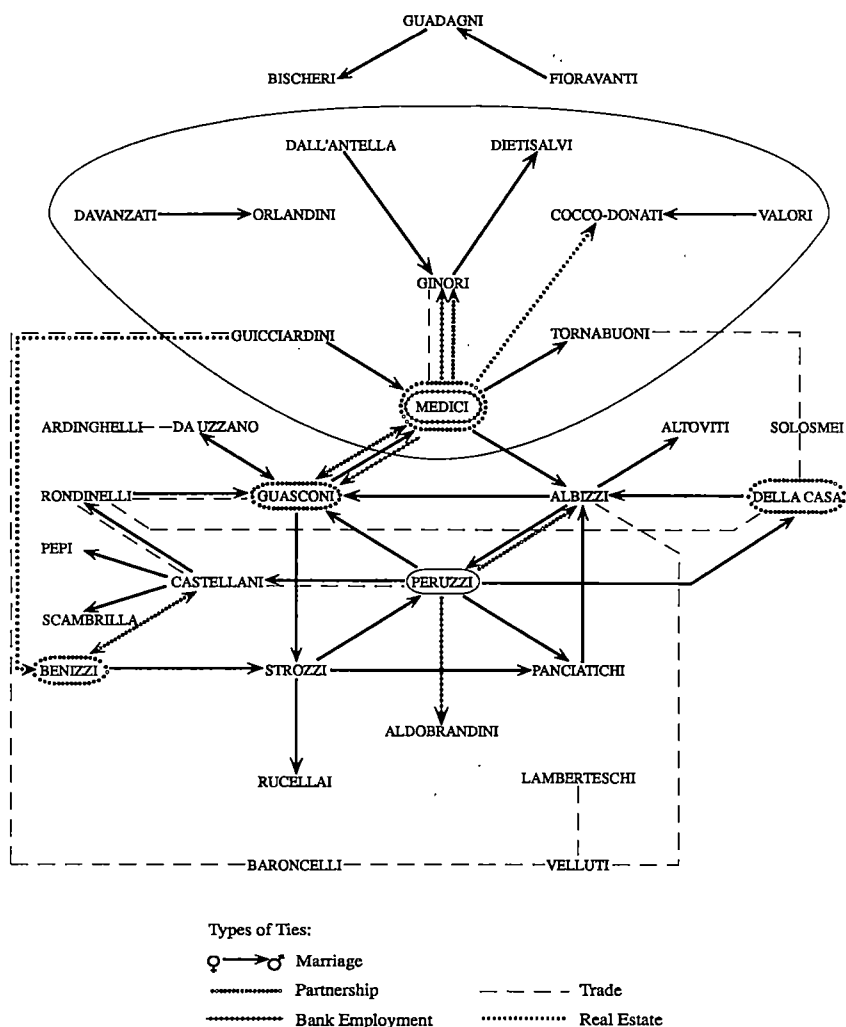


FIG. 2a.—Marriage and economic blockmodel structure (92 elite families)

contains families that had systematic access to the rest of the elite only through the Medici.

More specifically, 93% of the families within the triangulated circle were mobilized actively into the Medici party. Fifty-nine percent of all other families, including neutrals, were organized actively into the oligarch party. Excluding neutrals, 82% of all other partisan families joined the oligarch side (see App. A, tables A1 and A2, for details). Even errors in partisanship prediction here are a bit overstated: in fact, we would predict families in the cross-pressured Guasconi and Albizzi blocks to be

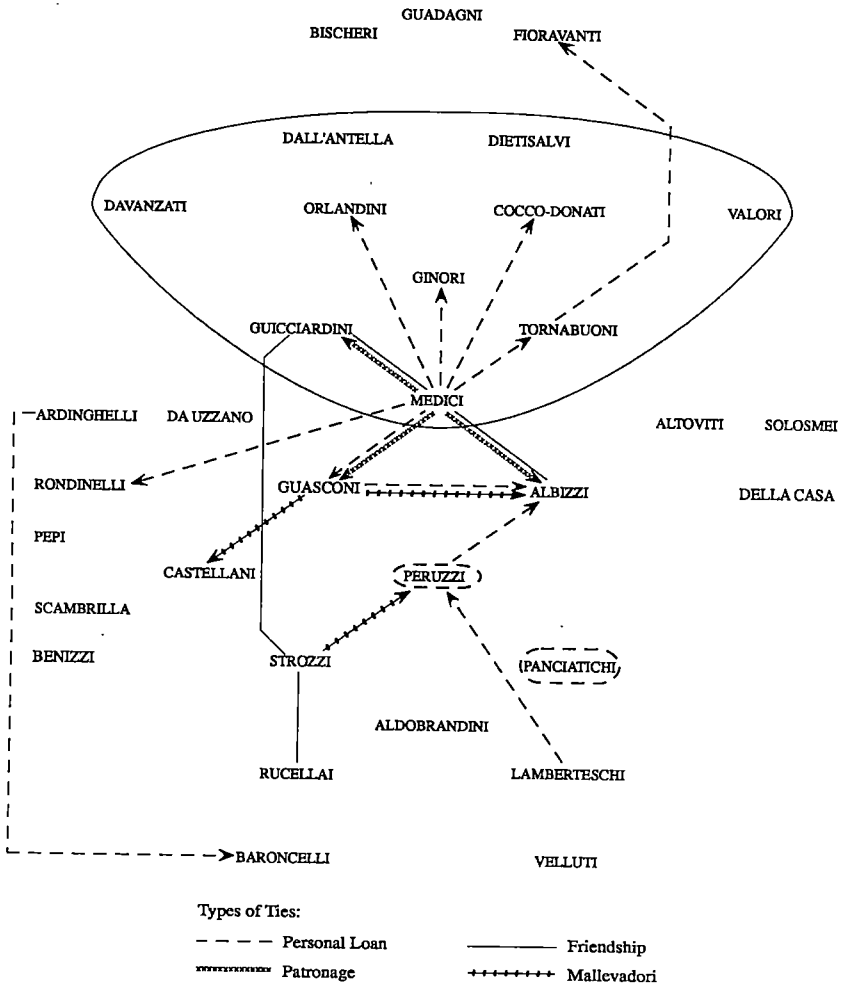


FIG. 2b.—“Political” and friendship blockmodel structure (92 elite families)

split in their partisan loyalties, which for the most part they were. Truly, the microstructure of marriage and economics was central to the formation of parties in Florence.³⁰ Rather than parties being generated by social

³⁰ The lack of congruence between patronage (fig. 2b) and the rest of the social structure (fig. 2a) is worthy of note. A standard image is that patronage is the glue that binds parties together internally. Here, however, this is not particularly true. Elites did tax, job-hunting, court, and miscellaneous favors for other elites, almost regardless of partisanship, as much as they did favors for their client followers. Even though patronage was pervasive, in Renaissance Florence party was far more deeply embedded in marriage and economic relations than it was in patronage relations.

groups, we argue, both parties and social groups were induced conjointly by underlying networks.

We will return in a later section to a causal discussion of the exact temporal unfolding of this intimate connection between social networks and party membership. For now, however, we discuss only the consequences of this social structure for political control.

Contemporaries and historians have long known that the Medici party was far more cohesive and tightly centralized than was the looser and more cross-pressured oligarch faction. With the aid of figure 2a, it is easy to see why.

The Medici party was an extraordinarily centralized, and simple, "star" or "spoke" network system, with very few relations among Medici followers: the party consisted almost entirely of direct ties to the Medici family.³¹ One important consequence for central control was that Medici partisans were connected to other Medici partisans almost solely through the Medici themselves. In addition, Medici partisans were connected to the rest of the oligarchic elite only through the intermediation of the Medici family. Medici partisans in general possessed remarkably few intralite network ties; compared to oligarchs, they were structurally impoverished.³² In such an impoverished network context, it is easy to understand how a solo dependence on a powerful family would loom very large indeed (Emerson 1962).

³¹ Graph centralization can be measured with the network betweenness statistic (C_B) of Freeman (1979, p. 230), usually interpreted as intensity of concentration of resource or information flows. Among Medicean families, marriage relations were concentrated at the level $C_B = .362$. Among oligarch partisans, marriage $C_B = .184$. On the economic front, $C_B = .429$ among Mediceans, compared to $C_B = .198$ among oligarchs. (Economic ties, including personal loans, were pooled for the latter calculations. Personal loans were included because otherwise the density of intra-Medicean ties was too low. All data were binarized and symmetrized for these calculations, as required by the Freeman measure.) We thank an *AJS* referee for the suggestion of calculating these statistics.

³² Statistics on two-step ties—the number of families that families tied to the Medici were tied to (by marriage or economic relations)—demonstrate this clearly, especially for the case of marriage. Within this 92-family data set, Medici marriage partners were married, on median, to only 2 other families. (According to blockmodel analysis, these two ties were structurally incoherent.) In contrast, the median number of marriage partners for two oligarch control groups (see below for description) were as follows: 6.5 for Santa Croce faction leader families and 4.5 for oligarchic superelite families. Data on "political" and friendship partners show the same pattern less dramatically: The median numbers of (marriage or economic) partners of Medici "political" or friendship partners were 2 and 3, respectively. This compares with the control groups' partners: 5.5 and 6.5 for the Santa Croce leaders, and 4 and 4 for the superelite. For economic partners, on the other hand, there was little difference: 3.5 for the Medici's partners versus 5 for Santa Croce partners, and only 3 for the partners of the superelite.

Conversely, the oligarch side was densely interconnected, especially through marriage. Dense structural interconnection, however, did not lead to cohesive collective action. The oligarchs were composed of too many status equals, each with plausible network claims to leadership. In dense networks in times of crisis, cacophony ensued, as each family conspired privately with other families to which they were tied about the proper course of action. Simultaneous and contradictory conversations redounded through ambagious private network channels, generating cross-pressure on each family instead of collective convergence.

A concrete behavioral example, the final "military" showdown between the two sides, will illustrate this point. On the morning of September 26, 1433, Rinaldo Albizzi, titular leader of the oligarchs, passed the word to his supporters to assemble their troops at a certain piazza in order forcibly to seize the city hall and the government (Kent 1978, pp. 332–34). At the appointed hour, only a portion of his supporters appeared. Each supporter looked around at, and no doubt consulted with, other supporters, and a stochastic threshold equilibrium ensued, in which repeated efforts by Rinaldo to assemble more troops (especially from Palla Strozzi) were offset by other supporters' changing their mind and drifting away.

In contrast, while the oligarchs dithered, the Medici immediately and decisively mobilized their supporters to join the Priors in the embattled Palazzo Vecchio. The clarity of this coordinated response was astonishing, given the fact that Cosimo and Lorenzo de' Medici were in Venice at the time.³³ As a result of this Medicean mobilization, no military engagement actually ensued, since it became clear to Albizzi supporters, if not to Rinaldo himself, that they had no chance. At this point, popular opinion swung massively behind the Medici, Cosimo was recalled from Venice in triumph, and Cosimo's oligarchic enemies were exiled, this time for good.

Kent (1978, pp. 228–34) also gives examples, less dramatic than this, of the greater cohesion of the Medici side during 1427–28 electoral maneuvering.

Even granted this explanation of the greater cohesion of the Medici side, an equilibrium puzzle remains: How could such a centralized spoke system maintain itself? Florentine clientage systems historically had been notoriously fluid. And the oligarchs had a clear incentive to marry into or do business with Medici supporters, thereby inducing cross-pressure and sowing dissent. The Medici supporters would appear to have had

³³ Temporarily, the Medici brothers had been exiled. They were not, however, successfully cut off from secretly communicating with and issuing standing orders to their supporters.

an equally clear incentive to respond, in order to alleviate their extreme dependence. At the very least, one would assume Medicean supporters would want to interbreed among themselves, for defensive if no other purposes, for only then would they have the organizational capacity to resist Medici domination, should they ever choose to do so. Structural isolation among Medici partisans would seem to have been in the long-term self-interest only of the Medici.

This structural atomization puzzle is only deepened through closer inspection of figures 2a and 2b. Two large blocks of Medici supporters were connected to the Medici essentially through marriage alone. Other blocks of Medici supporters were connected to the Medici solely by economic or by personal loan relations. The Medici had strongly multiplex ties with the oligarch Guasconi block,³⁴ but, within their own party, the Medici did not marry those families with whom they engaged in economic relations, nor did they do business with those whom they married.³⁵

This is in sharp contrast to the greater multiplexity of relations on the oligarch side: core oligarch families, the superelite and the Santa Croce faction leaders (see below), married into 40% and 38%, respectively, of the clans with whom they did business.

Most sociologists' Durkheimian presumptions are that the more overlapping ties one has with another, the more closely and holistically bound the other is to you. Obligations from one sphere spill over into another. The Medici, however, apparently believed the opposite: to control followers politically, segregate one's social relations with them. On the whole, multiplex ties (across marriage and economics) were discouraged. Thus, structural isolation operated in two ways: the marriage and economic isolation of partisans from all others (including other partisans), and the segregation of types of ties with the Medici themselves.

Comparative Statics of Attributes with Networks

The beginnings of an understanding of stability can be gained from a reexamination of the social attributes listed in Appendix B. The large Guicciardini and Tornabuoni blocks, with whom the Medici were intermarried, were composed virtually entirely of patricians (i.e., date of first Prior before 1343) who also had substantial wealth. In sharp contrast, the Ginori, the Orlandini, and the Cocco-Donati blocks, to whom the Medici were connected through economic or personal loan ties, were

³⁴ The Medici bank was a primary source of these exceptional multiplex ties. Often such multiply tied exceptions did not politically support the Medici in the end—e.g., two prominent families in the Guasconi block: the Bardi and the Guasconi themselves.

³⁵ There was only one exception to this rule: the Tornabuoni.

composed almost entirely of new men (date of first Prior after 1343). The remaining Medicean blocks—the Davanzati, the dall'Antella, the Dietisalvi, and the Valori blocks—were for the most part patricians of only modest wealth. These blocks of families were connected to the Medici indirectly, via intermarriage with economically connected new men.

The hybrid social class character of the Mediceans, revealed in tables 5 and 6, has now been clarified: actually, the Medici party was an agglomeration of structurally disjoint patrician and new men components. Different social class interests were embodied in the Medici party; (social) class contradiction was quite real. Not only that, but within the party these elements were separately clustered by Medici ties and then kept rigidly segregated from each other, to be connected indirectly only through the Medici themselves.

Given this, there is no particular mystery about the low intermarriage rate among Mediceans: patrician and new men supporters despised each other. Status-conscious patricians (Medici included) usually would not dream of sullyng their own honor by marrying into new men families; such would be a downward-mobility admission of status equality. Part of the distinctive Medicean party organization, in other words, was simply leveraged off and sustained by the ordinary cognitive classifications and social marriage rules of Florentine elites.

A close reexamination of Appendix B also reveals a second striking attributional cleavage between these Medicean social class segments, this time on the dimension of neighborhood. Almost none of the patricians in the Guicciardini and Tornabuoni blocks came from the San Giovanni quarter, where the Medici resided. The Medici, in other words, did not marry those with whom they lived.³⁶ However, 10 of the 14 families in the other Medici blocks—both new men and the indirectly tied patricians, married to new men—resided in San Giovanni. The Medici, in other words, did a great deal of business (plus personal loans) with those with whom they lived. In addition, they somehow induced local San Giovanni patrician supporters to marry into the families of their economic partners, rather than into their own.

Thus, the Medicean supporters were deeply cleaved on two attributional dimensions simultaneously—social class (i.e., prestige) and neighborhood. Not only did the various components despise each other; they did not run into each other much either. Only the Medici family itself linked the segments.

To give a more precise sense of the distinctiveness of this Medici agglomeration, we present table 9. The upper panels of the three parts

³⁶ Weissman (1982) makes vivid the dense piazza-oriented street life of Renaissance Florence, in which one constantly mingled with (or at least ran into) one's neighbors.

TABLE 9
SOCIAL ATTRIBUTES AND PARTISANSHIP OF TIES: MEDICI VERSUS SUPERELITE VERSUS SANTA CROCE FACTION LEADERS

| SOCIAL PRESTIGE: DATE OF FIRST PRIOR | | | | | | | |
|---|-----------|-----|--------------|-----------|-----|-----------------------------|-----------|
| Medici | | | Superelite | | | Santa Croce Faction Leaders | |
| Pre-1343 | Post-1343 | n | Pre-1343 | Post-1343 | n | Pre-1343 | Post-1343 |
| Allocation of types of ties: | | | | | | | |
| Marriage (M)..... | .07 | 28 | .93 | .07 | 58 | .87 | .13 |
| Friendship (F + M) | .00 | 9 | .87 | .13 | 31 | .79 | .21 |
| "Political" (L + P) | .38 | 45 | .86 | .14 | 21 | .69 | .31 |
| Economic (T + P + B + R) | .56 | 27 | .88 | .12 | 16 | .86 | .14 |
| Total | .31 | 109 | .90 | .10 | 126 | .81 | .19 |
| Political support consequences of ties:* | | | | | | | |
| Marriage (M)..... | .10 | | .73 | .31 | | .85 | .37 |
| Friendship (F + M) | ... | | .57 | .00 | | .84 | .00 |
| "Political" (L + P) | .97 | | .42 | .17 | | .82 | .20 |
| Economic (T + P + B + R) | .90 | | .81 | ... | | .85 | .50 |
| Total | .94 | | .65 | .25 | | .84 | .27 |
| ECONOMIC CLASS: WEALTH (in 1,000 Florins) | | | | | | | |
| Medici | | | Superelite | | | Santa Croce Faction Leaders | |
| More than 25 | 0-25 | n | More than 25 | 0-25 | n | More than 25 | 0-25 |
| Allocation of types of ties: | | | | | | | |
| Marriage (M)..... | .18 | 28 | .79 | .21 | 58 | .83 | .17 |
| Friendship (F + M) | .11 | 9 | .81 | .19 | 31 | .86 | .14 |

| | | | | | | | | | |
|--|-----|-----|-----|-----|-----|-----|-----|-----|----|
| "Political" (L + P) | .51 | .49 | 45 | .71 | .29 | 21 | .94 | .06 | 16 |
| Economic (T + P + B + R) | .56 | .44 | 27 | .50 | .50 | 16 | .52 | .48 | 21 |
| Total | .63 | .37 | 109 | .75 | .25 | 126 | .78 | .22 | 81 |
| Political support consequences of ties:* | | | | | | | | | |
| Marriage (M) | .72 | .80 | | .72 | .64 | | .74 | 1.0 | |
| Friendship (F + M) | .73 | ... | | .54 | .33 | | .61 | ... | |
| "Political" (L + P) | .67 | .74 | | .47 | .17 | | .60 | ... | |
| Economic (T + P + B + R) | .61 | .92 | | .79 | .81 | | .76 | .85 | |
| Total | .68 | .78 | | .63 | .54 | | .69 | .92 | |

NEIGHBORHOOD: CITY QUARTER

| | Medici | | n | Superelite | | n | Santa Croce Faction Leaders | | |
|--|--------------|-----------------|-----|------------|-----------------|-----|-----------------------------|-----------------|----|
| | San Giovanni | Outside Quarter | | In Quarter | Outside Quarter | | Santa Croce | Outside Quarter | n |
| Allocation of types of ties: | | | | | | | | | |
| Marriage (M)..... | .14 | .86 | 28 | .36 | .64 | 58 | .33 | .67 | 30 |
| Friendship (F + M) | .33 | .67 | 9 | .29 | .71 | 31 | .36 | .64 | 14 |
| "Political" (L + P) | .40 | .60 | 45 | .48 | .52 | 21 | .50 | .50 | 16 |
| Economic (T + P + B + R) | .59 | .41 | 27 | .31 | .69 | 16 | .43 | .57 | 21 |
| Total | .38 | .62 | 110 | .36 | .64 | 126 | .39 | .61 | 81 |
| Political support consequences of ties:* | | | | | | | | | |
| Marriage (M)..... | .58 | .76 | | .65 | .73 | | .80 | .77 | |
| Friendship (F + M) | .78 | .76 | | .33 | .57 | | .70 | .64 | |
| "Political" (L + P) | .82 | .62 | | .60 | .18 | | .75 | .50 | |
| Economic (T + P + B + R) | .84 | .60 | | .80 | .80 | | .78 | .82 | |
| Total | .80 | .68 | | .61 | .62 | | .76 | .72 | |

NOTE.—Superelite families are Peruzzi, Albizzi, Strozzi, and Gianfigliuzzi. Santa Croce faction leaders of the oligarch side are Peruzzi, Ricasoli, and Castellani. Amount shown, except for n's, are proportions.

* Political support consequences of ties = (no. of ties to fellow partisans)/(total no. of ties), with split families allocated proportionately. A tie to a neutral family is treated the same here as a tie to an enemy.

tabulate prestige, wealth, and neighborhood endogamy rates for the Medici and for two comparable oligarch reference groups: a set of superelite families—the Peruzzi, the Strozzi, the Albizzi, and the Gianfigliuzzi—and a set of Santa Croce faction leaders—the Peruzzi, the Ricasoli, and the Castellani families. The lower panels of the three parts give the partisanship consequences of the network mobilization ties tabulated in the upper panels.

We begin by discussing the first control group, the superelite. Table 9, part A, on prestige, shows that the Peruzzi, Strozzi, Albizzi, and Gianfigliuzzi families were patrician to the core. It mattered not what the type of network relation was—marriage, economic, “political,” or friendship; across the board, these families directed about 90% of their ties to fellow patricians. New men were snubbed in all spheres.

Table 9, part B, on wealth, tells a similar story, with one exception: the superelite did business with many less well off families. But this was just another sign of their exclusivity on prestige. Even though many patricians had fallen on hard times, the superelite stuck with them in their economic relations, rather than switch to wealthier new men.

Santa Croce factional leaders behaved little differently, except of course that their degree of social exclusivity was slightly less than that of the superelite, since their own status was slightly lower.

Compared to these reference groups, the Medici's own network strategies were, as already argued, more differentiated. In marriage and friendship, the Medici were even more snobbish than the superelite, if that were possible. However, in the economic sphere, the Medici associated heavily with the new men, quite unlike their elite counterparts. “Political” ties (i.e., personal loans and patronage) were intermediate in social class endogamy; the Medici established such relations with both their economic and marriage partners. The Medici's distinctiveness within the elite, in other words, was not that they represented new men; their distinctiveness was that they associated with them at all.

The political responsiveness of those new men with whom the Medici chose to associate was breathtaking: 90% and 96% of those new men tied to the Medici through economic or “political” relations became active Medici partisans. This rate of response was so overwhelming, indeed, that the fact that the Medici did not associate with more new men shows that party building was not the only thing on their minds. New men not explicitly mobilized through Medici ties showed no great enthusiasm for the Medici cause (see tables 5, 6).

Thus, we have an important insight into the reason for the strong contemporary image of the Medicean party affair as a social class struggle. It was not that the Medici did all that much actively to mobilize new men. It was that the oligarchs did so extraordinarily little. In as deeply

elitist a context as Florence, a mere pittance thrown the new men's way, even by such an archpatrician family as the Medici, could generate overwhelming response. Parvenu new men, anxious for inclusion but long barred from entry, were structurally available, awaiting mobilization, for reasons that had nothing to do with the Medici per se. The Medici opened the door just a crack, and for that they were polemically tarred by their enemies with the rhetorical brush of class traitor: the label "heroes of the new men" connoted contempt. *Cognitive classification of Medici group identity, both by contemporaries and by historians, was the product not of Medici action but of vitriolic oligarch polemics* (supported, no doubt, by wishful thinking among new men). Inscrutable Cosimo of course did nothing to deny it.

Table 9, part C, demonstrates statistically what we have already asserted about neighborhood. Both the superelite and the Santa Croce faction leaders were citywide in their networks, regardless of type of tie.³⁷ In contrast, the Medici's relations geographically were quite differentiated: only 14% of Medici marriages were within their home quarter, compared with 36% for the superelite. Yet 59% of Medici economic ties were within San Giovanni, compared with 31% for the superelite.

We now have a clear picture of the structure of the Medici party and of its roots in elite network strategies. The Medici party was an agglomeration of doubly disarticulated parts: structurally isolated new men living within San Giovanni, whom the Medici mobilized directly through economic relations, and structurally isolated patricians residing outside San Giovanni, whom the Medici mobilized directly through marriage. Conscious residential segregation, as well as "natural" social class segregation, were keys to the inhibition both of independent ties among followers and of multiplex ties with the Medici themselves. The result was an awesomely centralized patrimonial machine, capable of great discipline and "top down" control because the Medici themselves were the only bridge holding this contradictory agglomeration together.³⁸

³⁷ Perhaps an exception could be argued for the cases of loans and patronage.

³⁸ Our general position on the interrelation of social attributes and social networks can now be clarified. Obviously (contra some occasionally overstated polemics by network aficionados) we do not believe that social attributes are irrelevant: the particular way in which the Medici recombined social attributes through networks is the heart of the story here. What we object to is the arraying of attributes discretely as groups or spatially as grids—a procedure that presumes attributes to be behaviorally meaningful in a network vacuum. Of course, actors in the system, as well as researchers, do exactly these clustering procedures mentally when they analyze their own social structure; this is what "boundedly rational" cognitive classifications are all about. But there is a widely underappreciated gap between these macrocognitive (or "cultural") operations and microbehavioral "local action" taken by concrete individuals in very particular, heterogeneous, and often cross-pressured circumstances. Sim-

The specific micromechanisms that translated this network structure into Medici control were as follows:

1. Spoke structure induced common dependence of partisans on the Medici for access to the rest of the elite, and it forced any sensitive intraparty communication that required tie-enforced trust to be channeled through the Medici (cf. Molho 1979, p. 19).

2. Double segregation of attributes inhibited defensive counteralliance among mutually suspicious partisans: no "revolt of the colonels" was possible.³⁹

3. Interactionally, marrying geographically distant patricians, whom the Medici met casually only rarely, kept affine relations socially proper and formal—thereby inhibiting unwanted presumptions of familiarity and status equality. In contrast, doing business with new men inside the neighborhood engendered motivationally useful friendliness. The status gap between Medici and new men was so enormous that the Medici did not need to fear any slackening of their abject deference.

4. The attributional heterogeneity of the party made the Medici party a swing vote, potent beyond its numbers, in Florentine legislative politics. The reason, explained below, was the bitter class polarization that was reviving in Florence.

Thus, attributional heterogeneity and contradiction of group interests were not a problem for Medici party control. Quite the contrary, they were the keys to Medici control. For this result to be generally true, however, surrounding cognitive group identities (and animosities) must be intense. Stable monopoly of broker position is leveraged off this, and practical political organization becomes cognitively invisible (or at least murky) to the outside world precisely when it cuts across strong identities.

NETWORK DYNAMICS

The Dynamics of Party Formation: Patrician Marriage

We have insufficient space in this article to show how figure 2a emerged historically in detail, but in the next two sections we shall sketch the

plifying social reality into homogeneous subsets "with common interests" rips individuals out of their (often contradictory) multiple network contexts and obscures the very heterogeneity and complexity of which organizations like the Medici party are constructed.

³⁹ A "revolt of the colonels" requires more than just comparing dissatisfactions. Colonels have to have confidence that other colonels are not just stabbing them in the back. There has to be an organizational infrastructure, independent of the boss, through which they can coordinate. And there has to be something they are offering to each other that is better than their current state. Crosscutting Medici networks inhibited each of these preconditions.

driving dynamic: elite marriage and economic networks were reconfigured by working-class revolt and wartime fiscal crisis, respectively. Elite reconfiguration explains why oligarchs would not marry Medicean patri-cians.

Our core conclusion regarding party formation will be simple: Cosimo de' Medici did *not* design his centralized party, nor did he intend (until the very end) to take over the state. The network patterns of figure 2a were produced by oligarchs' earlier successful reassertions of their own control. The Medici party grew up around Cosimo and Lorenzo from raw network material unintentionally channeled to them by the oligarchs' previous smashing both of the wool workers known as *ciompi* (1378) and of the new men's challenge during the Milan and Lucca wars (1424–33). Only very late in the game, we shall argue, did the Medici adaptively learn of the political potential of the social network machine that lay at their fingertips. In almost Hegelian fashion, oligarchs crafted the networks of their own destruction.

Historical trends in neighborhood exogamy.—The first thing to appreciate in any dynamic analysis of Medicean marriage strategy is the fact, uncovered by Cohn (1980), that increasing rates of neighborhood exogamy were a historical trend in Florentine elite marriage behavior. The Medici were not tactical revolutionaries; they were simply the leading edge of an ongoing transformation in the network structure of the Florentine elite.

Table 10 presents a recalculated version of Cohn's neighborhood data, along with our own data for historical comparison. By coincidence, Cohn studied the mid-14th- and late 15th-century periods, which bracket the early 15th-century focus of this study. Since Cohn's primary focus was on changing marriage patterns within the lower classes, the volume of elite marriages is lower than that which was extracted from Kent.⁴⁰ Cohn's listing of his late 15th-century data permitted a further breakdown into "state marriages" and others—a concept that has no real meaning for the period before the Medici changed the republic into a de facto principality.⁴¹ It is reassuring, given different operational defini-

⁴⁰ For the lower classes, or *popolo minuto*, Cohn found a trend just the opposite of that of the elite: increasing rates of marriage *within* neighborhoods. Some portion, but not all, of this trend was due to growing immigrant ethnic enclaves, organized around the silk industry. Cohn argues forcefully that new administrative methods of lower-class control (containment within parishes) during the late 15th century inhibited cross-neighborhood contact and organization among workers. A recurrence of the Ciompi revolt became well-nigh impossible (cf. Molho 1979).

⁴¹ We label as "state marriages" those marriages registered by Lorenzo de' Medici's personal secretary. "The majority of these marriages were celebrated at the Medici palace and were witnessed by Lorenzo. In several instances Lorenzo even provided

TABLE 10
HISTORICAL TRENDS IN ELITE MARRIAGE

| | No. within Parish | No. within Gonfalone | No. within Quarter | No. across Quarter | Total |
|--|-------------------------|----------------------------|--------------------------|--------------------------|-------|
| I. Fourteenth-century elite marriages (1340-83)* | 5 (20) | 8.5 (34) | 12 (48) | 13 (52) | 25 |
| II. Early 15th-century elite marriages (1395-1434):† | | | | | |
| Patrician-patrician | | 16 (12) | 37 (29) | 92 (71) | 129 |
| Patrician-new man | | 8 (27) | 17 (57) | 13 (43) | 30 |
| New man-new man | | 1 | 1 | 1 | 2 |
| Total | | 25 (16) | 55 (34) | 106 (66) | 161 |
| III. Late 15th-century elite marriages (1450-1530):‡ | | | | | |
| State marriages:†† | | | | | |
| Patrician-patrician | | 0 (0) | 1 (8) | 11 (92) | 12 |
| Patrician-new man | | 1 | 1 | 1 | 2 |
| New man-new man | | 0 | 0 | 0 | 0 |
| Total | | 1 (7) | 2 (14) | 12 (86) | 14 |
| Other elite marriages: | | | | | |
| Patrician-patrician | | 0 (0) | 2 (29) | 5 (71) | 7 |
| Patrician-new man | | 1 (17) | 4 (57) | 3 (43) | 7 |
| New man-new man | | 0 (0) | 2 (40) | 3 (60) | 5 |
| Total | | 1 (5) | 8 (42) | 11 (58) | 19 |

NOTE.—Numbers in parentheses are percentages.

* "Elite" is defined by family name plus a dowry of at least 400 florins. Data are from Cohn (1980, p. 52). To make the data comparable with others in this table, we report the number of marriages rather than Cohn's "number of marriage relations," which, for Cohn, equaled two times the number of marriages, i.e., one for each partner.

† "Elite" is defined by political participation (see text). Data are from this article. Except for "Total" in this category, entries refer to marriages between a patrician and a patrician within the political elite, to marriages between a patrician and a new man within the political elite, etc.

‡ "Elite" is defined by having passed scrutiny plus a dowry of at least 600 forins. Figures are from marriage data listed in Cohn (1980, pp. 54-56.) Due to the passage of time, "new men" is defined here as post-Ciompi (post-1383), rather than post-1343 as in the rest of this paper.

†† These are marriages notarized by Lorenzo de' Medici's personal secretary (see Cohn 1980, p. 53).

tions of the elite, that neighborhood endogamy/exogamy estimates are perfectly consistent between our two studies.

These aggregate statistics demonstrate what has already been asserted—that rates of elite marriage outside neighborhood quarter increased progressively over the course of the 14th and 15th centuries. Cohn's elite marriage data are too scanty to disaggregate reliably by decade, but the combined data give the impression that the bulk of change occurred between 1383 and about 1420—namely, precisely at the time when the patrician elite reconsolidated its control after the disastrous (from their perspective) Ciompi revolution.

Cross-sectional estimates are also consistent: for both 15th-century periods, the higher the position in the elite hierarchy, the higher the rate of marriage outside neighborhood. Patricians married other patricians at higher exogamous rates than they married new men. And, most striking in spite of the low numbers, 92% of state marriages in the late 15th century were arranged across rather than within neighborhoods. In other words, what in the early 15th century was an innovative Medici strategy of partisan control, applicable only to their own marriages, later appears to have become official state policy, applicable to all their top supporters.

This trend is better interpreted from the perspective of its origin than from knowledge of its final result: patrician neighborhood clusters based on marriage gradually dissolved. The Peruzzi-Ricasoli-Castellani clique in Santa Croce, the Albizzi and Guadagni blocs in San Giovanni, and the Strozzi group in Santa Maria Novella indicate clearly that this process was hardly complete during our period (if indeed it ever was).⁴² But this "neighborhood solidarity" mode of elite organization, which linked patricians of different stature, was a residue of the past.

In particular, this mode historically descended from feudalism. Florentine neighborhoods originally (in the 1200s) were settled by immigration from contiguous regions of the surrounding rural countryside. Urbanizing feudal lords brought their hierarchical retinues with them, thereby creating self-sufficient neighborhood pockets with stronger ties to the rural homeland than to other parts of Florence (Weissman 1983, pp. 7–9). By the 15th century, "feudalism" is hardly an apt description of Florentine

the dowry. We find the state, in effect, taking an active role in the structuring of marriage relations, which were at the same time political alliances" (Cohn 1980, p. 53).

⁴² Given the vigor of F. W. Kent's recent defense (1987) of the continuing importance of neighborhood in quattrocento Florence, a clarification is in order. We are not arguing that the importance of neighborhood for patricians declined, in some totalistic sense. In fact, we agree with Kent's emphasis on neighborhood patronage (and, we would add, neighborhood economics). However, marriage was no longer the primary basis of elite consolidation within quarter.

neighborhoods, but hierarchical intraneighborhood marriage among patricians persisted.

As neighborhood marriage solidarity disintegrated roughly around the turn of the century, however, patricians may have been almost forced to reach outside the comfortable (and essentially defensive) shells of their own wards, parishes, and piazzas. Later, such marriage outreach would provide the organizational infrastructure to undergird the emergence of a self-consciously "city" elite, psychologically (though not materially) decoupled from its original neighborhood base.⁴³

The question therefore arises: What forces led to this widespread corrosion of elite neighborhood solidarity and to its replacement by elite neighborhood exogamy? The answer to this question will take us far toward understanding the historical roots of the Medici's own innovative marriage strategy.

Political vulnerability among Medicean patricians.—The mid-14th-century, pre-Ciampi story is best told by Brucker (1962). Progressive waves of new men were economically thrust up by the internationalization of trade and finance. Without aristocratic sponsorship, such mercantile new men (hardly radicals in any event) were doomed to political ostracism and frustration. However, this upward thrust often coincided with intraelite factional cleavage. The Albizzi-Ricci factions were the most important such split in the mid-14th century, with the Ricci garnering more new men's support. Intense politics ensued from 1343 to 1378 about who was to be eligible for election to the Signoria. The new men (with artisan support) took the tack of trying to label their opponents officially as lawless magnates, and the old guard countered by trying to get their opponents designated as traitorous antipapal Ghibellines.

Eventually, such aristocratic-mercantile feuding got out of hand, and the Ciampi rose up in alliance with the artisans to take over the state for two months in 1378. Through their major guilds, the merchants quickly realigned with artisans in minor guilds to reestablish control, but in 1382 this alliance in turn was overthrown by an embittered patrician elite (which included earlier cohorts of now old "new men" merchants). These "oligarchs" excluded the artisans and ruled Florence until the Medici took over in 1434.⁴⁴

⁴³ This extrapolation into psychology is not fanciful. Without much commentary, Cohn reports (1980, p. 37) that he had much trouble reconstructing the neighborhood residences of his late 15th-century families, because notaries of the time, who registered the marriage deeds, recorded these families only as *cives Florentinis* (citizens of Florence). In contrast, in the 14th century, residence down to the level of parish was routinely recorded, irrespective of status.

⁴⁴ In the field of Florentine studies, Brucker represents the highest quality of history written with groups as the primary agents. While I disagree with his group-based approach, I have nothing but the highest respect for his pathbreaking research.

For us, the clue in this account is to understand how first the Albizzi-Ricci factional feuds and then the searing revolt of the Ciompi placed strain on neighborhood solidarities. Could it be that intraelite factional struggles of the 14th century were so intense that, first following medieval patterns, they polarized each neighborhood into competing patrician clusters (cf. Barth 1959), but then, because of Ciompi escalation, they pulverized the losing patrician side into structural isolates, which the Medici much later exploited? If so, this would account in part for the increasing rate of neighborhood exogamy, as the winners and, even more, the losers had fewer compatible intraneighborhood marriage options to choose from. Assuming social class endogamy, Florentine patrician marriages would be forced outward because of neighborhood blockage, or structural "holes," within.

We will test the plausibility of this account in pieces. One observable corollary of the hypothesis is that Medicean patrician support derived disproportionately from losers of earlier factional struggles. We have gathered evidence from secondary sources (*a*) about patrician family membership in the mid-14th century Albizzi and Ricci factions and (*b*) about patrician support for the 1378–82 Ciompi/artisan regime. We also have very limited information about the Alberti faction (a transformed descendant of the Ricci) of 1385–1400.

From the detailed text of Brucker (1962), we could identify 39 14th-century families from the Albizzi or Ricci factions that also appeared in Dale Kent's (1978) list of 1434 partisans. The relationship between these two lists of partisans is shown in table 11. The evidence is much stronger for the historical continuity of the winning oligarchic side than it is for the losing "liberal" Ricci. The Medici did indeed gain some support from families of the old 14th-century Ricci faction, but this support is not a major factor in Medicean mobilization.⁴⁵

More convincing evidence of historical continuity dates from the 1378–82 period. Najemy (1982, p. 260) has published a list of families from "traditional inner elite" families that were members of the governing Signoria during the 1378–82 corporatist guild regime. This list of 15 "collaborator" patrician families is as follows: Alberti, Aldobrandini, Ardinghelli, del Bene, Corbinelli, Corsini, Davanzati, Medici, Pitti, Rinuccini, Salimbeni, Salviati, Scali, Strozzi, and Vecchietti.

Six of these families were unequivocal Medici supporters in 1434. Moreover, the split Salviati family leaned strongly to the Medici side

⁴⁵ In hindsight, perhaps we should not have expected much from such a lengthy time-lagged effect. Not only did much transpire between the Ricci and the Medici, but the leaders of the Ricci and Alberti factions were not just ostracized; their participation, and even life, in the regime was completely obliterated.

TABLE 11
LEGACY OF MID-14TH CENTURY PARTISANSHIP

| 1360 PARTISAN FAMILIES* | 1434 PARTISAN FAMILIES† | | |
|--------------------------------|-------------------------|-------|----------|
| | Medici | Split | Oligarch |
| Albizzi faction..... | 4 | 3 | 12 |
| Split loyalties | 4 | 4 | 2 |
| Ricci faction | 5 | 2 | 3 |
| Not mentioned in Brucker | 44 | 3 | 34 |

* Brucker (1962).

† Kent (1978).

(Kent 1978, p. 55), and the technically neutral Corsini were in the process of joining the Medici when 1434 arrived (Kent 1978, p. 53). Only three collaborator families supported the oligarchs in 1434.⁴⁶

The Alberti faction of 1385–1400 was the last barrier of opposition to the revived patrician oligarchy (Brucker 1977, p. 75–102). However, apart from descriptions of the extremely wealthy Alberti themselves, who were papal bankers before the Medici replaced them as a result of the Alberti's 1393 exile (Holmes 1968; Foster 1985), almost no published information exists on the composition of this faction. We do know, however, that Acciaiuoli support for the Medici dated from the exile of Alberti supporter Donato Acciaiuoli (Brucker 1977, p. 97; Kent 1978, p. 59).⁴⁷ In addition, the close friends of Acciaiuoli who put up 20,000 florins as security for his exile included three Cavalcanti, Luigi Guicciardini, and Nicola di Vieri de' Medici (Brucker 1977, p. 29)—all later patrician supporters of the Medici.

In sum, while the evidence that historical continuity among Medicean patricians went back as far as the mid-14th century is very weak, the evidence for grounding our understanding of patrician support for the Medici in the events of the Ciompi revolution and its immediate aftermath is quite strong (albeit still only suggestive). Our data, merged with Cohn's, on neighborhood exogamy support the view that elite neighborhood solidarity dissolved in the decades following the Ciompi revolution;

⁴⁶ The Rinuccini and Salimbeni, along with the exiled del Bene and Scali, no longer figured prominently in Florentine politics.

⁴⁷ A close relationship between the Acciaiuoli and the Medici families, however, predated this period. In the mid- to late 14th century, an important client of the banker Vieri di Cambio de' Medici was Bishop Angelo Acciaiuoli, who played a major role in Florentine politics while occupying the city's episcopal see (Brucker 1977, p. 10). A marriage around 1405 consolidated this family relationship.

the data on patrician factional continuity supports the further view that structural isolation among Medicean patricians dates from this period as well.

Oligarchic elite closure.—Establishing the existence of historical continuity, however important, is not the same thing as establishing the mechanism through which such continuity was created. A priori, there is no obvious reason that factional wounds could not have been gradually healed (perhaps under the goad of a massive external threat, like the Milanese war), that disaffected elites could not have been reabsorbed (under careful controls), or that the political system could not therefore have returned to its routinely tumultuous stochastic equilibrium. In reality, however, an irreversible “ratchet effect” appears to have occurred, which we need to explain.

In particular, we argue, elite marriage networks shifted permanently during the period from 1385 to about 1420 from a quasi-feudal pattern of parallel, intraneighborhood marriage hierarchies, which had incorporated most patrician families, toward a citywide elitist pattern of cross-neighborhood marriage cycles, which co-opted “politically correct” patrician families while structurally isolating patrician “class traitors” who had collaborated with the Ciompi and artisan guilds. The folding of a dense oligarchic core into itself (the rectangular set of blocks in fig. 2a) and the segregation of patrician Medici supporters from this core (the Guicciardini and Tornabuoni blocks in fig. 2a), in other words, were two sides of the same elite reconsolidation process.⁴⁸ The effect of this marriage transformation was to keep Ciompi-type challenges from ever arising again; no longer were there fluid elite factions to play off one another.

The process is illustrated graphically in figure 3. The solid lines give a stylized portrait of the quasi-feudal pattern of parallel neighborhood hierarchies before the Ciompi.⁴⁹ As mentioned above, this marriage pattern originally derived, centuries before, from rural lord-retinue hierarchies transplanted to local city neighborhoods. During the late medieval interim, however, neighborhood hierarchies were sustained by a logic of status: enshrouded within overtly symmetric political alliance marriages,

⁴⁸ The shift from neighborhood to citywide patrician elites is exemplified in fig. 2a by the contrast between, on the one hand, the old Santa Croce hierarchical organization of Peruzzi→Castellani→Pepi and, on the other hand, the newer self-encapsulating organization of Guasconi→Strozzi→Panciatichi→Albizzi→Guasconi (and similar cycles).

⁴⁹ This portrait may conflate prematurely the Albizzi-Ricci factional underpinnings with earlier Guelph-Ghibelline and Black-White Guelph marriage structures, but no data exist at present to trace medieval stages of transformation, if any. With the new Carte dell'Ancisa marriage information currently being processed, we will be able to investigate this further.

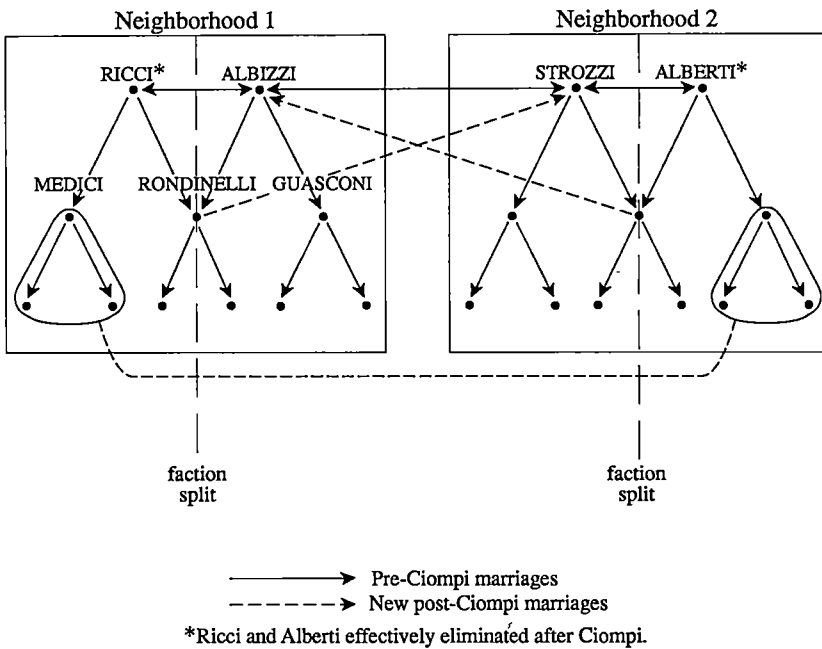


FIG. 3.—Mechanics of elite closure (1380–1420)

arranged by Florentine patriarchs,⁵⁰ was an implicit claim on the part of the family giving the dowried wife to be superior in status to the family receiving the dowried wife.⁵¹ The reasons for this were two: (a) culturally, in all ceremonial exchange or gift-giving systems, prestige accrues to the more (competitively) generous (Mauss [1925] 1967);⁵² (b) structurally, son-in-law relations often were the raw material for constructing political lieutenants (Kent 1978, p. 55).⁵³

⁵⁰ Needless to say, modern conceptions of romance are anachronistic in this deeply sexist context. Patrician fathers arranged marriages between sons around 35 years old and daughters in their teens (Herlihy and Klapisch-Zuber 1985).

⁵¹ This combination of implicit domination enshrouded in a cultural veneer of friendship is typical of patron-client relations.

⁵² This also fits with the Catholic principle of plenitude, or grace—the ideological foundation for European nobility (Lovejoy 1936; Duby 1980).

⁵³ This potential could be actualized most easily in two circumstances: (a) when the son-in-law came from a small disorganized clan, in which therefore the potentially countervailing father of the son was weak, or (b) when the son-in-law was a “black sheep” for whatever reason. Examples of both cases can easily be found in the Medici party.

Because of intense status competition, wife-giving families could not take back daughters from their wife-receiving affines without thereby relinquishing their claim to superiority. *Gonfalon*i neighborhoods in particular were strongly bounded hothouses, in which frequencies of both interaction and status rivalry were high (Weissman 1982; Kent 1977). Add asymmetric marriage to bounded neighborhoods, and the result is the network pattern drawn in figure 3: hierarchical linear trees or “pecking orders.”⁵⁴

Medieval factions were driven by local neighborhood antagonisms at their root, even when they aggregated under broader pope-versus-emperor banners (Waley 1969, p. 203). This is shown in figure 3 by factional splits between rival lords within each neighborhood. The factions link up, in geographical checkerboard fashion, across neighborhoods (cf. Barth 1959).⁵⁵ The medieval dynamic at this point was that things eventually would explode, with top leaders of the losing side suffering exile, expropriation of wealth, and burning of homes. After a rocky period of probation, however, losing lieutenants and those lower down slowly would be reabsorbed back into the system through asymmetric marriage (since new local challengers in the old winning faction reached to them for support).

The Albizzi-Ricci feuds of the mid-14th century, we argue, were just this sort of traditional medieval factional affair, except for one catastrophic complication: The Ciompi working-class revolt threatened the elite in toto, which suddenly was confronted with mass exile. Ricci partisans and, even more, patrician sympathizers with the Ciompi (like the Medici) now were not just normal elite from a losing faction; they were class traitors in a very direct and deep sense. Resurgent oligarch consciousness was seared by the Ciompi event to a degree that it structured both polemics and virtually every domestic institutional reform for decades (Brucker 1977; Najemy 1982).

After the oligarchs’ return to power in 1382, the reverberating consequences for intraelite marriage networks were profound.

1. Class traitor patrician sympathizers with the Ciompi were shunned in marriage by victorious oligarch families. This is the source of the

⁵⁴ Of course, top-level lords also had to get wives from somewhere. Inability to accept daughters from below, within their neighborhood, forced them to trade among themselves, across neighborhoods, but only at the very top. Some neighborhood bosses were prestigious enough to obtain wives from outside the city.

⁵⁵ That is, each local faction was surrounded by its enemies. The aggregation logic here is balance theory: “[neighboring] enemies of my [neighboring] enemies are my friends.”

structural barrier to marriage between the oligarchs and the Guicciardini and Tornabuoni Medicean blocks, observed in figure 2a. To see the dynamic of how this came to be, refer again to figure 3: (a) Ricci and Alberti families were effectively expunged from the diagram, the normal consequence of being leaders of losing medieval factions. (b) This left losing and now leaderless lieutenant families, such as the Medici, structurally isolated and cut off from their victorious neighbors. (c) Cross-pressured "swing-vote" families, such as the Rondinelli, who might be potential bridges for reintegrating the losers, were temporarily left hanging. But (d) marriage ostracism of patrician losers by victorious oligarchs was exceedingly intense and vengeful.

This last assertion is not mere hypothesis. When the Alberti males were finally banished in 1397, virtually no one would marry them (Foster 1985, p. 321). Before that, in 1394, the very prestigious moderate, Filippo Bastari, was sentenced to perpetual exile merely for trying to talk Rinaldo Gianfigliuzzi into changing his mind about withdrawing from a marriage agreement with the Alberti. Gianfigliuzzi had withdrawn from the Alberti agreement in the first place because of pressure he had received from Maso degli Albizzi (Brucker 1977, pp. 95–96).⁵⁶ This prominent example apparently was taken to heart, and routinized into marriage maxims, by the patrician rank and file desirous of advancement within the new regime.⁵⁷

2. Not only were Ciompi sympathizers shunned, however, but oligarchs themselves were restructured by their vengeful ostracism: cross-

⁵⁶ Such cross-family consultations about politically significant marriages involving only one of them may have been common. Kent (1978, p. 130) cites a much later 1431 case of Almanno Salviati's consulting Averardo de' Medici, his father-in-law, about the proposed marriage of Salviati's daughter to a Frescobaldi. (Averardo apparently okayed the deal.)

⁵⁷ In the early 15th century, Giovanni Morelli wrote as follows in his memoirs, intended as posthumous instruction for his sons: "[When you decide to marry] think of this first of all: don't demean yourself with an inferior person; try instead to improve your condition, though not to such an extent that she would want to be the husband and make you the wife. . . . Aside from being of old stock in the city, they should also be Guelfs who are honored by the Commune and who are free of all stains, such as those associated with treason. . . . In marriage, connect yourself with a Guelf family in government; it ought to be a powerful and trusted family, free of all scandal" (cited in Martines 1963, pp. 58–59). According to Martines, Morelli was particularly sensitive about this subject because he himself had made the mistake of marrying into the Alberti. "According to Morelli's testimony, this marriage hurt his career in public life, undermined his contacts, and left him exposed, during a period of twenty years, to tax rates aimed at his ruin" (Martines 1963, p. 59; we thank Samuel Cohn for suggesting the relevance of Morelli's memoirs).

neighborhood marriage cycles began to form.⁵⁸ A dense, citywide elite was created thereby, which transcended its earlier neighborhood base.⁵⁹

The short-term motivation for cross-neighborhood marriage cycles, we argue, was co-optation, especially of potentially bridging swing-vote families, such as the Rondinelli in figure 3. Oligarchs wanted the doors completely slammed in traitors' faces, without leaving open the traditional route for rehabilitation. And they certainly did not want to give patrician outcasts any network room to pluck away wavering supporters from themselves.

The complication was the status implications of intermarriage, mentioned above. "Big cheese" families could not reach down to, and accept daughters from, middling swing-vote patricians in the same neighborhood, without at the same time jeopardizing their own status positions in the local pecking order. But other prominent oligarch families, outside the neighborhood, could do so without disrupting either local neighborhood order. Given the decline in neighborhood intermarriage because of the ostracism, moreover, there was a demographic push to do just that. Cross-neighborhood co-optative daughter flows are illustrated as dotted lines in figure 3. Marriage cycles are the straightforward, even if unintended, result.

Since this co-optation process was occurring in all neighborhoods simultaneously (the consequence of the checkerboard pattern), the oligarchic elite as a whole closed in on itself: given the unexpected windfall of exogamous marriage into the truly elite, patricians of moderate position had fewer daughters available to send down. But this new cross-neighborhood elite was far more expansive and inclusive in status than had been the earlier medieval pattern of a federated alliance of neighborhood bosses: because of co-optation, the truly elite had fewer sons available to trade among themselves. The net result was the rectangular clique of oligarch blocks in figure 2a.

3. A final consequence was cross-neighborhood intermarriage among structurally isolated class traitor patricians. Given successful co-optation of swing votes into the reconsolidated elite and their own geographical scatter, outcast patricians were left with very few status-appropriate mar-

⁵⁸ From a network perspective, triadic cycles cause networks to fold into themselves, thereby creating clear "group" boundaries. Understanding how cyclic marriage triads formed, therefore, is the microbehavioral essence of understanding how the reconstituted patrician elite congealed.

⁵⁹ Civic humanism, a 1400–1430 ideology of citywide elites, arguably was built on top of this new elite marriage network foundation, even as it was catalyzed more proximately by 1390–1402 wars with Milan (Baron 1966).

riage options in their own neighborhoods. (We have argued, of course, that that was precisely the point.)

This left such patricians with two marriage options. (a) They could start to accept daughters from lower social groups. But this moved patricians down to the new men's status level, in other patricians' eyes.⁶⁰ (b) Alternatively, they could start to marry fellow isolates outside the neighborhood. This at least preserved their claim to status, even if it did nothing to restore power. For outcasts who wanted to remain patricians, this extraneighborhood marriage game was the only game in town. Hence, even though exogamy rates were increasing for all, intrapatrician exogamy rates were higher among outcasts than among oligarchs: Oligarchs married both outside and inside the neighborhood.

In short, this is how we claim the outcast Medici discovered their "leading edge" and highly consequential marriage strategy. Near exclusive intermarriage with patricians outside their own neighborhood was a network strategy forced on the Medici by the resurgent oligarchs' successful blockage of Medici marriage with their San Giovanni patrician neighbors. The half of the Medici party that was based on marriage was "chosen" by the Medici family only in the sense of positional chess: oligarchs structurally induced their choice.

Thus, the oligarchic clique and the Medicean spoke marriage networks were not two separate party organizations, later to butt heads as autonomous units. They emerged in tandem, as a single network, each reflexively and asymmetrically structuring the other.⁶¹

Data on change in elite marriage strategy over time.—We still have not shown how the Medici themselves, alone among patrician outcasts, eventually came to marry into the oligarchy. But before we solve this final puzzle, let us present evidence in support of the above account.

We have attempted to date the 162 marriages in our data set, which span the period from about 1395 to 1434. Fifty-three of these marriages are precisely dated by Kent in her book. For 84 of the remaining 109 marriages, we have been able to estimate the decade of the marriage, usually through triangulating the known dates of relatives with genealogical information contained in Florentine names.⁶² Because of this estima-

⁶⁰ This is not to say that patricians never would do this—say, for money. But wealthy dowries from new men daughters were always implicitly purchased at the cost of lowered prestige. The dowry money was tainted, a sign of patricians on hard times.

⁶¹ Padgett (1990, p. 450) develops a similar "reflexive structuring through mutual contestation" argument in a totally different empirical context, that of professionalism and plea bargaining in U.S. criminal courts.

⁶² Florentine names usually include the father, and frequently the grandfather, in them—e.g., Cosimo di Giovanni di Bicci de' Medici. This is yet another indicator of the Florentine concern with patrilineage.

TABLE 12
ELITE MARRIAGES OVER TIME

| Neighborhood by Estimated Date of Marriage | Overall | 1395-1410 | 1411-20 | 1421-29 | 1430-34 | No Date |
|--|-----------|-----------|---------|---------|---------|---------|
| Medici marriage strategies: | | | | | | |
| San Giovanni..... | 4 (.143) | 0 | 0 | 3 | 0 | 1 |
| Santo Spirito | 10 (.357) | 1 | 2 | 3 | 4 | 0 |
| Santa Maria Novella | 9 (.321) | 2 | 4 | 2 | 0 | 1 |
| Santa Croce..... | 5 (.179) | 2 | 1 | 2 | 0 | 0 |
| Total | 28 | | | | | |
| Peruzzi + Ricasoli + Castellani marriage strategies: | | | | | | |
| San Giovanni..... | 10 (.345) | 2 | 3 | 1 | 3 | 1 |
| Santo Spirito | 5 (.172) | 0 | 1 | 0 | 4 | 0 |
| Santa Maria Novella | 6 (.207) | 3 | 1 | 1 | 1 | 0 |
| Santa Croce..... | 8 (.276) | 4 | 0 | 1 | 3 | 0 |
| Total | 29 | | | | | |
| Strozzi + Gianfigliazzi marriage strategies: | | | | | | |
| San Giovanni..... | 11 (.344) | 4 | 2 | 1 | 2 | 2 |
| Santo Spirito | 6 (.188) | 2 | 0 | 0 | 3 | 1 |
| Santa Maria Novella | 9 (.281) | 4 | 2 | 1 | 1 | 1 |
| Santa Croce..... | 6 (.188) | 3 | 1 | 0 | 2 | 0 |
| Total | 32 | | | | | |
| Albizzi + Guadagni marriage strategies: | | | | | | |
| San Giovanni..... | 13 (.591) | 2 | 5 | 5 | 0 | 1 |
| Santo Spirito | 4 (.182) | 2 | 0 | 1 | 1 | 0 |
| Santa Maria Novella | 3 (.136) | 3 | 0 | 0 | 0 | 0 |
| Santa Croce..... | 2 (.091) | 0 | 1 | 0 | 0 | 1 |
| Total | 22 | | | | | |

NOTE.—Values in parentheses are percentages of total.

tion procedure, table 12 contains measurement error, which will be reduced through ongoing archival work. For our purposes, however, these best estimates are sufficient to reveal broad trends in strategic behavior of the elite.

Table 12 presents, for the leading elite families in various quarters, the number of their marriages into families in all other quarters (including their own), broken down by decade. The Medici lived in the western portion of San Giovanni; the rival but intimately connected Albizzi and Guadagni lived in the eastern half. The Peruzzi, Ricasoli, and Castellani were leaders of Santa Croce, as already mentioned. And the Strozzi and

Gianfigliuzzi were the most prominent families in Santa Maria Novella. No family in Santo Spirito had a volume of marriages high enough to warrant inclusion in this table.

If one looks first at the overall figures, what is most striking about these data is the fact that all of the most prestigious oligarch families intermarried at the highest rate with patrician families in San Giovanni—the Medici's own homeland. We have already established that the Medici themselves married at very low rates into their own neighborhood. We now have a better sense of why: they were surrounded by patrician enemies (Kent 1978, p. 170) whom the oligarchs already had successfully wired into themselves. The more detailed timing data also support this conclusion: the oligarchs were active in the 1395–1420 period in establishing marriage links with San Giovanni, during which time the Medici made not a single marriage (in our data set at least). Apparently, the oligarchs were quite aware of the potential for trouble from this quarter (also home to the Ricci) and took active steps to contain it at its source.

The case of the Rondinelli, mentioned above as a swing vote, illustrates the oligarchs' strategy more precisely. In the 1340s the Medici, the Rondinelli, and the della Stufa had formed a San Giovanni clique, apparently of the traditional sort (Kent 1978, p. 65). As late as the 1370s, the Rondinelli were active, along with a portion of the Medici, in the Ricci's San Giovanni faction (Brucker 1977, pp. 34, 125). Hence, the Rondinelli were prime candidates for neighborhood intermarriage with the Medici and for pariah status. However, an early wife-giving marriage with the Strozzi (themselves split between Albizzi and Ricci factions), followed by both types of marriage with the Guasconi (who themselves furnished an early wife to the Strozzi) apparently saved them from ostracism and turned them into avid oligarchs. In this manner, the Strozzi dissolved the Medici's local patrician clique.

In addition to such innovative cross-neighborhood raiding into the Medici's base by the Santa Maria Novella and the Santa Croce elites, the data reveal that the Medici's San Giovanni heartland was being squeezed in a traditional way. For reasons we do not know, from 1410 on, the old-line Albizzi and Guadagni retreated to an old-fashioned but successful neighborhood consolidation strategy. This retreat made them less integrated into the elite than one might suspect from their stature (cf. Kent 1978, p. 177). But it also made it harder for the Medici to rebuild their own local patrician base.

So far, the data are consistent with our expectations. In addition to this (albeit loose) confirmation, however, the data contain a surprise. The oligarchy worked so hard on controlling San Giovanni that they apparently overlooked the "older suburb" quarter of Santo Spirito (that

is, until the 1430s). Perhaps this was a simple and traditional oversight on their part, based on the slightly less prestigious character of this district; but we suspect that the overemphasis on San Giovanni, relative to Santo Spirito, was at least partly a conscious allocation of scarce marriage resources.⁶³

In any event, this oversight represented a clear “structural hole” within the oligarchic marriage network, which the data reveal the Medici to have exploited—at first gradually, but then with a vengeance, as they incrementally but clearly learned about the political potential in Santo Spirito: by the early 1430s, 100% of the Medici’s own marriages were directed to this quarter. Three of every four of these late Medici marriages were wife-receiving rather than wife-giving—an indication that the Medici by this point were not particularly picky about relative status claims. One of these wife-receiving marriages was with the Guicciardini, a family fairly well integrated into the elite, in order to reinforce an old marriage alliance under siege.⁶⁴ But all the others (with the Corbinelli, the Corsini, and the Ridolfi) fit the classic Medicean pattern of marriage to patrician structural isolates.

The oligarch families apparently were not fools, and so moved quite late to repair this breach in their own defenses. As was the oligarchs’ wont, all of these Santo Spirito marriages in the 1430s were to old-line families, already well integrated into the Florentine marriage network, albeit not at the highest levels. Thus, the late but intense mobilization of Santo Spirito patricians, by both sides, followed in microcosm the same pattern as that found in the global blockmodel—successful Medicean mobilization of structurally isolated patricians, and successful oligarchic co-optation of fairly well integrated patricians.

It may be of more general interest to pause here for an observation about the apparent microbehavioral decision processes revealed in these data. This sequence of structural holes, created in the first place by active elite attention focused elsewhere and then exploited gradually but surely by opponents, suggests more than anything that elite tactics evolve not as part of interacting sets of omniscient “grand strategies,” à la game

⁶³ Santo Spirito, located across the Arno river (on the “other side of the tracks”), originally was more like an old suburb than a part of the city’s geographical core (Weissman 1982, p. 6). It was the heart of the Ciompi wool workers’ district. This hardly implied that the quarter contained nothing but workers and new men—after all, many old magnate families resided there. But it does imply that the elite of this quarter were somewhat less densely integrated by marriage (though not economics) into themselves and into other neighborhood elites.

⁶⁴ The Peruzzi also arranged a marriage with the Guicciardini, in 1434, in an apparent effort to induce the Guicciardini to switch sides. The net effect of these offsetting moves was a Guicciardini family split in its partisan loyalties.

theory, but rather as a mutually adaptive learning process (Lave and March 1975).

Florentine families were extremely shrewd and opportunistic, but nonetheless they were always engaged in boundedly rational local action dictated by their own deep embedding through networks in localistic and highly idiosyncratic circumstances. Any complicated and endogenous marriage system presents each actor with narrow and changing opportunities, derived like vacancy chains (White 1970) from the actions and inactions of others in other arenas. Yet no one, not even Cosimo de' Medici, possessed the clear global overview of figure 2a. Everyone looked out at elite structure egocentrically from the vantage point of his or her own network location. This conclusion is not to doubt that families learned, and often shrewdly so, but to question the plausibility of historical explanations that place much weight on Machiavellian foresight and planning, especially in tumultuous times (cf. Skocpol 1979, pp. 14–18).

The Medici's own structurally anomalous position.—Finally, we need to explain: Given a global predisposition to form parties, why the Medici in particular as leaders of that party? After all, given the success of their opponents' control strategy against them, and especially given the depth of their involvement in previous discredited regimes,⁶⁵ the Medici emergence to take over the state in 1434 would seem, from the perspective of 1400, to be little short of miraculous.

One possible explanation might be the Medici bank, founded on ties with the pope. But this is not convincing. As a "cash cow" generator of personal wealth, which the Medici used both for personal patronage (Molho 1979) and for city loans (Molho 1971, p. 168), the role of the bank is clear. However, an identical economic position did not save the Alberti, who were the pope's primary bankers before their exile. And the Medici bank as an institution (not just as a generator of wealth) was useless for political mobilization: apart from the fact that not many fami-

⁶⁵ Salvestro di Alamanno de' Medici was the forceful, and somewhat demagogic, leader of the campaign against the Parte Guelfa that triggered the Ciompi revolution (Brucker 1962, chap. 8). For this role, and his subsequent leadership, he was the Ciompi's hero. On the very day of their street triumph, as patrician houses burned, the Ciompi "mob" knighted Salvestro for his services to the *popolo*. Other Medici followed in Salvestro's footsteps, through their heavy involvement with the Alberti faction. In 1397, shortly after the 1393 downfall of the Alberti, Bastardino de' Medici (along with Maso de' Ricci) was executed for attempting to assassinate Maso degli Albizzi and Rinaldo Gianfigliuzzi and thereby to overthrow the regime (Brucker 1977, p. 100). In 1400, the Medici were involved in another such conspiracy and assassination attempt, with the result that the entire Medici family (along with the Alberti and Ricci) were excluded from holding public office for 20 years. Actions more anathema to the oligarch regime can hardly be imagined.

lies were involved, the bank was heavily infiltrated with oligarchic partners and employees (particularly the Bardi, but also the Guasconi and della Casa), later purged (de Roover 1966, p. 56).

A better answer, we believe, has to do with the Medici's own anomalous or structurally contradictory position in the patrician networks. They were the only major Florentine family to span the structural chasm created by elite closure and to participate simultaneously in the two disarticulated patrician worlds. (Refer to Medici links to the Guasconi and Albizzi blocks in figure 2a.) To understand this oddity, we need more information on the Medici's own, rather peculiar, family history.

Brucker's article (1957) on the 14th-century Medici makes it clear that the Medici were hardly a mercantile banking family throughout most of their history. On the whole, they were a rather violent and lawless bunch—*popolani* imitating magnates. Only Vieri di Cambio engaged in banking, and even then the Medici economic fortunes did not really begin to rise until Giovanni de' Medici used the remnants of Vieri's firm to capture the pope's business from the discredited Alberti (Holmes 1968). Perhaps in part because of his relative Salvestro's Ciompi exploits (see n. 65), Giovanni himself assiduously avoided politics throughout his lifetime (1360–1429) and tried to talk his sons, Cosimo and Lorenzo, into the same course (Machiavelli 1988, p. 161).

More particularly, Machiavelli describes (on the basis of earlier chronicles) two incidents that may account for how the Medici avoided utter, rather than just heavy, ostracism after 1382.

In 1393, immediately after the Alberti were finally banished by the Albizzi oligarchs, a crowd of guildsmen and *popolo minuto* ran to the house of Vieri di Cambio, who was left head of the Medici clan after the death of Salvestro in exile in 1388. According to Machiavelli (1988, pp. 139–140), this crowd implored Vieri in the name of Salvestro to lead a pro-Alberti revolt to topple the oligarchic regime. Vieri's kinsman Antonio also urged Vieri to commit the Medici to immediate revolt. However,

Going among them in the piazza and from there into the palace, he said before the Signoria that he could not in any mode regret having lived in such a manner that the people of Florence loved him, but that he regretted very much the judgment that had been made of him, which his past life did not deserve. . . . He therefore begged the Signoria that the ignorance of the multitude not be imputed to his sin because, as far as he was concerned, as soon as he could he had put himself in [the Signoria's] power. . . . After these words, Messer Veri returned to the piazza and joined his followers with those who had been led by Messer Rinaldo [Gianfigliuzzi] and Messer Donato [Acciaiuoli]. . . . He begged [the mob] to put down their arms and obey the Signoria [which they did]. [Machiavelli 1988, p. 140]

Much later, in 1426, Giovanni de' Medici behaved in much the same way, although a change in who initiated the action reveals how far roles had altered in the interval. Rinaldo Albizzi came to Giovanni to ask him to join his faction of oligarchs in their efforts to disenfranchise new men (about which more below). He refused, saying that he opposed factions of any sort.

These things, so dealt with, were learned of outside and brought more reputation to Giovanni and hatred to the other citizens. But Giovanni sought to detach himself from this so as to give less spirit to those who might plan new things under the cover of his favor. . . . Many of those who followed his part were malcontent at this because they would have liked him to show himself more active in things. Among these was Alamanno de' Medici [son of Salvestro], who was fierce by nature and did not cease inciting him for his coldness and for his mode of proceeding slowly, which, he said, was the cause of his enemies' dealing against him without respect. . . . Inspiring him also in the same way was his son Cosimo. Nonetheless, Giovanni . . . did not budge from his position. [Machiavelli 1988, p. 156]

In other words, throughout the decades following the Ciompi revolt, the leaders of the Medici clan tried hard to distance themselves from their more fractious relatives and to reestablish their family within the conservative oligarchic regime. Not only that, they went out of their way to keep the lid on trouble. As the Machiavelli accounts make clear (see also Martines 1963, p. 55), this docile political behavior did not expunge the pro-new-men image that the Medici name had in the popular mind. But, for their role in squelching discontent, the oligarchs apparently were begrudgingly grateful.

Even so, it was not until the 1420s that the oligarchs relented and began to co-opt the Medici through marriage cycles. Except for marriages with the Bardi, all of the marriages between the Medici and the oligarch blocks in figure 2a were recent.⁶⁶ This readmission of the Medici into elite circles was just what the cautious Giovanni had struggled for all his life. But what would have worked splendidly with the older generation of Vieri and Giovanni was too late for the fractious Young Turks Cosimo and Lorenzo di Giovanni and their cousin Averardo di Francesco. Events sketched in the next section led the die to be cast.

Thus, we lay at Vieri di Cambio's doorstep the explanation of the structurally anomalous position that the Medici house held in 15th-

⁶⁶ The early-1420s period of this co-optative marriage behavior is quite narrow, so the volume of evidence is not overwhelming. But what we have in mind here are the 1420s wife-giving marriages with Luca di Maso Albizzi (after the death of his father), with the Gianfigliuzzi, and with the Barbadori—in other words, with the Albizzi block of figure 2a. In addition, there was a wife-receiving marriage with the Guasconi.

century patrician marriage networks. Giovanni followed up Vieri's abject plea with exemplary circumspection, but it was the defusion of popular rebellion by Vieri di Cambio, we argue, that saved the Medici name in oligarch eyes.⁶⁷

Even so, oligarchic acceptance of Giovanni de' Medici, however delimited, suspicious, and constrained, did not occur without a debate. "Niccolo da Uzzano did not fail to alert the other citizens [about the political inclusion of Giovanni di Bicci] by pointing out how dangerous it was to foster one who had such a reputation in the generality of people, and how easy it was to oppose disorders in their beginnings, but how difficult it was to remedy them when they were left alone to increase; and he recognized in Giovanni many parts superior to those of Messer Salvestro. Niccolo was not heeded by his peers because they were envious of his reputation and desired to have partners in defeating him" (Machiavelli 1988, p. 148).

The Dynamics of Party Formation: New Men Economic Ties

We shall now sketch, in exceedingly brief compass because of space limitations, the 1420s and 1430s Medici mobilization, through economic ties, of the second half of their party—the San Giovanni new men. In this reconstruction, we rely heavily on hard-earned data and interpretation in Molho (1971), Brucker (1977), and Kent (1978). The Milan and Lucca wars of 1424–33, we shall argue, were the short-term catalysts that galvanized San Giovanni new men into support and triggered, thereby, self-consciousness of the Medici as party. The argument in brief is as follows:⁶⁸

The Milan and Lucca wars triggered tax extraction of such magnitude that entire family patrimonies, both of patricians and of new men, were being destroyed. This set off a frantic scramble, among everyone, to escape ruinous tax assessment.

Taxes were levied administratively by neighborhood; thus, fiscal crisis revived the politics of neighborhood. Neighbors looked to neighbors to gain leverage on other neighbors who allocated assessments through rotating offices.

⁶⁷ Further evidence of this is the fact that only the descendants of Vieri were exempted from the oligarchs' blanket proscription of the Medici clan in 1433 (Kent 1978, p. 295).

⁶⁸ Readers interested in documentation of the claims herein can write the authors for an earlier draft of this paper. We have chosen to respond to page limitations by drastically shortening this section on new men, because the argument in this section is primarily a synthesis of secondary sources.

Administrative assessment procedures could more easily uncover fixed assets like real estate than liquid assets like cash and business investments. Patricians went on the legislative offensive to squeeze more offices and taxes out of mercantile new men.

New men responded initially by banding together in parish-based secret societies: the religious confraternities. While not class homogeneous, this corporate form was a first step toward organized class solidarity and resistance.

Oligarchs responded with vicious and successful repression. Abolition of confraternities left new men bereft of coordinated local neighborhood support from each other.

New men responded in the only way left: supplication of local neighborhood patrician patrons for help. But successful mobilization of patricians for class repression had locked in most oligarchs against responding to appeals from below. The enforcement mechanism behind this coordinated class rejection of new men was the dense marriage network analyzed above.

The Medici were the only exception: their structurally contradictory position within elite marriage networks gave Giovanni and Cosimo the truly discretionary choice of responding or not to pleas from San Giovanni new men. Giovanni de' Medici, late in his life, responded to Rinaldo Albizzi's explicit request to join him in repression in the manner described in the quotation from Machiavelli above. In the sharply polarized context of the time, the hotheaded Rinaldo probably took Giovanni's equivocal response as throwing down the Ciompi (or at least new men) gauntlet. Henceforth (after 1426), the Medici family was severed from budding oligarchic co-optation and was now afloat as a distinct political faction.

San Giovanni new men received patronage, through economic ties, from their neighbors the Medici, but other new men did not, from the Medici or any other source.⁶⁹

Therefore, we believe that the surge of supplication from San Giovanni new men during the Milan war is what triggered Medici self-consciousness of themselves as a political party (as opposed to just an outcast family struggling desperately to regain admittance to the club). Oligarchs funneled new men's support to them and then cut off any possibility of equivocating response. Certainly, the first evidence of intentional and coordinated Medici manipulation of elections dates from 1427, right after this period.

⁶⁹ Hence, the 1434 peak of newly admitted families is only one-fourth as large as the historical average. See fig. 1.

NETWORK IDENTITIES

Robust Action

With these analyses of Medicean party dynamics in hand, we are now in position to understand the structural preconditions for robust action. We can only assume that Cosimo learned sphinxlike behavior at his father Giovanni's knee, and then adapted that to revolutionary circumstances. The strategy certainly was not unknown in Florence (Weissman 1989). But robust action is not just a matter of behaving ambiguously. Others are too shrewd not to see through behavioral facades down to presumed self-interested motivations. To act credibly in a multivocal fashion, one's attributed interests must themselves be multivocal.

Within the Medici party itself, the structural underpinnings for contradictory attributions of "Medici self-interest" are clear. The Medici were heroes of the new men not just because they inherited the historical legacy of Salvestro d'Alamanno. They did sponsor a few San Giovanni families of new men, quite unlike their oligarch opponents. And, after Giovanni spurned Rinaldo Albizzi's class alliance request, oligarchs heaped the scornful opprobrium of that label on Medici heads in a polemical effort to whip up patrician fury. At the same time, the Medici's deep patrician roots were clear enough to those whom they married. One can easily imagine Medicean patrician supporters knowingly winking at each other on hearing the public charges of favoring the new men that swirled around their in-laws. Such patricians' hatred of the oligarchs ran deep, and "everyone knows that tactical alliances are sometimes necessary to reestablish an old regime." After all, it would not be the first or last time in Florentine history that the new men had been sold down the river.

Which attribution was true? Plausible evidence could be assembled for either view, but new men and patrician supporters of the Medici hardly ever had the opportunity to get together privately to compare contradictory notes. Even if they had, trust between them was so low that neither should have believed a word of the other. Robust action by the Medici was credible precisely because of the contradictory character of their base of support.

Descending to more micro levels does not help to clarify attributions. Everyone knew that the Medici wanted, as bankers, to make money; as families, to increase prestige; as neighborhood patrons, to amass power. But which of the goals (which really are roles) was in play when? Given fixed role frames, self-interests (and attributions) are clear, but in complicated chaos like the Milan and Lucca wars the games themselves are all up for grabs. Rational choice requires a common metric of utility for

footing, but revealed preferences (the basis for inferring trade-offs across goals/roles) only exist post hoc.

A Medicean goal of taking over the state can be inferred from the historical record, but only after their choice already had been narrowed to that or exile. Before that, the Medici appear to have been traditional incrementalists, trying only to worm their way back into oligarchs' good graces. Yet, at the level of sphinxlike style, it is not at all clear that Cosimo and Giovanni were any different. Both were shrewd and multivocal opportunists, pursuing openings whenever they presented themselves. Clear goals of self-interest, we conclude, are not really features of people; they are Florentine (and our) interpretations of varying structures of games.

Legitimacy

Besides multivocal placement in contemporary conflict, however, there was a second dimension to Medicean robust action. Not only has Cosimo been remembered in history as a Machiavellian deep thinker, but also at his death he was legally enshrined as *pater patriae*, father of his country, by his contemporaries (cf. Schwartz 1983). The very ambiguity of his placement in self-interest somehow became elevated, in the public mind, into the essence of public interest.

This transposition from hero of the new men to Solomonian sage, we believe, can be understood by returning to those Florentines who have remained on the margins of our account so far—the political neutrals. Cosimo never beat the oligarchs in pitched battle; he was recalled from Venetian exile in public triumph, as savior of the republic.⁷⁰

The key is the cognitive category "oligarch." After all, when the oligarchs were firmly in control, they were not labeled "oligarchs"; they were republican "public citizens of the state." Loss of legitimacy and Medici victory are what got them their pejorative tag. No longer public-spirited and selfless in attribution, they came to epitomize class self-interest in Florentine eyes.

How did this delegitimizing change in attribution come to pass? Our answer, basically, is positional chess. The Medici themselves never slung "oligarch" mud back into oligarch eyes. As mentioned in the introduction, Cosimo never said a clear word in his life. Instead, others—new men and eventually political neutrals—were the agents of active tactical slashing.

The sequence went like this: As already described, the fiscal crisis of

⁷⁰ The irony, of course, is that most historians regard Cosimo as the destroyer of republicanism (Rubinstein 1966).

the wars repolarized social classes and tempted oligarchs into a successful (in the short run) repression campaign. This campaign earned them their label in new men's eyes. However, as Kent (1978) makes clear, the oligarchs' attack was not just a rash tilt at windmills, based on lack of sophisticated foresight. Oligarchs' ability to control the legislative process (especially the colleges), and therefore to attain their goals peacefully, had been hindered recently by the network tentacles of the duplicitous Medici faction. Medicean latent appeal to new men and heterogenous control of some offices gave them swing-vote influence out of proportion to their numbers—even though legislatively all they did was block. In the context of vanishing patrician patrimonies, however, mere blockage was sufficient to force oligarchs onto the offensive. Either they purged the opposition or else all their wealth would be gone.

Cosimo, in contrast, behaved in his typically reactive fashion of only responding to requests. He funneled a sizable portion of the assets of his bank into funding the state's short-term debt, for which he was rewarded with brief public office—member of the *Ufficiali del Banco*, from November 1427 to December 1428.⁷¹ The monetary catch was this: as incentive to grant such emergency loans, the state offered above-average short-term returns. Thus, while his opponents faced financial catastrophe, Cosimo actually may have made money.⁷² More to the point here, once Florence survived, Cosimo de' Medici took on the appearance of financial savior of the city (Molho 1971).

Knowing this, once the Lucca war had ended, the oligarchs moved quickly to send Cosimo, Lorenzo, and other Medici and lieutenants into exile in Venice (and elsewhere). In anticipation of this very move, Cosimo had shifted much of his wealth out of the city, away from potential expropriation, and used it to cultivate Venetian support (de Roover 1966, p. 54; Kent 1978, pp. 304–8). Oligarchic legitimacy was now in deep trouble. They still did not have enough control to purge sufficiently the Signoria electoral bags,⁷³ so, when the lottery randomly produced too many Medicean officeholders (Kent 1978, p. 328), the oligarchy was forced to take desperate action. Rinaldo Albizzi sent out the word to

⁷¹ Even after Cosimo left this post, his supporters continued to dominate this financial nerve center (Molho 1971).

⁷² A number of oligarchs possessed as much, or almost as much, raw wealth as did Cosimo, but theirs often was not as liquid as his—a consequence of Cosimo's special ties to the pope.

⁷³ The Florentines had an elaborate electoral system that involved electing candidates to bags, one bag for every class of offices. Offices were filled every two months by randomly drawing names from the bags and checking these against various legal restrictions. The whole republican purpose of the system was to make it hard for any one faction to consolidate control. See Najemy (1982).

assemble the troops in order forcibly to seize the Signoria, with the stochastic turnout consequences described above. Political neutrals joined the cry for Medici return, and Cosimo became in public acclamation the political as well as financial savior of Florence.

War of course was the *sine qua non*, but note that at each step of the way Cosimo's careful positional maneuvering forced or enticed oligarchs into offensive lines of action, which connoted private self-interest. These were clear, irrevocable, and hence foreseeable. The reactive character of his robust and multivocal actions gained for Cosimo the revolutionary the legitimizing aureole of protector of the status quo. Party transmuted into state.

CONCLUSION

We shall not summarize the arguments in this paper, except for this: state centralization and the Renaissance emerged from the grinding of tumultuous historical events, as these were filtered through elite transformation. Cosimo did not create the Medici party, but he did shrewdly learn the rules of the networks around him. Rather than dissipate this power through forceful command, Cosimo retreated behind a shroud of multiple identities, impenetrable to this day. These credibly imparted multivocal meanings to all his reactive actions. Robust discretion, in the face of unpredictably hostile futures, and Solomonic legitimacy, above the self-interested fray, were the intended or unintended (who knows?) consequences.

We close on this methodological note: to understand state building, we have argued, one needs to penetrate beneath the veneer of formal institutions and apparently clear goals, down to the relational substratum of people's actual lives. Studying "social embeddedness," we claim, means not the denial of agency, or even groups, but rather an appreciation for the localized, ambiguous, and contradictory character of these lives. Heterogeneity of localized actions, networks, and identities explains both why aggregation is predictable only in hindsight and how political power is born.

APPENDIX A

Blockmodeling Methods

We adopt correlation as our operational measure of structural equivalence, just as in CONCOR (White, Boorman, and Breiger 1976). A frequently used alternative measure, Euclidean distance (Burt 1976), gives weight to volume of ties as well as to pattern of ties (Faust and Romney

1985). In the current context, in which parties and class crosscut, this weighting unhelpfully differentiates elite from nonelite rather than one party from another. Unlike CONCOR, however, we prefer agglomerative to divisive clustering.⁷⁴

Thus, our method is to correlate columns of “stacked” matrices (and transposes), across all strong tie networks, and then to input the resulting correlation matrix into the standard Johnson’s complete-link clustering algorithm.⁷⁵ This produced the partition of families given in Appendix B. To get the structural portraits of figures 2a and 2b, blockmodel images of social bonds among clusters were generated by (a) aggregating each raw network matrix according to Appendix B’s clustering of families and then (b) defining and drawing a social “bond” whenever the number of raw network ties between clusters equaled or exceeded two.⁷⁶

Tables A1 and A2 address goodness-of-fit issues. There are two such issues: How well do the simplified blockmodels represent the more complex actual network data? How well does the blockmodel partition predict actual party memberships, as recorded in Kent? The first issue is assessed, in table A1, in two ways: (a) by the percentage of actual ties among families represented in figure 2’s image bonds among blocks and (b) by correlations between raw data matrices and block mean densities, based on the partitions in Appendix B (Noma and Smith 1985).

All correlations in tables A1 and A2 are significant at the $P < .001$ level, according to the QAP procedure of Baker and Hubert (1981).⁷⁷

⁷⁴ The reason is the two procedures’ differing treatments of structural isolates. We prefer to inhibit these “irrelevant” actors from contaminating our structural picture of relations within the core. (This does not mean that information on ties between central and isolated families is ignored in the discovery of the core. Such information already has been incorporated into the original correlation measure.)

⁷⁵ In addition to this computer output, we did one manual correction: we merged the Capponi/Busini block with the Strozzi solo block. This merger improved (marginally) goodness of fit. More important, Kent (1978, p. 184) makes a strong case for the structural equivalence of Strozzi and Capponi. In addition, it was clear (from comparing D. Kent [1978] to F. W. Kent [1977]) that we possessed only limited marriage and economic data on Capponi.

⁷⁶ Often, density percentages are used as cutoff criteria, instead of raw number of ties, as is the case here. The reason for our approach is that, with small block sizes, percentages are not robust: a single actual tie can mean as much as a 50% density.

⁷⁷ In the nonparametric QAP procedure (Baker and Hubert 1981), an empirical distribution of random correlations is generated by repeatedly permuting rows and columns of the raw data matrix and then correlating this with the fixed image matrix. In no case, out of about 1,000 trials, did any of our own simulated random correlations exceed .10. More formal parametric significance tests are rarely possible with network data, since the data grossly violate the “independent observations” assumptions of traditional tests.

TABLE A1

GOODNESS OF FIT OF BLOCKMODEL IMAGE TO RAW NETWORK DATA

| TYPE OF RELATION | OVERALL | | | MEDICI | |
|---------------------|-------------|-----------------------------------|------------------------------------|-------------|-----------------------------------|
| | No. of Ties | Proportion of Ties in Image | Correlation with Block Means | No. of Ties | Proportion of Ties in Image |
| Marriage | 161 | .646 | .492 | 28 | .964 |
| Trade | 92 | .674 | .561 | 10 | .800 |
| Partnership | 45 | .556 | .513 | 13 | .846 |
| Bank | 13 | .769 | .415 | 10 | .800 |
| Real estate | 22 | .091 | .378 | 5 | .400 |
| Subtotal* | 333 | .610 | | 66 | .848 |
| Mallevadori | 31 | .194 | .338 | 3 | .000 |
| Friendship | 50 | .360 | .381 | 14 | .714 |
| Personal loan | 89 | .404 | .408 | 25 | .840 |
| Patronage | 36 | .222 | .389 | 21 | .381 |
| Subtotal* | 206 | .330 | | 63 | .619 |
| Total | 539 | .503 | | 129 | .736 |

* The blockmodel partitions were derived from data on the first five of these relations only. Goodness-of-fit measures for the last four relations derive from the superposition of best-fitting partitions from the first five relations. All correlations are significant at the $P < .001$ level, according to the nonparametric QAP procedure (Baker and Hubert 1981), implemented in UCINET.

TABLE A2

GOODNESS OF FIT OF BLOCKMODEL PARTITIONS TO PARTY MEMBERSHIP

| No. of Families | Medici Blocks | All Other Blocks | Elite Blocks | Nonelite Blocks |
|-----------------------------|------------------|---------------------|-----------------|--------------------|
| Mediceans | 26 | 4 | 2 | 2 |
| Split loyalty | 2 | 10 | 6 | 4 |
| Oligarchs | 0 | 31 | 8 | 23 |
| Neutrals | 1 | 18 | 2 | 16 |
| Proportion mobilized* | .966 | .714 | .889 | .644 |
| Proportion own party† | .958 | .823 | .737 | .871 |

* % Mobilized measures relative recruitment of blocked families into any party; i.e., proportion mobilized = (no. of families - no. of neutral families)/(no. of families).

† % Own party measures relative partisanship of activists; i.e., proportion own party = (no. of families in own party)/(no. of families - no. of neutral families), with split families allocated proportionately.

Moreover, 50% of the total volume of ties,⁷⁸ 61% of marriage and economic ties (fig. 2a), and 33% of “political” and friendship ties (fig. 2b) are captured by the central tendencies in the blockmodel. Given that the blockmodel was derived from the strong tie marriage and economic relations alone, we were pleasantly surprised by the weak tie performance.

More particularly, it is clear that marriage and trading relationships are the primary driving forces behind this blockmodel portrait of the Florentine elite. In part, this is because of their high rates of inclusive success, but it is also due to the higher volume of data. Partnerships and especially bank employment relations were very important when they appeared, but they do not span much of the elite. Real estate relations essentially were irrelevant.

On the weak tie side, personal friendships and personal loans operated in large part within the framework of marriage and economic relations. *Mallevadori* and patronage relations, however, were not well predicted by marriage and economic ties.

The success with which the blockmodel predicts political partisanship, in the table A2, speaks for itself. Ninety-three percent of the families in the Medicean blocks were mobilized into the Medici party. Fifty-nine percent of the families in the non-Medicean blocks were mobilized into the oligarch party.

⁷⁸ Considering that this blockmodel has been disaggregated to an average of 2.7 families per block, and considering that the average overall density of the nine historical networks here is an extremely sparse 0.7%, this is excellent performance.

APPENDIX B

TABLE B1

BLOCK MEMBERSHIPS: CLUSTERING OF CORRELATIONS OF MARRIAGE, TRADE,
PARTNERSHIP, BANK, AND REAL ESTATE RELATIONS

| <i>Reggimento</i> Family | Party* | Gross Wealth (Florins) | Date of First Prior | Neighborhood (<i>Gonfalone</i>)† |
|-----------------------------|-----------------------|------------------------------|------------------------|---------------------------------------|
| Medicean blocks: | | | | |
| MEDICI: | | | | |
| Medici..... | Medici (5) | 199,672 | 1291 | 41 |
| Carnesecchi..... | Medici | 42,316 | 1297 | 42 |
| Berlinghieri..... | Medici | 6,117 | 1365 | 22 |
| TORNABUONI: | | | | |
| Tornabuoni/ | | | | |
| Tornaquinci..... | Medici | 121,310 | Magnate | 34 |
| Salviati..... | Split loyalties (1/1) | 29,964 | 1297 | 24 |
| Serristori..... | Medici | 56,675 | 1392 | 23 |
| Giugni..... | Medici (3) | 41,086 | 1291 | 24 |
| Pecori..... | Medici | 17,244 | 1284 | 42 |
| Corsini..... | | 16,387 | 1290 | 13 |
| Vecchietti..... | Medici | 17,212 | Magnate | 34 |
| GUICCIARDINI: | | | | |
| Acciaiuoli..... | Medici | 28,200 | 1282 | 31 |
| Guicciardini..... | Medici | 60,060 | 1302 | 12 |
| Ridolfi..... | Medici | 46,196 | 1290 | 13 |
| Pitti..... | Medici (2) | 9,676 | 1283 | 12 |
| Corbinelli..... | Medici | 58,955 | 1286 | 12 |
| GINORI: | | | | |
| Ginori..... | Medici (3) | 34,831 | 1344 | 41 |
| Martelli..... | Medici (8) | 7,502 | 1343 | 41 |
| DIETISALVI: | | | | |
| Dietisalvi..... | Medici (2) | 3,943 | 1291 | 41 |
| Ciai..... | Medici | 22,331 | 1389 | 41 |
| DALL'ANTELLA: | | | | |
| dall'Antella..... | Split loyalties (1/2) | 18,437 | 1282 | 21 |
| Bartolini..... | Medici | 19,477 | 1299 | 42 |
| ORLANDINI: | | | | |
| Orlandini..... | Medici | 11,012 | 1420 | 42 |
| Lapi..... | Medici | 5,303 | 1394 | 44 |
| DAVANZATI: | | | | |
| Davanzati..... | Medici (2) | 19,887 | 1320 | 32 |
| COCCO-DONATI: | | | | |
| Cocco-Donati..... | Medici (2) | 2,580 | 1376 | 22 |
| Arnolfi..... | Medici | 4,160 | 1318 | 42 |
| Pandolfini..... | Medici | 30,520 | 1381 | 43 |
| VALORI: | | | | |
| Valori..... | Medici | 15,213 | 1322 | 43 |
| del Benino..... | Medici | 22,629 | 1345 | 13 |
| Non-Medicean blocks: | | | | |
| ARDINGHELLI: | | | | |
| Ardinghelli..... | Oligarch | 57,596 | 1282 | 32 |
| da Panzano..... | Split loyalties (1/1) | ... | 1312 | 22 |

TABLE B1 (Continued)

| <i>Reggimento Family</i> | <i>Party*</i> | <i>Gross Wealth (Florins)</i> | <i>Date of First Prior</i> | <i>Neighborhood (Gonfalone)†</i> |
|------------------------------|-----------------------|---------------------------------------|--------------------------------|--------------------------------------|
| DA UZZANO: | | | | |
| da Uzzano..... | | 75,737 | 1363 | 11 |
| Bucelli | Oligarch | 20,394 | 1284 | 22 |
| GUASCONI: | | | | |
| Guasconi | Oligarch (3) | 29,074 | 1314 | 41 |
| Bardi | Split loyalties (3/5) | 189,452 | Magnate | 11 |
| Cavalcanti | Split loyalties (1/1) | 50,122 | Magnate | 31 |
| Pazzi..... | Medici | 72,550 | Magnate | 43 |
| RONDINELLI: | | | | |
| Rondinelli | Oligarch | 21,053 | 1296 | 41 |
| Brancacci..... | Oligarch (4) | 15,697 | 1317 | 14 |
| Mancini..... | | 6,838 | 1284 | 22 |
| ALDOBRANDINI: | | | | |
| Aldobrandini | Oligarch | 7,171 | 1307 | 34 |
| Raugi..... | Oligarch | 3,634 | 1304 | 21 |
| PERUZZI: | | | | |
| Peruzzi | Oligarch (8) | 104,795 | 1283 | 23 |
| Ricasoli | Oligarch (2) | 36,178 | Magnate | 21 |
| degli Agli | | 9,402 | Magnate | 42 |
| CASTELLANI: | | | | |
| Castellani..... | Oligarch (5) | 61,696 | 1326 | 21 |
| Spini..... | Oligarch | 39,553 | Magnate | 32 |
| Fagni | | 10,106 | 1295 | 23 |
| PEPI: | | | | |
| Pepi..... | Oligarch | 2,865 | 1301 | 23 |
| Doffi..... | Oligarch | 12,700 | 1393 | 22 |
| Morelli | | 27,535 | 1387 | 23 |
| SCAMBRILLA: | | | | |
| Scambrilla..... | Oligarch (2) | 148 | 1387 | 21 |
| Sertini..... | | . . . | 1376 | 34 |
| BENIZZI: | | | | |
| Benizzi | Oligarch (2) | 9,672 | 1301 | 12 |
| Manelli..... | | 16,421 | Magnate | 12 |
| STROZZI: | | | | |
| Strozzi..... | Oligarch (4) | 296,250 | 1283 | 33 |
| Capponi | Medici | 54,027 | 1287 | 12 |
| Busini | | 57,019 | 1345 | 23 |
| RUCELLAI: | | | | |
| Rucellai | | 54,968 | 1302 | 33 |
| Baldovinetti | Oligarch (2) | 9,831 | 1287 | 31 |
| Sacchetti | | 29,092 | 1335 | 22 |
| PANCIATICHI: | | | | |
| Frescobaldi | Oligarch | 28,898 | Magnate | 12 |
| Panciatichi | Oligarch | 151,542 | Consular | 42 |
| Manovelli | Oligarch | 13,438 | 1283 | 42 |
| ALBIZZI: | | | | |
| Albizzi | Split loyalties (1/2) | 92,599 | 1282 | 43 |
| Gianfigliazzi | Split loyalties (1/5) | 47,853 | Magnate | 32 |
| Barbadori | Split loyalties (1/2) | 98,663 | 1295 | 12 |
| Belfradelli | Oligarch (2) | 9,014 | 1321 | 12 |
| Bencivenni | Split loyalties (1/1) | 1,811 | 1389 | 43 |

TABLE B1 (Continued)

| <i>Reggimento</i> Family | Party* | Gross Wealth (Florins) | Date of First Prior | Neighborhood (<i>Gonfalone</i>)† |
|-------------------------------|-----------------------|------------------------------|------------------------|---------------------------------------|
| ALTOVITI: | | | | |
| Altoviti..... | Oligarch (2) | 42,357 | 1282 | 31 |
| del Palagio | | 8,676 | 1328 | 43 |
| Corsi..... | Split loyalties (1/3) | 26,588 | 1354 | 23 |
| DELLA CASA: | | | | |
| della Casa | Split loyalties (1/1) | 31,069 | 1393 | 42 |
| Adimari..... | | 45,689 | Magnate | 44 |
| Serragli | | 63,866 | 1325 | 14 |
| SOLOSMEI: | | | | |
| Solosmei | Oligarch (2) | 5,757 | 1364 | 41 |
| LAMBERTESCHI: | | | | |
| Lamberteschi..... | Oligarch (2) | 52,524 | Consular | 21 |
| Baronci | Oligarch | 12,251 | 1330 | 42 |
| VELLUTI: | | | | |
| Velluti..... | Split loyalties (1/1) | 22,372 | 1283 | 12 |
| Arrigucci | Oligarch | 5,736 | Magnate | 42 |
| BARONCELLI: | | | | |
| Baroncelli | | 67,966 | 1287 | 21 |
| Rossi..... | Oligarch | 24,649 | Magnate | 44 |
| GUADAGNI: | | | | |
| Guadagni..... | Oligarch (5) | 25,179 | 1289 | 43 |
| BISCHERI: | | | | |
| Bischeri..... | Oligarch | 55,230 | 1309 | 44 |
| Arrighi | | 23,499 | 1373 | 43 |
| FIORAVANTI: | | | | |
| Donati..... | | 26,099 | Magnate | 43 |
| Scolari..... | | 12,074 | Magnate | 43 |
| Fioravanti..... | Medici | 19,501 | 1344 | 43 |
| Miscellaneous (unblocked): | | | | |
| Carducci..... | Medici (2) | 28,909 | 1380 | 31 |
| Fortini | | 30,645 | 1386 | 43 |
| del Forese | Oligarch | 4,220 | 1296 | 24 |
| Bartoli | Oligarch | 54,956 | 1345 | 32 |

* For Medici and oligarch parties, numbers in parentheses are numbers active, if more than one. For split loyalties, numbers in parentheses are, first, number of Mediceans and, second, number of oligarchs. Blank spaces indicate that there were no active partisans.

† For Santo Spirito quarter, 11 = *Scala gonfalone*, 12 = *Nicchio gonfalone*, 13 = *Ferza gonfalone*, 14 = *Drago Verde gonfalone*. For Santa Croce quarter, 21 = *Carro gonfalone*, 22 = *Bue gonfalone*, 23 = *Lion Nero gonfalone*, 24 = *Ruote gonfalone*. For Santa Maria Novella quarter, 31 = *Vipera gonfalone*, 32 = *Unicorno gonfalone*, 33 = *Lion Rosso gonfalone*, 34 = *Lion Bianco gonfalone*. For San Giovanni quarter, 41 = *Lion d'oro gonfalone*, 42 = *Drago San Giovanni gonfalone*, 43 = *Chiavi gonfalone*, 44 = *Vaio gonfalone*.

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Embeddedness and Immigration: Notes on the Social Determinants of Economic Action¹

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This article contributes to the reemerging field of economic sociology by (1) delving into its classic roots to refine current concepts and (2) using examples from the immigration literature to explore the different forms in which social structures affect economic action. The concept of social "embeddedness" provides a suitable theoretical umbrella, although in analyzing its specific manifestations, the article focuses on the concept of social capital. The various mechanisms through which social structures affect economic action are identified and categorized and their consequences, positive and negative, highlighted. The propositions that summarize the different parts of the discussion attempt to move these concepts beyond sensitizing generalities to hypothesis-like statements that can guide future research.

Recent work in economic sociology represents one of the most exciting developments in the field insofar as it promises to vindicate the heritage of Max Weber in the analysis of economic life and, by the same token, to rescue this vast area from the exclusive sway of the neoclassical perspective. Spearheaded by Mark Granovetter's (1985) critique of a pure "market" approach to economic action, the sociological perspective has been reinforced by the introduction and subsequent use of the concepts of "social capital" (Bourdieu 1979; Bourdieu, Newman, and Wocquant 1991; Coleman 1988), the emphasis on the predictive power of contextual variables in addition to individual characteristics (Wellman and Wortley 1990), and extensive research on the structure and dynamics of so-

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cial networks (Marsden 1990; Laumann and Knoke 1986; Mintz and Schwartz 1985; White 1970).

Granovetter's treatment of the concept of "embeddedness" represents a veritable manifesto for those whose sociological cast of mind have led them to question individualistic analyses of such phenomena as socioeconomic attainment and the culturalistic arguments that neoclassical economists sometimes invoke when their own perspective can go no further. The concept was originally coined by Karl Polanyi and his associates (Polanyi, Arensberg, and Pearson 1957) in their analysis of trades and markets but, in its more recent formulation, it has sparked renewed interest in what sociology has to say about economic life.

The purpose of this article is to contribute to this emerging perspective by (1) delving into the classical roots of recent theoretical developments so as to refine the concepts invoked by present-day economic sociologists; (2) fleshing out the concepts of embeddedness and, in particular, social capital into more specific components; (3) using the resulting typology as the basis for a series of hypotheses amenable to empirical research; and (4) showing how this theoretical program relates to the recent literature on immigration and ethnicity and can be advanced by it.

As developed so far, the concepts of the new economic sociology represent a broad programmatic statement in need of further specification. Embeddedness, for example, provides a very useful standpoint for criticizing neoclassical models, but when turned around to provide concrete propositions, it suffers from theoretical vagueness. The observation that outcomes are uncertain because they depend on how economic action is embedded does not help us meet the positivistic goals of predictive improvement and theoretical accumulation. To fulfill these goals, we must better specify just how social structure constrains, supports, or derails individual goal-seeking behavior. Our attempt to move in this direction takes two forms.

First, we try to arrive at a more systematic understanding of the different sources of what is, today, called social capital, by tracing the roots of the concept in the sociological classics. Second, we utilize contemporary research on immigration to document the operation of some of these sources and their effects, positive and negative. In keeping with the goal of theoretical specificity, our strategy is to use knowledge about immigrant economic adaptation to generate propositions of more general applicability.

The following analysis focuses on the concept of social capital, introduced in the recent sociological literature by Pierre Bourdieu and developed in English by James Coleman, since we believe it is more suitable to the enterprise of theoretical fleshing out than the more general notion of embeddedness. Coleman (1988, p. S98) defines social capital as a vari-

ety of entities with two characteristics in common: "They all consist of some aspect of social structures, and they facilitate actions within that structure." The facilitational component is highlighted by Coleman who likens "social" to "material" and "human" capitals as resources available to individuals to attain their ends. The main difference is that social capital is more intangible than the other forms, since it inheres in the structure of relations within which purposive action takes place.

Although insightful, Coleman's contribution suffers from two shortcomings: first, a theoretical indefiniteness that leaves open the question of what those social entities facilitating individual goal attainment are and where they come from; second, a marked instrumentalist orientation that views social structural forces only from a positive perspective. This positive bent sacrifices the insight (present in Granovetter's broader analysis of embeddedness) that social structures can advance as well as constrain individual goal seeking and that they can even redefine the content of such goals. We can respecify at this point the purpose of our own analysis as an attempt to further refine the concept of social capital by (a) attempting to identify its different types and sources and (b) clarifying conditions under which it can not only promote but also constrain or derail economic goal seeking.

Before plunging into this task, it is important to say a word about the source of the empirical material used in the remainder of this article. This source—immigration studies—has been frequently, albeit haphazardly, mined by writers in the theoretical literature on economic sociology. Coleman (1988), for example, uses Asian immigrant families as an illustration of what he labels "closure" of social relations, and both he and Granovetter (1985) highlight the significance of the immigrant rotating credit association as an example of either embeddedness or social capital. This frequent utilization of immigration research is not surprising, because foreign-born communities represent one of the clearest examples of the bearing contextual factors can have on individual economic action. With skills learned in the home country devalued in the receiving labor market and with a generally poor command of the receiving country's language, immigrants' economic destinies depend heavily on the structures in which they become incorporated and, in particular, on the character of their own communities. Few instances of economic action can be found that are more embedded. The task before us will be to review this empirical literature more systematically with an eye to developing propositions of general applicability.

SOCIAL CAPITAL AND ITS TYPES

The effervescence of research following the reconceptualization of economic sociology in recent years has somewhat obscured the fact that

many of these same ideas have been present all along in the sociological tradition and that they are, in a sense, central to the founding of the discipline. Our purpose here is not historical exegesis but the investigation of classic sources for clues to the various mechanisms through which social structures affect economic action. We begin by redefining social capital as those expectations for action within a collectivity that affect the economic goals and goal-seeking behavior of its members, even if these expectations are not oriented toward the economic sphere.

This definition differs from Coleman's, where the emphasis is on social structures facilitating individual rational pursuits. As we shall see below, this positive emphasis is only half of the story because the same constellation of forces can have the opposite effects. As redefined here, social capital seems sufficiently general to encompass most uses of the term in the recent sociological literature. These include Bourdieu's original formulations, as well as more specific analyses concerning the behavior of various social groups in the marketplace (Light and Bonacich 1988; White 1970; Eccles and White 1988; Bailey and Waldinger 1991). However, by its very generality, the concept encompasses such a plurality of situations as to make its empirical application difficult. Thus we must further specify what those collective expectations are, what their sources are, and how they are likely to affect economic behavior.

It is possible to distinguish four specific types of economically relevant expectations. The first, *value introjection*, takes its cue from Durkheim and a certain interpretation of Weber to emphasize the moral character of economic transactions that are guided by value imperatives learned during the process of socialization (Parsons 1937; Parsons and Smelser 1956). The central role accorded to this process in American functionalist sociology draws its inspiration from passages such as the following, from Weber's analysis of Puritan values: "One's duty in a calling is what is most characteristic of the social ethic of capitalist culture and is, in a sense, the fundamental basis of it. It is an obligation which the individual is supposed to feel and does feel towards the content of his professional activity, no matter in what it consists" (Weber [1904] 1958, p. 54).

Similarly, Durkheim's analysis of the "noncontractual elements of contract" provides powerful intellectual justification for a sociological analysis of economic transactions as reflections of an underlying moral order: "The contract is not sufficient by itself, but is only possible because of the regulation of contracts, which is of social origin. This is implicit . . . because the function of the contract is less to create new rules than to diversify preestablished rules in particular cases" (Durkheim [1893] 1984, p. 162).

Value introjection is a first source of social capital because it prompts individuals to behave in ways other than naked greed; such behavior

then becomes appropriable by others or by the collectivity as a resource. Although criticized later as an "oversocialized" conception of human action (Wrong 1961), this source remains central to the sociological perspective and figures prominently in contemporary examples of the effects of social structure on economic action (Swedberg 1991). Economists bent on dismissing the "sociological approach" to economic behavior also tend to target their criticism in this first source (Leff 1979; McCloskey and Sandberg 1971; Bates and Faulkner 1991).

The second source of social capital takes its clue from the classic work of Georg Simmel ([1908] 1955) to focus on the dynamics of group affiliation. As elaborated by exchange theorists, social life consists of a vast series of primary transactions where favors, information, approval, and other valued items are given and received. Social capital arising from *reciprocity transactions* consists of the accumulation of "chits" based on previous good deeds to others, backed by the norm of reciprocity. In contrast to the first type, individuals are not expected to behave according to a higher group morality, but rather to pursue selfish ends. The difference from regular market behavior is that transactions center not on money and material goods, but social intangibles (Gouldner 1960; Blau 1964; Hechter 1987). Nevertheless, the analogy of social capital to money capital is nowhere closer than in these exchange analyses of group life.

The third source of social capital, *bounded solidarity*, focuses on those situational circumstances that can lead to the emergence of principled group-oriented behavior quite apart from any early value introjection. Its classic sources are best exemplified by Marx's analysis of the rise of proletarian consciousness and the transformation of workers into a class-for-itself. The passages of the *Communist Manifesto* where this type of social capital makes its appearance are familiar, but no attempt at paraphrasing can do justice to the original: "With the development of industry the proletariat not only increases in number; it becomes concentrated in greater masses, its strength grows, and it feels that strength more. The various interests and conditions of life within the ranks of the proletariat are more and more equalized. . . . The collisions between individual workmen and individual bourgeois take more and more the character of collisions between two classes" (Marx and Engels [1848] 1948, pp. 17–18).

The weapon of the working class in this struggle is precisely its internal solidarity born out of a common awareness of capitalist exploitation. This emergent collective sentiment transforms what had hitherto been individual market encounters between employer and employee into a group affair where subordinates have the advantage of numbers. Starting from an analysis of pure market competition, Marx hence arrives at sociability. It is not, however, the sociability underlying the "noncontrac-

tual elements of the contract” or that arising out of Puritan values, but the defensive banding together of the losers in the market struggle. Their individual self-interests are welded together into a higher form of consciousness that, in some passages of Marx’s writings, acquires the force for social control that Weber assigned to Puritan values.

As a source of social capital, bounded solidarity does not arise out of the introjection of established values or from individual reciprocity exchanges, but out of the situational reaction of a class of people faced with common adversities. If sufficiently strong, this emergent sentiment will lead to the observance of norms of mutual support, appropriable by individuals as a resource in their own pursuits. The social dynamics at play will be illustrated in greater detail below.

The final source, *enforceable trust*, is captured in Weber’s ([1922] 1947) classic distinction between formal and substantive rationality in market transactions. Formal rationality is associated with transactions based on universalistic norms and open exchange; substantive rationality involves particularistic obligations in monopolies or semimonopolies benefiting a particular group. With substantive rationality, we are, of course, in the realm of embeddedness, because group goals govern economic behavior. The significant point, however, is that individual members subordinate their present desires to collective expectations in anticipation of what Weber designates as “utilities,” that is long-term market advantages by virtue of group membership. The process and some of the institutional structures underlying it are summarized as follows: “Relationships which are valued as a potential source of present or future disposal over utilities are, however, also objects of economic provision. The opportunities of advantages which are made available by custom, by the constellation of interests, or by conventional or legal order for the purposes of an economic unit, will be called economic advantages” (Weber 1947, p. 165).

Social capital is generated by individual members’ disciplined compliance with group expectations. However, the motivating force in this case is not value convictions, but the anticipation of utilities associated with “good standing” in a particular collectivity. As with reciprocity exchanges, the predominant orientation is utilitarian, except that the actor’s behavior is not oriented to a particular other but to the web of social networks of the entire community.

For the sake of clarity, table 1 formalizes the typology of social capital elaborated so far. The table summarizes the processes through which individual maximizing behavior is constrained in ways that lead to reliable expectations by others; under certain conditions these expectations can be appropriated as a resource. While the first two types in the table are the core of entire schools of sociological thought, the last two have been less theorized. Both depend on a heightened sense of community and

hence have the greatest affinity to the experience of immigrant groups. As the examples below will illustrate, it is the particular circumstance of "foreignness" that often best explains the rise of these types of social capital among immigrants.

The linkage is highlighted in table 1 where examples of modern analyses of the last two types are drawn from the literature on immigrant adaptation. The remainder of the article focuses on the processes underlying these two sources of social capital, seeking to formulate propositions of general applicability and to document both the positive and negative consequences of each source. The material presented is not intended to "prove" the formal propositions concluding each section, but rather to demonstrate their plausibility. As indicated at the start, our goal is to flesh out the implications of general concepts rather than to provide a definitive test of these implications.

BOUNDED SOLIDARITY

The last riot in Miami in 1989 was triggered by the shooting of two black cyclists by a Colombian-born policeman. Officer William B. Lozano was suspended without pay from the Miami police force and found himself facing the wrath of the entire black community. To defend himself against the hostile mood among much of the local population, he hired one of Miami's best criminal attorneys. As the legal bills mounted, the unemployed Lozano found that he had no other recourse but to go to the local Spanish-language radio stations to plead for help from his fellow Colombians and other Latins. Lozano had no means of verifying his claims to innocence and, as a potential felon, he should have received little sympathy from most citizens. However, he counted on the emergent feeling among Colombians that he was being turned into a scapegoat and on the growing sympathy toward that position in the rest of the Latin community. After his first radio broadcast, Lozano collected \$150,000 for his legal bills; subsequent appeals have also produced substantial sums.²

The mechanism at work in this case is labeled bounded solidarity since it is limited to members of a particular group who find themselves affected by common events in a particular time and place. As in Marx's ([1894] 1967) description of the rise of class consciousness, this mechanism

² Officer Lozano was initially convicted by a Miami jury, but an appeals court threw out the conviction on the grounds that he could not get a fair trial in the city. At the moment of this writing, the case is still pending. The venue of the retrial was first moved to Orlando and later to Tallahassee. Throughout the process, Lozano has continued to make appeals for financial support through the Spanish-language radio stations (*Miami Herald* 1990, 1991; fieldwork by authors).

depends on an emergent sentiment of "we-ness" among those confronting a similar difficult situation. The resulting behaviors are, of course, not well explained by utility-maximizing models of economic action. Instead, forms of altruistic conduct emerge that can be tapped by other group members to obtain privileged access to various resources. The fundamental characteristic of this source of social capital is that it does not depend on its enforceability, but on the moral imperative felt by individuals to behave in a certain way. In this sense, it is akin to value introjection, except that it represents the emergent product of a particular situation.

The confrontation with the host society has created solidary communities among immigrants both today and at the turn of the century. Nee and Nee (1973), Boswell (1986), and Zhou (1992) describe the plight of early Chinese immigrants in New York and San Francisco who were subjected to all forms of discrimination and lacked the means to return home. Barred from factory employment by nativist prejudice and prevented from bringing wives and other family members by the Chinese Exclusion Act, these hapless seekers of the "Mountain of Gold" had no recourse but to band together in tightly knit communities that were the precursors of today's Chinatowns (Zhou 1992). Solidarity born out of common adversity is reflected in the "clannishness" and "secretiveness" that outsiders were later to attribute to these communities. Such communities also provided the basis for the rapid growth of fledgling immigrant enterprises. Today, Chinese immigrants and their descendants have one of the highest rates of self-employment among all ethnic groups, and their enterprises are, on the average, the largest among both native- and foreign-born minorities (U.S. Bureau of the Census 1991; *Wall Street Journal* 1991).

The confrontation with the receiving society is capable not only of activating dormant feelings of nationality among immigrants but of creating such feelings where none existed before. In a well-known passage, Glazer refers to the case of Sicilian peasants coming to New York in the early 1900s whose original loyalties did not extend much beyond their local villages. These immigrants learned to think of themselves as Italian and to band together on that basis after the native population began to treat them in the same manner and to apply to them the same derogatory labels. This situation created unexpected outcomes: "Thus the American relatives of Southern Italians (to whom the Ethiopian war meant nothing more than another affliction visited upon them by the alien government to the North) became Italian patriots in America, supporting here the war to which they would have been indifferent at home" (Glazer 1954, p. 167).

Not all immigrant groups have experienced equal levels of confrontation, which accounts, in part, for the different strength of reactive soli-

darity. The cultural and linguistic distance between home country and receiving society and the distinctness of immigrants relative to the native-born population govern, to a large extent, the magnitude of the clash. A second factor critical to forging solidarity is the possibility of "exit" from the host society to return home. Immigrants for whom escape from nativist prejudice and discrimination is but a cheap ride away are not likely to develop as high levels of bounded solidarity as those whose return is somehow blocked. Turn-of-the-century Chinese immigrants offer an example of the latter situation and so did Russian Jews who came escaping Tsarist persecution at home (Rischin 1962; Dinnerstein 1977). Today, blocked return is characteristic of many political refugees and higher levels of internal solidarity have indeed been noted among such groups (Gold 1988; Forment 1989; Perez 1986). The dynamics at play can be summarized in this first proposition: The more distinct a group is in terms of phenotypical or cultural characteristics from the rest of the population, the greater the level of prejudice associated with these traits, and the lower the probability of exit from this situation, then the stronger the sentiments of in-group solidarity among its members and the higher the appropriable social capital based on this solidarity.

In addition to charitable contributions, like those solicited by Officer Lozano, a more common use of this source of social capital is in the creation and consolidation of small enterprises. A solidary ethnic community represents, simultaneously, a market for culturally defined goods, a pool of reliable low-wage labor, and a potential source for start-up capital. Privileged access to these resources by immigrant entrepreneurs is, of course, not easily explainable on the basis of economic models focused on individual human capital and atomized market competition (Baker and Faulkner 1991). Aldrich and Zimmer (1986, p. 14) make essentially the same point when they note that "conditions that raise the salience of group boundaries and identity, leading persons to form new social ties and action sets, increase the likelihood of entrepreneurial attempts by persons within that group and raise the probability of success."

However, reaction to cultural differences and outside discrimination alone do not account fully for the observed differences in the strength of bounded solidarity among different immigrant communities. We find such differences among groups subjected to similar levels of discrimination and even among those whose exit is equally blocked. The missing element seems to be the ability of certain minorities to activate a cultural repertoire, brought from the home country, which allows them to construct an autonomous portrayal of their situation that goes beyond a mere adversarial reaction.

German and Russian Jews arriving in the 19th century and in the early 20th represent the paradigmatic example of a group whose situational

solidarity, when confronted with widespread native prejudice, was not limited to an adversarial stance, but went well beyond it by taking advantage of a rich cultural heritage. Jewish-American society developed its own autonomous logic governed not so much by what "natives were thinking of us" than by concerns and interests springing from the group's distinct religious and cultural traditions (Howe 1976; Rischin 1962). Chinese-Americans were not far behind. According to Nee and Nee (1973), the "bachelor society" of San Francisco's Chinatown was organized along lines that reproduced in close detail the influence of Kwangtung Province, where most immigrants originated.³ As in Kwangtung, the basis of social organization in Chinatown was the kinship group or clan that incorporated males who claimed descent from a common ancestor: "The Wong, Lee, and Chin families were the largest and most powerful clans in Chinatown. Basic everyday needs were dealt with within the framework of the clan unit in which a sense of shared collective responsibility and mutual loyalty were central values" (Nee and Nee 1973, p. 64).

The reproduction of Chinese practices and values to deal with adverse circumstances continues to our day (Zhou 1992). The opening chapter of Amy Tan's (1989) autobiographical novel tells of the recreation in San Francisco of a weekly club originating in Kweilin (Guilin) during the Japanese invasion of China. Organized by immigrant women, the Joy Luck Club had the purpose of easing the difficulties of poverty and cultural adjustment by providing an atmosphere of camaraderie through good food and games. A generation later the club was still functioning with its members discussing joint investments in the stock market while they sat around the Mah Jong table.

The salience of many cultural practices and their reenactment after immigration do not come about spontaneously, but usually result from the clash with the host society and they are, in this sense, an emergent product. The fundamental source of solidarity is still situational, since it is the reality of discrimination and minority status that activates dormant home customs (Yancey, Ericksen, and Juliani 1976).

Because of its recency, the Nicaraguan immigrant community of Miami provides an excellent example of the birth of bounded solidarity and the reactivation of a cultural repertoire brought from the home country. In the words of one community leader, Nicaraguan refugees resent that "people think we're all uneducated, poor people without documents" (Branch 1989, p. 20). To reassert their own identity and distinctness, Nicaraguans have resorted to a variety of practices including the revival

³ Nee and Nee use the Wade-Giles spelling system. In Pinyin, the romanization system now used in China, the province name is spelled Guangdong.

of near-forgotten folk items. Ethnic stores, for example, do a brisk business selling Nicaraguan products such as *cotonas* (a cotton shirt usually worn only by Nicaraguan Indians) to well-to-do refugees. As one store owner put it, "The people who always wore American brands and European clothes in Nicaragua now come shopping for a *cotona* to wear to parties" (Veciana-Suarez 1983, p. 10).

Not all immigrants have the same opportunity to reinforce the emergent solidarity arising out of confrontation with a foreign society with a sense of cultural continuity and autonomous presence. Among groups whose ethnicity was "made in America," such as those described by Glazer, the necessary elements for construction of a collective identity may be missing or may have been forgotten, forcing the minority to borrow them from the very cultural mainstream to which it is reacting. Peasants from Southern Italy and the eastern reaches of the Austro-Hungarian empire recruited to work in American factories and mines had but a faint idea of the nations they left behind. As they clustered together in ethnic communities in America, they often had to accept definitions of their own identity based on host-society stereotypes. Similarly, Polish immigrants arriving in the early years of this century often learned about their nationality in the United States; in Poland, the rural lords were the Poles, the peasants were just peasants (Glazer 1954; Greeley 1971; Sowell 1981).

Although most contemporary immigrants have a clear idea of their national identities, exceptional circumstances still exist that prevent their reenactment in places of reception. Peasant refugees from the remote highlands of Southeast Asia resettled in American cities offer an appropriate example. Unable to reimplement cultural practices from a preindustrial past in such a vastly different environment, they often lapse into despair and various forms of emotional disorder (Rumbaut and Ima 1988). The following statement from a Hmong refugee in southern California illustrates a situation reminiscent of those recorded by Thomas and Znaniecki ([1918–20] 1984) among Polish peasant immigrants at the turn of the century: "In our old country, whatever we had was made or brought in by our own hands; we never had any doubts that we would not have enough for our mouth. But from now on to the future, that time is over. So you see, when you think these things over, you don't want to live anymore. . . . Don't know how to read and write, don't know how to speak the language. My life is only to live day by day until the last day I live, and maybe that is the time when my problems will be solved" (Rumbaut 1985, pp. 471–72).

These contrasting experiences lead to the following second proposition: Social capital arising out of situational confrontations is strongest when the resulting bounded solidarity is not limited to the actual events but

brings about the construction of an alternative definition of the situation based on reenactment of past practices and a common cultural memory.

ENFORCEABLE TRUST

The fourth source of social capital is also based on the existence of community except that, in this case, it is not sentiments of solidarity based on outward confrontation, but the internal sanctioning capacity of the community itself that plays the central role. In his article, Coleman (1988) identifies this mechanism as the difference between open and closed social structures: "Closure of the social structure is important not only for the existence of effective norms but also for another form of social capital: the trustworthiness of social structures that allows the proliferation of obligations and expectations" (Coleman 1988, pp. S107–S108).

What Coleman refers to as "closure" is, of course, the degree to which a particular collectivity forms a group at all, as opposed to a mere aggregate of individuals. Commonalities in experiences of departure from the home country and conditions at arrival in the United States create bonds among immigrants and give rise to a multiplicity of social networks that frequently coalesce into tightly knit ethnic communities. The social capital emerging from the monitoring capacity of these communities is best referred to as enforceable trust.

As seen above, bounded solidarity shares with the first source of social capital (value introjection) an element of moral obligation. Individuals behave in certain ways because they must—either because they have been socialized in the appropriate values or because they enact emergent sentiments of loyalty toward others like themselves. Such behavior can occur even in the absence of reward or punishment. The final source of social capital discussed here shares with the second (reciprocity exchanges) a strong instrumental orientation. In both cases, individuals behave according to expectations not only because they must, but out of fear of punishment or in anticipation of rewards. The predictability in the behavior of members of a group is in direct proportion to its sanctioning capacity. Hence, the oxymoron: *trust* exists in economic transactions precisely because it is *enforceable* by means that transcend the individuals involved.

The economic-sociology literature has already noted that the rewards and sanctions administered by ethnic communities are generally nonmaterial in character, but that they can have very material consequences in the long run. A key aspect of the latter is access to resources for capitalizing small enterprises. Economic sociology has here one of the prime instances showing the utility of its approach as compared with individualistic accounts of economic attainment. For illustration, however, sociol-

ogists seem to have hit on only one example: the rotating credit association. Since Ivan Light (1972) called attention to this form of small-firm capitalization, rotating credit associations have become de rigueur as an illustration of the significance of embeddedness (Granovetter 1985), social capital (Coleman 1988), and "group solidarity" (Hechter 1987, p. 108). Other network-based mechanisms exist, however as the following two examples show.

Dominicans in New York City

The Dominican immigrant community in New York City was characterized until recently as a working-class ghetto composed mostly of illegal immigrants working for low wages in sweatshops and menial service occupations. A study conducted under the auspices of the U.S. Congressional Commission for the Study of Immigration contradicts this description and points to the emergence of a budding entrepreneurial enclave among Dominican immigrants (Portes and Guarnizo 1991). The city-within-a-city that one encounters when entering the Washington Heights area of New York with its multiplicity of ethnic restaurants and stores, Spanish-language newspapers, and travel agencies is, to a large extent, a Dominican creation built on the strength of skills brought from the Dominican Republic, ready access to a low-wage labor pool, and the development of informal credit channels.⁴

New York hosts several formally registered Dominican finance agencies (*financieras*) but, in addition, networks of informal loan operations grant credit with little or no paperwork. Capital comes from profits of the drug trade, but also from established ethnic firms and savings of workers who obtain higher interest rates in the ethnic finance networks than in formal banking institutions. These sources are reinforced by flight capital from the Dominican Republic. Money circulates within community networks and is made available for business start-ups because recipients are fully expected to repay. This expectation is based first on the reputation of the recipient and second on swift retribution against those who default. Such punishment may include coercive measures but is more often based on ostracism from ethnic business circles. Outside the

⁴ According to the Federation of Dominican Industrialists and Merchants of New York, the city hosts some 20,000 Dominican-owned businesses, including about 70% of all Spanish grocery stores or *bodegas*, 90% of the gypsy cabs in Upper Manhattan, three chains of Spanish supermarkets, and several newspapers and radio stations. Allowing for a measure of prideful exaggeration in these figures, they still point to the diversity of business initiatives in which Dominican immigrants can be found (Guarnizo 1992).

immigrant enclave, Dominicans have very few opportunities other than low-wage menial labor.

These patterns can be illustrated by the experiences of a Dominican entrepreneur interviewed in the course of fieldwork in New York. This man whom we shall call Nicolas is 38 years old and already owns five shops in New York City and a *financiera* in the Dominican Republic. He employs a staff of 30, almost all of whom are Dominican relatives or friends of relatives. For finance, he relies exclusively on the informal system of Washington Heights. Sometimes he acts as an investor and sometimes as a borrower. As an investor, Nicolas has earned a good reputation that enables him to collect several thousand dollars to be invested in his businesses in New York and Santo Domingo. These investments generally come from other immigrants who do not yet have enough capital to initiate businesses themselves. As a borrower, he seems to enjoy ample credit. At the time of the interview, Nicolas had two active loans—one for \$125,000 and the other for \$200,000—only one of which was accompanied by some signed papers. He was paying a monthly interest of 2.6%.⁵

Cubans in Miami

Conventional accounts of business success among Cuban exiles in South Florida attributed their advance to material capital brought by the earlier arrivals. Subsequent studies have shown the inadequacy of this explanation since few of the businesses that formed the core of the Miami enclave were capitalized in this fashion (Wilson and Portes 1980; Wilson and Martin 1982). The rotating credit association did not exist as a cultural practice in Cuba, so a different type of mechanism had to give rise to the first ethnic firms. By the mid-1960s, a few small banks in Miami were owned by wealthy South American families who began hiring unemployed Cuban exile bankers first as clerks and then as loan officers. Once their own jobs became secure, these bankers started a program of lending small sums—from \$10,000 to \$30,000—to fellow exiles for business start-ups. These loans were made without collateral and were based exclusively on the personal reputation of the recipient in Cuba.⁶

This source of credit became known as “character” loans. Its effect was to allow penniless refugees who had no standing in the American

⁵ From field interviews in New York conducted as part of the same study (Portes and Guarnizo 1991).

⁶ This material was gathered by the senior author during fieldwork in Miami and will be included in a forthcoming book on that city (Portes and Stepick 1993).

banking system to gain a foothold in the local economy. A banker who took part in this operation described it as follows:

At the start, most Cuban enterprises were gas stations; then came grocery shops and restaurants. No American bank would lend to them. By the mid-sixties we started a policy at our bank of making small loans to Cubans who wanted to start their own business, but did not have the capital. These loans of \$10,000 or \$15,000 were made because the person was known to us by his reputation and integrity. All of them paid back; there were zero losses. With some exceptions they have continued being clients of the bank. People who used to borrow \$15,000 on a one-time basis now take \$50,000 in a week. In 1973, the policy was discontinued. The reason was that the new refugees coming at that time were unknown to us. [Portes and Stepick 1991]

Character loans made possible the creation of a thick layer of small and mid-size firms that are today the core of the Cuban ethnic economy. Bounded solidarity clearly had something to do with the initiative since fellow exiles were preferred to other potential recipients. However, this mechanism was not enough. The contrast between the exiles arriving in the 1960s to whom these loans were available and those who came after 1973 who were ineligible for them marks the boundaries of a prerevolutionary community of business people in which personal reputation and social ties were a precondition for success. Once in Miami, these connections became all the more valuable because penniless refugees had little else on which to rebuild their careers.

The Cuban bankers, therefore, had good reasons for making these loans because they were certain that their clients would pay back. Anyone defaulting or otherwise violating the expectations built into such deals would be excluded from the community and, as it was with the Dominicans in New York, there was precious little else in Miami in the way of economic opportunity. Character loans were backed, therefore, by much more than sentiments of loyalty or a written promise to repay, but by the sanctioning capacity built into the business networks of the enclave. The fact that bounded solidarity did not suffice is demonstrated by the exclusion of Cubans who came after the exodus of the prerevolutionary business elite was complete.⁷

Community Resources

As a source of social capital, enforceable trust varies greatly with the characteristics of the community. Since the relevant behaviors are guided

⁷ For a periodicization of the stages of the Cuban exodus, see Diaz-Briquets and Perez (1981) and Pedraza-Bailey (1985).

by instrumentalist expectations, the likelihood of their occurrence is conditioned by the extent to which the community is the sole or principal source of certain rewards. When immigrants can draw on a variety of valued resources—from social approval to business opportunities—from their association with outsiders, the power of their ethnic community becomes weaker. Conversely, when outside prejudice denies them access to such rewards, observance of community norms and expectations becomes much more likely. After reviewing studies of business behavior of the overseas Chinese in the Philippines and of Asian Indians in Kenya, Granovetter (in press) arrives essentially at the same conclusion noting that “the discrimination that minority groups face can actually generate an advantage. . . . Once this discrimination fades, intergenerational continuity in business is harder to sustain.” These observations lead to the following third proposition: As a source of social capital, enforceable trust is directly proportional to the strength of outside discrimination and inversely proportional to the available options outside the community for securing social honor and economic opportunity.

What happens on the outside must be balanced, however, with the resources available in the ethnic community itself. It may be that a second- or third-generation Chinese-American or Jewish-American faces no great prejudice in contemporary American society, yet she or he may choose to preserve ties to the ethnic community because of the opportunities available through such networks. The durability of institutions created by successful immigrant groups may have less to do with the long-term persistence of outside discrimination than with the ability of these institutions to “compete” effectively with resources and rewards available in the broader society. Conversely, a resource-poor immigrant community will have trouble enforcing normative patterns even if its members continue to face severe outside discrimination.

An ongoing study of second-generation Haitian students in Miami high schools illustrates the point.⁸ Like other immigrant groups before them, Haitian parents want their children to preserve their culture and language as they adapt to the American environment. However, Haitian parents lack the means to send their children to private schools, and, in any case, there are none in Miami that teach in French or foster Haitian culture. As a result, many Haitian-American students must attend the same high school that serves the inner-city area known as Liberty City. There Haitian students are socialized in a different set of values, includ-

⁸ Study in progress entitled, “Children of Immigrants: The Adaptation Process of the Second-Generation.” The material presented herein comes from preliminary fieldwork conducted by the senior author in South Florida during the summer of 1990 (see Portes and Stepick 1993, chap. 8).

ing the futility of trying to advance in life through education. They find their culture denigrated by native-born minority students who often poke fun at Haitians' accents and docility. Since immigrant parents have very little to show for their efforts, and the Haitian community as a whole is poor and politically weak, second-generation students have few incentives to stay within it, and many opt to melt into the mainstream. In this instance, "mainstream" does not mean the white society, but the impoverished black community of Liberty City. As this happens, social capital based on immigrant community networks is dissipated.⁹

Last, the effectiveness of collective sanctions through which enforceable trust is built depends on the group's ability to monitor the behavior of its members and its capacity to publicize the identity of deviants. Sanctioning capacity is increased by the possibility of bestowing public honor or inflicting public shame immediately after certain deeds are committed. Means of communication, in particular the ethnic media, play a crucial role in this regard (Olzak and West 1991). Foreign-language newspapers, radio stations, and television spots exist not only to entertain and inform the respective communities, but also to uphold collective values and highlight their observance or violation (Formant 1989, pp. 63–64). As such, the existence of well-developed media channels within an ethnic community represents a powerful instrument of social control.

These observations can be summarized in a fourth proposition: The greater the ability of a community to confer unique rewards on its members, and the more developed its internal means of communication, then the greater the strength of enforceable trust and the higher the level of social capital stemming from it.

By failing to take into account the differential presence of resources giving rise to enforceable trust, orthodox economic models of minority poverty and mobility deprive themselves of a crucial analytical tool. Recent research shows that levels of entrepreneurship vary significantly among ethnic minorities and that such differences are positively associated with average incomes (Fratone and Meeks 1985; Aldrich and Zimmer 1986; Light and Bonacich 1988; Borjas 1990). Other analyses indicate that neither the origins of ethnic entrepreneurship nor its average higher levels of remuneration are fully explained by human capital differences (Portes and Zhou 1992). Social capital arising from enforceable trust may well account for the remaining differences. These favorable consequences are, of course, congruent with those hypothesized by Coleman (1988). Having concluded the analysis of different types of social capital and the

⁹ See n. 8 above. On the condition of Haitians in South Florida, see also Stepick (1982) and Miller (1984).

processes giving rise to them, it is appropriate at this point to consider the other side of the matter.

NEGATIVE EFFECTS

Coleman's analysis of social capital sounds a note of consistent praise for the various mechanisms that lead people to behave in ways different from naked self-interest. His writing adopts at times a tone of undisguised nostalgia, reminiscent of Tönnies' longing for the times when there was more social closure and when *gemeinschaft* had the upper hand (Tönnies [1887] 1963). Indeed, it is our sociological bias to see good things emerging out of social embeddedness; bad things are more commonly associated with the behavior of *homo economicus*. Many examples could be cited in support of the sociological position in addition to those presented by Coleman (see Hechter 1987; Uehara 1990). To do so, however, would only belabor the point. Instead, this final section considers the other side of the question. When it is put on a par with money capital and human capital, an instrumentalist analysis of social capital is necessarily biased toward emphasizing its positive uses—from capitalizing minority enterprises to cutting down the number of lawyers required for enforcing contracts.

It is important, however, not to lose sight of the fact that the same social mechanisms that give rise to appropriable resources for individual use can also constrain action or even derail it from its original goals. At first glance, the term "social debit" might seem appropriate in order to preserve the parallel with money capital. However, on closer examination, this term is inadequate because the relevant phenomena do not reflect the *absence* of the same forces giving rise to social capital but rather their other, presumably less desirable, manifestations. The following examples illustrate these alternative facets.

Costs of Community Solidarity

The existence of a measure of solidarity and trust in a community represents a precondition for the emergence of a network of successful enterprises. However, the exacerbation of these sentiments and obligations can conspire against exactly such a network. In his study of the rise of commercial enterprises in Bali, Clifford Geertz observed how successful entrepreneurs were constantly assaulted by job- and loan-seeking kinsmen. These claims were buttressed by strong norms enjoining mutual assistance within the extended family and among community members in general. Balinese social life is based on groups called *seka*, and individuals typically belong to several of these. "The value of *seka* loyalty,

putting the needs of one's group above one's own is, along with caste pride, a central value of Balinese social life" (Geertz 1963, p. 84). Although entrepreneurship is highly valued in this community, successful businessmen face the problem of numerous claims on their profits based on the expectation that economic decisions "will lead to a higher level of welfare for the organic community as a whole" (Geertz 1963, p. 123). The result is to turn promising enterprises into welfare hotels, checking their economic expansion.

Granovetter (in press), who calls attention to this phenomenon, notes that it is the same problem that classic economic development theory identified among traditional enterprises and that modern capitalist firms were designed to overcome. Weber ([1922] 1963) made the same point when he identified arm's length transactions guided by the principle of universalism as one of the major reasons for the success of Puritan enterprises. Hence, cozy intergroup relationships of the sort frequently found in solidary communities can give rise to a gigantic free-riding problem. Less diligent group members can enforce on successful members all types of demands backed by the same normative structure that makes the existence of trust possible.

In the indigenous villages surrounding the town of Otavalo in the Ecuadorian Andes, male owners of garment and leather artisan shops are often Protestant (or "Evangelicals" as they are known locally) rather than Catholic. The reason is not that the Protestant ethic spurred them to greater entrepreneurial achievement nor that they found Evangelical doctrine to be more compatible with their own beliefs, but a rather more instrumental one. By shifting religious allegiance, these entrepreneurs remove themselves from the host of social obligations for male family heads associated with the Catholic church and its local organizations. The Evangelical convert becomes, in a sense, a stranger in his own community, which insulates him from free riding by others who follow Catholic-inspired norms.¹⁰

Among present-day immigrant communities in the United States scattered instances of this phenomenon appear to be operating. Southeast Asian, and particularly Vietnamese, businesses in California have been affected by a particularly destructive form of collective demands from fellow exiles, including Vietnamese youth and former military officers (Chea 1985; Efron 1990). In the course of fieldwork in Orange County, California, Rubén Rumbaut interviewed a successful Vietnamese electronics manufacturer who employed approximately 300 workers in his

¹⁰ Preliminary results of an ongoing study of indigenous entrepreneurship in the Andes being conducted by the Latin American School of Social Sciences (FLACSO) in Quito. Personal communication with the study director, Dr. Jorge Leon, June 1991.

plant. Not one of them was Vietnamese. The owner had anglicized his name and cut most of his ties to the immigrant community. The reason was less a desire for assimilation than fear of the demands of other Vietnamese, especially the private "security services" organized by former members of the Vietnamese police.¹¹

Constraints on Freedom

A second manifestation of negative effects consists of the constraints that community norms put on individual action and receptivity to outside culture. This is an expression of the age-old dilemma between community solidarity and individual freedom in the modern metropolis, already analyzed by Simmel ([1902] 1964). The dilemma becomes acute in the case of tightly knit immigrant communities since they are usually inserted in the core of the metropolis, yet are simultaneously upholding an exotic culture. The city-within-a-city sustained by the operation of solidarity and trust creates unique economic opportunities for immigrants, but often at the cost of fierce regimentation and limited contacts with the outside world. The Spanish-language media, so instrumental in maintaining community controls among Latins in South Florida (Forment 1989) also imposes, in the opinion of many observers, a virtual censorship. Joan Didion reports the views on the matter of a dissident exile banker: "This is Miami. . . . A million Cubans are blackmailed, totally controlled by three radio stations. I feel sorry for the Cuban community in Miami. Because they have imposed on themselves, by way of the right, the same condition that Castro has imposed in Cuba. Total intolerance" (Didion 1987, p. 113).

Until a few years ago, San Francisco's Chinatown was a tightly knit community where the family clans and the Chinese Six Companies ruled supreme. These powerful associations regulated the business and social life of the community, guaranteeing its normative order and privileged access to resources for its entrepreneurs. Such assets came, however, at the cost of restrictions on most members' scope of action and access to the outside world. In their study of Chinatown, Nee and Nee (1973) report on the continuing power of the clans and the Chinese companies and their strong conservative bent. What put teeth in the clans' demands was their control of land and business opportunities in the Chinese enclave and their willingness to exclude those who violated normative consensus by adopting a "progressive" stance. One of the Nees' informants

¹¹ This interview was conducted in June 1985 and is reported in Portes and Rumbaut (1990, pp. 3-4).

complained about this conservative stronghold in terms similar to those of the Miami banker above:

And not only the Moon Family Association, all the family associations, the Six Companies, any young person who wants to make some changes, they call him a communist right away. He's redcapped right away. They use all kinds of tricks to run him out. You see, in old Chinatown, they didn't respect a scholarly person or an intelligent person. . . . They hold on to everything the way it was in China, in Kwangtung. Even though we're in a different society, a different era. [Nee and Nee 1973, p. 190]

Like Chinatown in San Francisco, the Korean community of New York is undergirded by a number of associations—from traditional extended family groups and various types of *gye* (rotating credit associations) to modern businesses and professional organizations. The role of this associational structure in generating social capital for collective advancement follows closely the pattern of enforceable trust already described. The flip side of this structure takes, however, a peculiar form among Koreans. As described by Illsoo Kim (1981), the South Korean government, represented by its consulate general, has played a very prominent role in the development of the ethnic community. “Partly because Korean immigrants have a strong sense of nationalism and therefore identify with the home government, the Korean Consulate General in New York City . . . has determined the basic tone of community-wide politics” (Kim 1981, p. 227).

This position has, in the past, enabled the South Korean government to promote its own interests by rewarding “loyal” immigrants with honors and business concessions and by intimidating its opponents. Especially during the government of Park Chung-hee, the Korean CIA (KCIA) was active in the community rooting out anti-Park elements and silencing them with threats of financial ruin or even physical harm. In an American context, this heavy political hand became excessive, leading other community organizations to mobilize in order to weaken its hold. The consulate remained, however, an integral part of this community and a significant institutional factor.

The solidarity and enforcement capacity that promote ethnic business success also restrict the scope of individual expression and the extent of extracommunity contacts. Combined with the first type of negative effect discussed above, these examples can be summarized in the following fifth proposition: The greater the social capital produced by bounded solidarity and community controls, then the greater the particularistic demands placed on successful entrepreneurs and the more extensive the restrictions on individual expression.

Leveling Pressures

The first two negative effects above are not intrinsically at odds with economic mobility, but represent its marginal costs, as it were: successful individuals are beset by fellow group members relying on the strength of collective norms, and highly solidary communities restrict the scope of personal action as the cost of privileged access to economic resources. The last form discussed in this section conspires directly against efforts toward individual mobility by exerting leveling pressures to keep members of downtrodden groups in the same situation as their peers. The mechanism at work is the fear that a solidarity born out of common adversity would be undermined by the departure of the more successful members. Each success story saps the morale of a group, if that morale is built precisely on the limited possibilities for ascent under an oppressive social order.

This conflict is experienced by the Haitian-American teenagers discussed above, as they are torn between parental expectations for success through education and an inner-city youth culture that denies that such a thing is possible. Assimilation to American culture for these immigrant children often means giving up the dream of making it in America through individual achievement.¹² The neologism "wannabe," arguably the latest contribution of inner-city youth to the cultural mainstream, captures succinctly the process at hand. Calling someone by this name is a way of ridiculing his or her aspiration to move above his or her present station and of exercising social pressure on the person to remain in it. In his ethnographic research among Puerto Rican crack dealers in the Bronx, Bourgois (1991) calls attention to the local version of the "wannabe" among second-generation Puerto Rican youngsters—the "turnover." He reports the views of one of his informants:

When you see someone go downtown and get a good job, if they be Puerto Rican, you see them fix up their hair and put some contact lens in their eyes. Then they fit in. And they do it! I have seen it! . . . Look at all the people in that building, they all turn-overs. They people who want to be white. Man, if you call them in Spanish it wind up a problem. I mean like take the name Pedro—I'm just telling you this as an example—Pedro be saying (imitating a whitened accent) "My name is Peter." Where do you get Peter from Pedro? [Bourgois 1991, p. 32]

In their description of what they label the "hyperghetto" of Chicago's South Side, Wacquant and Wilson (1989) speak of a similar phenomenon in which solidarity cemented on common adversity discourages individuals from seeking or pursuing outside opportunities. Notice that in each

¹² For a similar situation confronting Mexican and Central American immigrant children in California schools, see Suarez-Orozco (1987).

such situation, social capital is still present, but its effects are exactly the opposite of those found among other immigrant communities. Whereas among Asian, Middle-Eastern, and other foreign groups, social capital based on bounded solidarity is one of the bases for the construction of successful enterprises, in the inner city it has the opposite effect.

This contrast is all the more telling because it often involves groups from the same broad cultural origin. In this regard, the use of Spanish in Miami and in the Bronx is instructive. In the Bronx, shifting to English and anglicizing one's name is a sign that the individual aspires to move up by leaving behind his or her ethnic community. In Miami, the same behavior would bring exclusion from the business networks of the enclave and the unique mobility opportunities that they make available. In both instances, public use of Spanish signals membership in the ethnic community, but the socioeconomic consequences are very different.

Perhaps the most destructive consequence of this negative manifestation of social capital is the wedge that it drives between successful members of the minority and those left behind. To the extent that community solidarity is exclusively based on an adversarial view of the mainstream, those who attempt to make it through conventional means are often compelled to adopt majority opinions antithetical to their own group. Bourgois (1991) illustrates this point with the example of a former Bronx pusher, now a born-again Christian and insurance salesman in the Connecticut suburbs:

If anything when you look at me you know I'm Hispanic. When I jog down the neighborhood, people get scared. It's not a problem for me because I have self-confidence . . . every once in a while I used to get a crank call in the house, saying "Hey, spic," you know "spic" and other stuff, but I don't worry about that.

In a sense, I've learned to be in their shoes. You see what I mean. Because I've seen what minorities as a group can do to a neighborhood. So I step into their shoes and I understand, I sympathize with them. Cause I've seen great neighborhoods go down. [Pp. 34-35]

For the lone escapee from the ghetto, "self-confidence" takes the place of group support. Confronting prejudice alone often means accepting some of its premises and turning them against one's own past. The suburban white, perceived from the ghetto as an adversary, is transformed into a sympathetic figure "in whose shoes" one can stand.

It is beyond the scope of this article to explore the contextual forces leading to the widely different outcomes of ethnic solidarity. The existing literature suggests, however, one common strand in every manifestation of this last type of negative effect based on downward leveling norms. Each such instance has been preceded by extensive periods of time, sometimes generations, in which the upward mobility of the group has been

blocked (Marks 1989; Barrera 1980; Nelson and Tienda 1985). This has been followed by the emergence of collective solidarity based on opposition to these conditions and an accompanying explanation of the group's social and economic inferiority as caused by outside oppression. Although correct historically, this position frequently produces negative consequences for individual mobility through the operation of the mechanism discussed above. These observations lead to the following sixth and final proposition: The longer the economic mobility of a group has been blocked by coercive nonmarket means, then the more likely the emergence of a bounded solidarity that negates the possibility of advancement through fair market competition and that opposes individual efforts in this direction.

The six propositions presented above are summarized in figure 1, which attempts to formalize the discussion of antecedents and effects of solidarity and trust. As indicated previously, the propositions have the character of hypotheses that have been drawn from past and ongoing research on immigration, but will be subject to the test of additional evidence. The present state of knowledge does not allow a more refined analysis of the character of relationships between antecedent and consequent factors including, for example, whether they involve additive or interactive effects. Such refinements and possible corrections to the set of hypothesized relationships must await additional work.

Figure 1 does not fully resolve the apparent ambiguity in the final proposition which, at first glance, contradicts earlier ones concerning the positive effects of bounded solidarity. In fact, it supports them: the reactive mechanism giving rise to bounded solidarity in response to outward discrimination is the same as outlined earlier. The crucial difference lies in the extent of discrimination and its duration. Protracted periods of oppression, especially in a no-exit situation, undermine the cultural and linguistic resources available to a group for constructing an alternative definition of the situation (second proposition). A situation of permanent subordination also deprives a collectivity of the resources necessary to reward or punish its members independently (fourth proposition), so that its enforcement capacity is entirely dependent on outside discrimination that forces its members to band together (third proposition). The downward leveling pressures reviewed here are a reaction to the partial breakdown of this last source of sanctioning capacity, as fissures in the barriers confronting the group allow some of its members—"wannabes" or "turnovers"—to escape its hold.

SUMMARY AND CONCLUSION

In this article, we have attempted to contribute to the reemerging field of economic sociology by delving into its classic roots and by using empirical

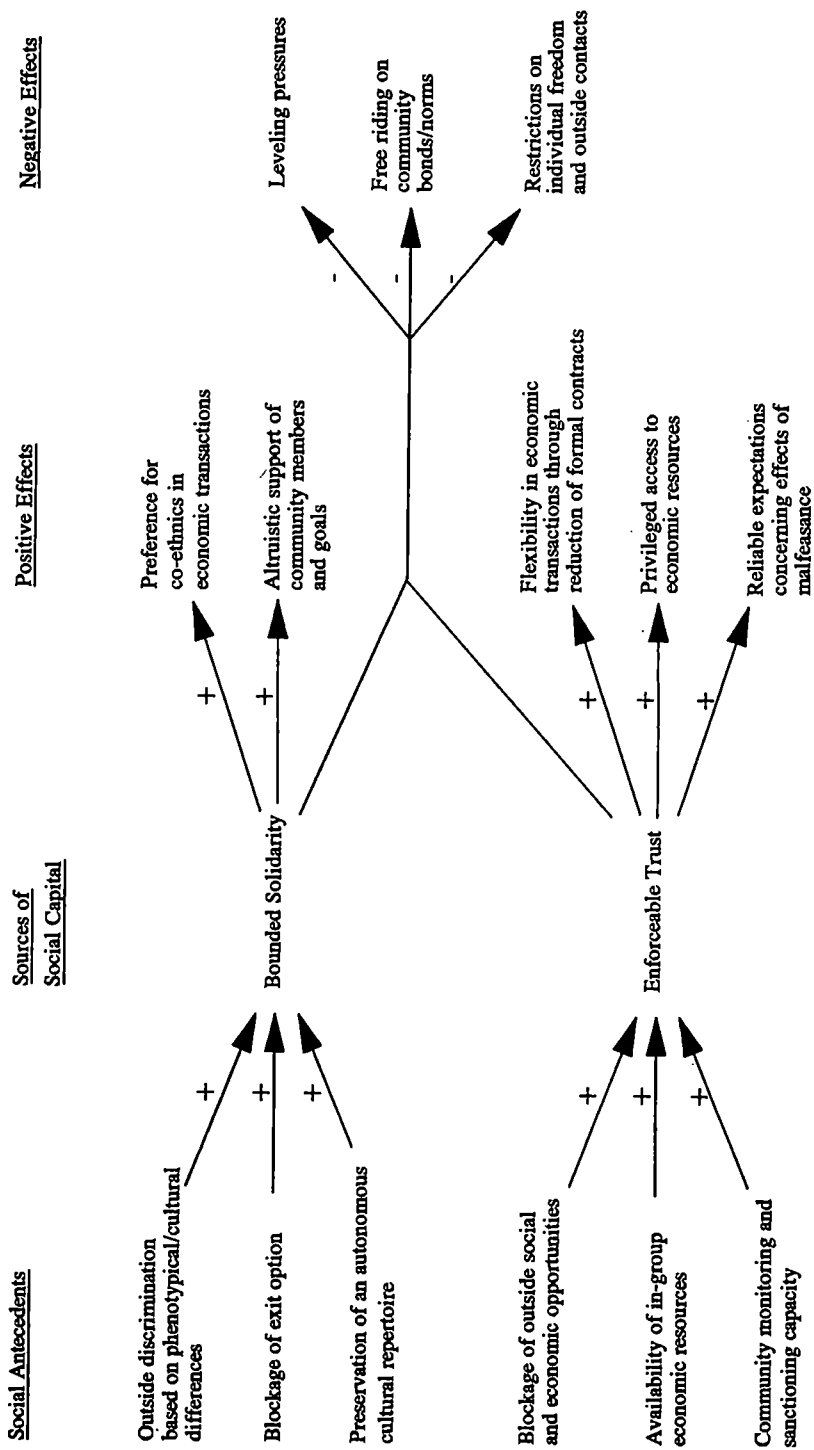


FIG. 1.—Antecedents and effects of two types of social capital among immigrant communities

examples from the immigration literature to explore the different forms in which social structures can affect economic action. Social embeddedness provides a suitable conceptual umbrella for this exploration although, to analyze its specific manifestations, we have focused on the concept of social capital. We have argued that previous analyses of this concept have been too vague concerning its origins and too instrumentalist about its effects. Accordingly, the aim of our analysis has been to identify the various mechanisms leading to the emergence of social capital and to highlight its consequences, positive and negative.

Economic sociology traces its origins not only to Max Weber and other sociological classics, but also to economists such as Joseph Schumpeter who saw in this field a needed corrective to the simplifications of classical economy theory. Schumpeter and other "historical school" economists in Germany, as well as Veblen and later institutionalist economists in the United States, struggled mightily to stem the growing tide to convert individuals into "mere clotheslines on which to hang propositions of economic logic" (Schumpeter 1954, pp. 885–87; Swedberg 1991). The effort to highlight the economic significance of what Schumpeter called "social institutions" collapsed, however, under the relentless expansion of individualism and rational utilitarian models, to the point that these perspectives have begun to make significant inroads into sociology itself.

In this context, the efforts of Granovetter (1985, 1990), Block (1990), and others to reopen space for social structures in the analysis of economic life represent a commendable, but still fragile venture. In our view, such efforts will not prosper if limited, as in the past, to a critique of neoclassical theories without there being anything proposed to replace them. A viable strategy for filling this gap is not to ground sociological perspective in armchair speculation, but in established bodies of empirical knowledge. The modest but rapidly growing immigration-research literature offers a rich source for such efforts, exemplified but not exhausted by the exercise in these pages.

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Socioeconomic Status and Depression: The Role of Occupations Involving Direction, Control, and Planning¹

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This article proposes a social causation explanation for the association between SES and depression/distress. The model links SES, occupational direction, control, and planning (DCP), personality factors, and depression/distress in a causal sequence. The data to test the social causation model against alternative, social selection models are derived from samples of psychiatric patients and community residents in Washington Heights, New York City. The key factor of DCP is operationalized using ratings from the *Dictionary of Occupational Titles*. The results support the social causation model and cannot be accounted for by several tests derived from social selection models. Thus the results increase the plausibility of the social causation model and suggest the need for further research on the links between occupational conditions and mental disorder.

A central theoretical proposition of sociology states that social structural positions have dramatic effects on life chances. The possibility that this principle extends to the risk of developing a severe mental disorder is suggested by a heterogeneous set of studies that consistently link mental disorders to socioeconomic standing (for reviews of these studies see Dohrenwend and Dohrenwend [1974], Dohrenwend, Dohrenwend, et al. [1980], and Link and Dohrenwend [1989]). Rates of mental disorder aggregated across the different types are more than 2.5 times higher in the lowest socioeconomic group than in the highest, according to 20 studies conducted in North America and Europe between 1950 and 1980 (Neugebauer, Dohrenwend, and Dohrenwend 1980).

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These studies, however, do little more than allow the possibility of an effect of social position on mental disorders. Equally compatible with the facts are explanations which claim that the relationship between socioeconomic status and mental disorder is due to the negative effects of mental disorders on status attainment. One obvious route is through impaired functioning (e.g., absenteeism, poor job performance) caused by mental disorder. According to another, more complex explanation, individuals vulnerable to mental disorder also are likely to perform poorly in the competition for desirable occupational positions. In such cases even persons experiencing their first onset of disorder may be of lower socioeconomic standing because of selection factors.

Given this ambiguity, how can we make progress in understanding the association between socioeconomic status (SES) and mental disorder? An approach advocated by Link and Dohrenwend (1989) suggests that the two explanations for the SES relationship—social causation and social selection—imply very different intervening mechanisms. If the association is due to selection, we should find factors such as genetic endowments or early environmental risk factors (with parental class held constant) that determine both the onset of mental disorder and low SES. Alternatively, from a social causation perspective, we should find risk factors (e.g., stressful circumstances) that are a consequence of SES and in turn are related to mental disorder. The success of attempts to locate mechanisms consistent with each of these broader selection/causation explanations will lead eventually to a clearer interpretation of the association between SES and mental disorder.

Investigations of the social causation/social selection issue are far more common for schizophrenia than ~~for~~ other types of mental disorders (Dunham, Phillips, and Shrinivasan 1966; Eaton 1980; Goldberg and Morrison 1963; Link, Dohrenwend, and Skodol 1986; Turner and Wagenfeld 1967). The main controversies have concerned studies of the occupational careers of people who have developed schizophrenia, as compared to their fathers and to various control populations. These studies themselves have given rise to considerable controversy (Dohrenwend and Dohrenwend 1969; Fox 1990). Despite an intriguing debate about the mechanisms that might produce an association between SES and schizophrenia (Kohn 1972; Mechanic 1972), few studies actually have investigated these mechanisms (but see Link et al. 1986).

The relation of SES to depression has been studied far less than the relationship of SES to schizophrenia. Yet we will show evidence for an inverse relationship between SES and depression. Moreover, recent evidence suggests that social causation may play a stronger role in this relationship than in the relationship between SES and schizophrenia (Dohrenwend et al. 1992). We focus on locating and testing a mechanism

that might be a part of the social causation process. To this end we consider a particular characteristic of occupations, namely the extent to which they involve direction, control, and planning as measured in the *Dictionary of Occupational Titles* (DOT; U.S. Department of Labor 1977). According to the DOT, an occupation involves direction, control, and planning when the "worker is in a position to negotiate, organize, direct, supervise, formulate practices, or make final decisions." If the planning is for one's own activities only, a rating of direction, control, and planning is not assigned (U.S. Department of Labor 1972, p. 297). From a viewpoint of social causation, occupations involving direction, control, and planning are a plausible mechanism through which SES might be related to depression for two reasons. First, there is persuasive evidence (see below) to suggest that incumbency in occupations that entail *direction, control, and planning* (or DCP) may protect against depression. Second, occupations that entail these activities are uncommon in lower socioeconomic positions.

SES AND DEPRESSION

Data concerning the distribution of episodes of depression defined by explicit criteria (e.g., Research Diagnostic Criteria [Spitzer et al. 1978] or DSM III-R [APA 1987]) are rare. Studies conducted before 1980 tended to report results in terms of broad psychotic-versus-neurotic distinctions (Dohrenwend and Dohrenwend 1974). Episodes of major depression could fall into either one of these categories and therefore were grouped together with many other disorders (e.g., manic depressive psychosis, phobias, panic, obsessive compulsive disorder). As a result, we cannot draw conclusions from these earlier studies about the distribution of major depression per se by SES.

When we rely on evidence from the small number of more recent studies, we generally find that depressive disorder shows the same inverse relationship with SES that characterizes mental disorder in general. In Brown and Harris's (1978) Camberwell community study, working-class women have a rate of depression almost four times as great as middle-class women. Weissman and Myers's (1978) New Haven study showed current prevalence rates for major depression of 4.1% in the lowest SES group (Class V), compared to 1.5% in the highest. When less severe minor depressions were included, the lowest class had a rate seven times as great as the highest classes (Weissman and Myers 1978, p. 1307). Also, Dohrenwend et al.'s (1992) prevalence study conducted in Israel showed higher rates of current major depression among those with less than a high school education (4.5%) than among college graduates (2.6%). The five-site Epidemiological Catchment Area Study (Robbins and Regier

1990) has not shown similarly strong relationships between SES and depression (Mirotznik, Dohrenwend, and Kohn 1990). In fact, in some sites, for some indicators of SES, the association was found to be nonsignificant when age and gender were controlled (Leaf et al. 1986). Still, aggregated results from the five sites, which use composite indexes of SES (education, occupation, and income) show almost a twofold difference between persons in each of the two lowest SES quartiles and those in the top quartile even when age and gender are controlled (Holzer et al. 1988). Moreover, when we turn to scales of depressive symptoms assessed with the Epidemiological Catchment Area's diagnostic interview (the Diagnostic Interview Schedule), we find more than twice as many lower-SES persons as high-SES persons reporting high symptom levels (Eaton and Ritter 1988).

Finally, studies of the distribution of depressed mood and distress as assessed through commonly used symptom scales (e.g., Center for Epidemiological Studies Depression Scale) show an inverse relationship with indicators of SES such as education and occupational standing (Link and Dohrenwend 1980; Mirowsky and Ross 1986) with very few exceptions (Kohn et al. 1990). Taken together, this evidence suggests that, in general, an inverse relationship exists between depression and SES.

THEORETICAL MODELS

We investigate the possibility that experience in occupations involving direction, control, and planning (DCP) partially explains the relationship between SES and depression. Our reasoning is that advantaged SES increases access to occupations involving these factors and that access to such occupations increases a general sense of mastery and control over life's challenges. Finally, mastery and control reduce the chances of developing depression. Figure 1, model A depicts these processes as well as some alternative processes that we will consider in our analysis (fig. 1, models B and C). We proceed by developing in greater detail the conceptualization of the evidence for each connection specified in figure 1, model A.

Support for the Theoretical Model

SES and occupational direction, control, and planning.—The connection between SES and occupational conditions is fundamental. As Kohn and Schooler (1983) have shown, for example, strong associations exist between father's SES, respondent's education, and respondent's job prestige and occupational conditions such as job complexity, routinization, and closeness of supervision. Against this background it is reason-

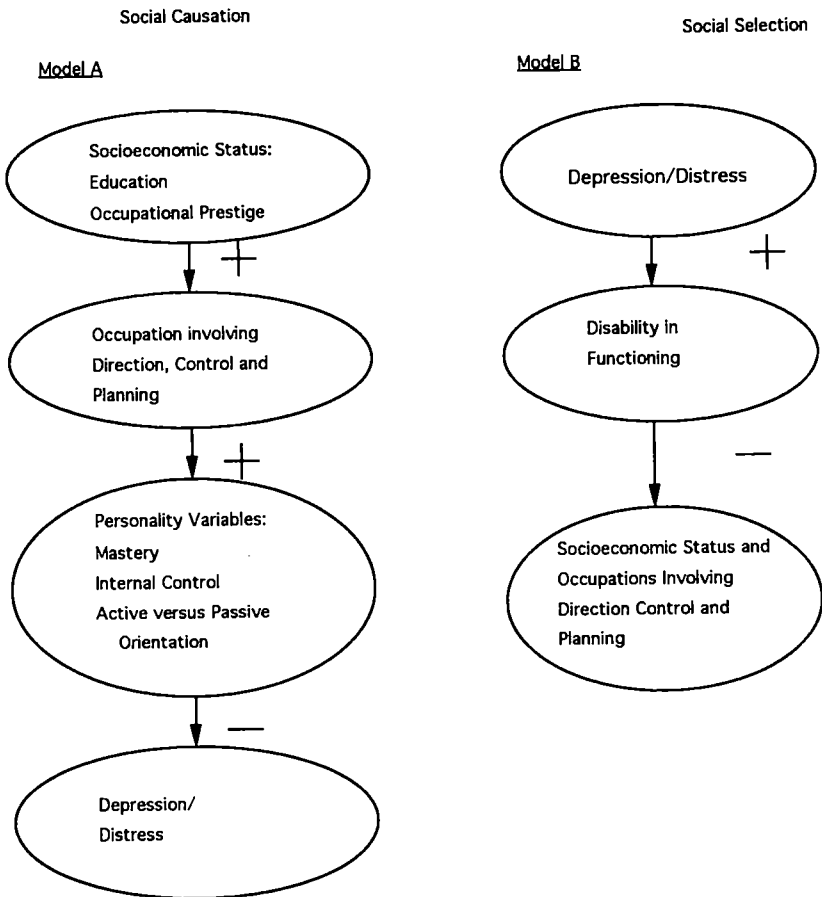


FIG. 1.—Theoretical models A and B, linking SES, occupational conditions, personality characteristics, and depression/distress.

able to hypothesize that SES will be associated with the occupational characteristic, “direction, control, and planning.” As a person obtains socioeconomic credentials, such as a college education, he or she is more likely to have access to occupations that allow these elements.

Occupational control and perceived personal control.—We argue that direction, control, and planning of occupational activities will foster a sense of mastery and personal control. Previous research provides some evidence for a link between occupational control and personal control. For example, Kohn and his colleagues (Kohn et al. 1990; Kohn and Schooler 1983) show that occupations that permit self-direction enhance self-directed personality orientations. These studies, however, like other

research linking occupational conditions with personality, focus on control over one's own work activities. The importance of power, or control over others' work activities, has been relatively neglected in studies of the psychological consequences of jobs (Kasl 1989).

Our measure of occupational control assesses control over others' work activities. It differs from occupational self-direction in that occupational control is coded as absent in the *DOT* when it is exercised only over one's own situation. However, involvement in the direction, control, and planning of others implies self-direction.² Thus the main difference between our measure and a measure of occupational self-direction is the inclusion of control over others' work activities. Occupations requiring direction, control, and planning involve both occupational self-direction and control over others; therefore, the link between DCP and perceived personal control may operate via either (or both) of two mechanisms.

Underlying the first mechanism is the fact that people who direct, control, and plan for others are likely to have control over their own work. Thus, in keeping with Kohn and Schooler's (1983) reasoning, occupational self-direction would explain any connection between the *DOT*'s measure of DCP and perceived personal control. According to this reasoning, workers in occupations characterized by self-direction must respond actively to repeated challenges encountered in the work environment. Doing so enhances an active orientation to life's problems and engenders confidence in one's ability to respond effectively to future challenges. In support of this idea, Kohn and Schooler (1983) demonstrate that occupational self-direction—that is, having a job that is complex, is not closely supervised, and does not involve routine tasks—is a significant determinant of psychological functioning. Specifically they found that occupational self-direction leads to greater intellectual flexibility, to a self-directed personal orientation, and to less psychological distress and anxiety. These firm findings were obtained with controls for baseline characteristics in a longitudinal design that allowed them to test for the opposite effect of personality characteristics on recruitment into occupations involving self-direction.

The second mechanism we consider derives directly from the "control over others" aspect of the measure of DCP. For one thing, having control over others entails having the resources to attain goals through the work of others. This situation implies an expanded ability to get things done

² This assumption is supported by data from our sample of community residents (described below). Only two of 106 occupations involving direction, control, and planning (DCP) are below the median on substantive complexity, a key component of Kohn and Schooler's (1983) assessment of occupational self-direction. This suggests that the vast majority of occupations that involve the direction, control, and planning of others' work also involve self-direction.

and thereby an enhanced sense of personal efficacy and control (Kanter 1976).

In addition, we observe what might be termed a labeling effect. When one person directs, controls, and plans the activities of another, each draws conclusions about his or her ability, not only for the specific task at hand but for other situations as well. Evidence for this proposition comes from a series of experimental studies conducted by Langer and her colleagues (Langer and Benevento 1978; Langer and Rodin 1976). In these studies, investigators assigned subjects randomly to "boss" and "worker" roles and then required them to perform an assigned task together. Apparently, this intervention induced differential expectations of competence in the boss/worker dyad because "bosses" subsequently performed better than "workers" in an unrelated task. Moreover, when "workers" were asked whether they typically were good at tasks like the one assigned, they showed less confidence in their ability than did the "bosses." It seems that when people observe themselves performing more or less well than others, they draw inferences concerning their more general abilities to perform effectively (see also Berger, Wagner, and Zelditch 1985). On the basis of this evidence we expect that people who direct, control, and plan the activities of others tend to be more confident that they can control life's exigencies than people who are directed, controlled, or planned for.

Thus the link between occupational direction, control, and planning of others' work and perceived personal control might be explained through mechanisms that are consistent with the benefits of having occupational self-control, with the benefits of having control over others, or both. To assess the relative importance of each would require more detailed measures of the processes specified by each than are available; therefore we leave this step to future research. Our main point here is that both theoretical and research findings support a connection between occupations involving direction, control, and planning and a personal orientation of mastery and self-direction. The next issue becomes whether the belief that one can control situations, once in place, is related to depression.

Perceived control and depression.—An association between perceived control and depressive symptoms has been found again and again in surveys assessing depressive symptoms (see Mirowsky and Ross 1986). Moreover, measures of personal control have been shown to account for or otherwise enhance our understanding of why variables like SES, life events, and gender are related to depression. For example, Ross and Mirowsky (1989) argue that the association of income and education with depressive symptoms derives partially from the fact that low-SES people feel less control over their life circumstances than do more highly educated people with higher incomes. Further, Wheaton (1980, 1983) has

shown that "fatalism," a concept he measured with items reflecting personal control, was strongly related to symptoms of depression particularly when exposure to recent stressful life events was high. Similarly, Rosenfield (1989) showed the relevance of a sense of personal control to explaining some apparent inconsistencies in gender differences in symptoms of depression and anxiety. In her study married women with little power (defined as low income relative to their husband's) or excessive demands (full-time employment and responsibility for child care) have higher symptom levels than men. Both of these conditions seem likely to engender a sense of having little control over one's life circumstances. In keeping with this reasoning, Rosenfield showed that the association between these two conditions and psychological distress can be explained by a sense of personal control. On the basis of this literature, there is good reason to expect that the influence of direction, control, and planning on a sense of personal control will help explain why occupational conditions of control are related to depression (see fig. 1, model A).

Alternative Models

Although the model proposed above is plausible, it would be challenged by proponents of social selection. Instead of a chain of causal links beginning with SES and ending with depression, selection theorists could maintain that depression and proneness to depression lead to low SES. For example, impairments in social functioning caused by depression (Broadhead et al. 1990; Wells et al. 1989) might lead to poor status attainment and low SES (see fig. 1, model B). According to a more complicated hypothesis, genetic vulnerabilities (Goldin and Gershon 1988) or early traumas (Dohrenwend et al. 1986) would impair status attainment and simultaneously predispose to depression (see fig. 2, model C).

Thus, in assessing the plausibility of our social causation model, we also evaluate these competing social selection alternatives. We test the first social selection explanation, model B, by comparing three groups: (1) people experiencing their first episode of depression, (2) people experiencing a recurrence of depression, and (3) people who have never been seriously depressed. Critical to the test is the fact that the occupation assessed for the depressed groups is the one held before the onset of the first (group 1) or most recent (group 2) episode of depression. Because model B specifies that depression causes and therefore must precede occupational position, one would expect an association between depression and direction, control, and planning only when the initial onset of depression preceded the assessment of occupational position—the recurrent-episode cases (group 2). First-episode cases (group 1), whose relevant

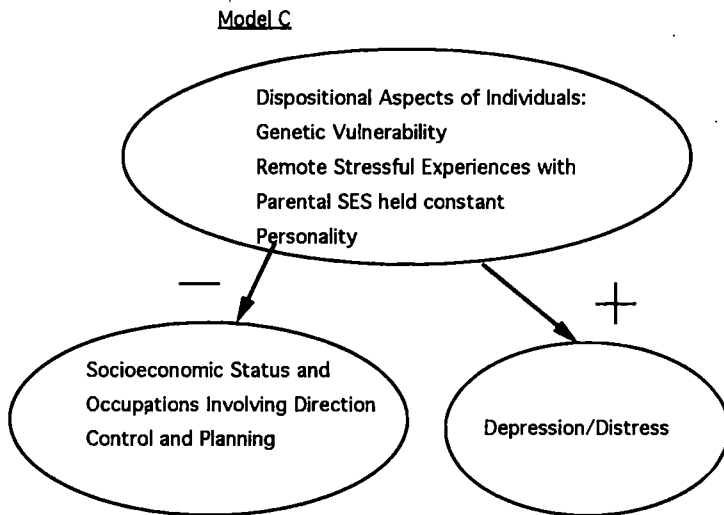


FIG. 2.—Theoretical model C, linking SES, occupational conditions, personal-ity characteristics, and depression/distress.

occupations predate the development of depression, should have occupations much like those of persons who have never been seriously depressed. If occupational conditions cause depression, however, as specified in model A, we would expect to find an association between depression and occupations characterized by direction, control, and planning among group 1 and group 2 when compared to the nondepressed respondents (group 3).

The second social selection approach is depicted in model C (fig. 2). It would receive its strongest support if personal vulnerabilities render the SES-depression association spurious. Important vulnerability factors from the selection perspective include genetic predisposition, problematic personalities, or, with parental SES held constant, early stressors such as illnesses or being raised in a single parent family.³

We examine the plausibility of the predictions made by these models with data from two different types of studies. The first is a case-control study involving persons who experienced recent episodes of major depres-

³ Remote life-threatening illness or injury and being raised by a mother alone are considered social selection factors rather than causation factors because, when parental SES is held constant, these variables could lead to poor status attainment and depression. In this scenario the adult SES-depression association would be spurious—not the result of causation. Of course, insofar as parental SES leads to exposure to these conditions, the association would be interpreted as causation. For this reason we specify these variables as selection variables with parental SES held constant.

sion and controls selected from a community sample. The second is a community-based cross-sectional study that uses a symptom scale of "demoralization" (Dohrenwend, Shrout, et al. 1980). Rather than assessing the presence or absence of a specific disorder, this symptom scale is a continuous measure of "non-specific psychological distress" (Dohrenwend, Shrout, et al. 1980, p. 1229) that correlates very highly with scales like the Center for Epidemiological Studies Depression Scale (Shrout, Skodal, and Dohrenwend 1986).

METHOD

Samples

Between 1980 and 1983 we administered two face-to-face interviews to samples of community residents ($N = 429$) and psychiatric patients with DSM III (APA 1980) diagnoses of major depression ($N = 98$) from the Washington Heights section of New York City (Dohrenwend et al. 1986). We recruited the community respondents initially to participate in a methodological study of symptom scales, and we interviewed them again approximately six months later for this research. In the original community sample, we enumerated and then canvassed households to learn whether an eligible respondent between 19 and 59 years of age lived there. We also obtained information about ethnic background to permit us to sample roughly equal proportions of blacks, Hispanics, and non-Hispanic whites from this urban neighborhood, in which most residents are Hispanic. In 93% of the households, screening information was provided; 68% of these contained one eligible respondent or more ($N = 943$). Of these, 57% (541) were interviewed successfully.

The original methodological study involved random assignment of respondents to one-month or one-year recall of symptoms from the Psychiatric Epidemiology Research Interview (PERI). Given the methodological focus, we made no intensive efforts to interview hard-to-schedule respondents or convert refusals. When we conceived and implemented the substantive study near the end of the methodological study and when questions of generalizability became more compelling, we made special efforts to interview a subsample of hard-to-schedule subjects. This group accounted for 48 of the 541 respondents in our total sample.

We also attempted to ensure an adequate reinterview rate in the second wave of data collection. As a result, we located and reinterviewed 79% of the initial sample (429). When compared to census data for the same area, data for this reinterviewed group showed no difference in gender and age (within the 19–59 age range). Differences emerge on educational level and ethnicity, but these can be attributed largely to the fact that

we purposely undersampled the Hispanic majority. Because Hispanics have less education than either blacks or non-Hispanic whites, our sampling decision contributed to an underrepresentation of less well educated respondents. Moreover, it was more difficult to recruit such respondents; probably they are also underrepresented in the community sample for this reason. Because of the stratification, we include controls on ethnicity in all the following analyses.

To assess further the representativeness of the community sample, we compared the small sample of converted refusals and hard-to-schedule respondents with the rest of the sample. We reasoned that these respondents would be more similar to the larger pool of nonrespondents than to the more easily obtained respondents because they would have been nonrespondents without our extensive efforts. We found no statistically significant difference between this hard-to-obtain subsample and other respondents on age, gender, education, or ethnicity.

The patient sample was selected from outpatient and inpatient facilities in the same area of New York City. Because our goal was to select patients with major depression, all patients thought to be experiencing symptoms consistent with this disorder were referred to our project. The patients were diagnosed independently according to DSM III criteria (APA 1980). These diagnoses then were reviewed systematically by members of the Biometrics Department at the New York State Psychiatric Institute and were changed if they were thought to be in error (see Skodol et al. 1984). We made considerable effort to locate people in their first episode. In the end, we interviewed 98 cases with major depression, 49 of whom were experiencing their first episode of this disorder. We cannot give a participation rate for the patient cases because the diagnosis was made only after the patient was recruited; thus we have no denominator to use in calculating such a rate. Very few patients refused outright to participate.

Study Designs

We use both a case-control and a community cross-sectional design to investigate our hypothesis. Our outcome in the case-control design is a diagnosis of major depression ($N = 122$). Most of the "cases" ($N = 98$) came from the patient sample described above. We also screened the 429 community residents for high levels of psychological distress as measured by the demoralization scale and asked master's level health professionals to interview these high scorers with a modified version of the Diagnostic Interview Schedule (DIS) (Robins et al. 1981). Diagnoses were made according to DSM III criteria by researchers from the Biometrics Department at New York State's Psychiatric Institute. In this manner we identi-

fied 24 community residents with current major depression, none of whom were experiencing their first episode of disorder. For eight of the 122 cases we were unable to assign a *DOT* code because of missing occupational information; thus 114 remained for analysis.

In analyses of the case-control component of our study, we use as controls 331 community residents (occupational data are missing for five subjects) who are neither highly distressed nor clinically depressed. We do this to maximize contrasts with both the depressed group (most of whom have high scores on the scale of psychological distress) and the highly distressed but not clinically depressed group. Constructing the control group in this manner, however, could compromise our ability to investigate whether DCP is related to DSM III major depression *per se*. If control over work were related only to distress, a difference between the depressed cases and a control group from which cases high on distress had been removed could be an artifact of the presence of distressed persons in the depressed case group (Schwartz and Link 1989). We hypothesize effects on both distress and major depression; to establish these effects, we always check to see whether the depression results hold when the control group is expanded to include the highly distressed but not the clinically depressed community respondents.

The community cross-sectional design involves the full sample of 429 community residents and uses the PERI demoralization scale as a measure of psychological distress. Because of missing data on occupation for 7 cases and on distress for 2, 420 cases are available for the cross-sectional analysis.

Measures

Direction, control, and planning.—The measure of direction, control, and planning (DCP) is one of the 44 *DOT* ratings derived from on-site assessments by occupational analysts at the U.S. Department of Labor (1972). In all, the *Dictionary of Occupational Titles* (U.S. Department of Labor 1977) includes 12,099 distinct occupational descriptions, each of which is associated with a nine-digit code. In order to assign data from the *DOT* to subjects in our sample, we obtained information about the respondent's most recent occupation by asking about current occupation or, if the respondent was unemployed, most recent occupation.⁴ For the case groups composed of persons with recent episodes of major depres-

⁴ We used most recent occupation for persons who are not currently in paid-work jobs (e.g., unemployed persons and homemakers) so that we would not have to exclude them from the sample. However, as described later, we also check our results by excluding respondents who are not currently involved in work for pay.

sion, we used the same procedure but selected the occupation held when the episode occurred or, if the respondent was unemployed, the last occupation held before the onset of the episode. This procedure was designed to ensure that occupational exposure preceded the episode of psychiatric disorder. On the basis of the information about occupations, subjects received the nine-digit *DOT* identifiers by the consensus of two coders.

We chose the *DOT* rating of direction, control, and planning rather than one of the two "substantive complexity" measures that were derived from the same source by Cain and Treiman (1981; see also Lennon 1987) and Kohn and Schooler (1983, p. 75). Our theory requires a measure of occupational control; even though substantive complexity measures suggest occupational control, the DCP rating was the most direct measure available in the data. As the reader will recall, this measure assesses control over others' work but also typically involves occupational self-direction (see n. 2 below). The DCP measure, however, is related to substantive complexity as well as to other features of occupations. We deal with this potential overlap between various *DOT*-based measures by controlling for them in our analysis.

A shortcoming of the *DOT* ratings, including the measure of DCP that we employ, arises from the fact that the *DOT* misses variability within occupations. Therefore our *DOT* measure is restricted to between-occupation variance; its association with other variables may be attenuated as a result. At the same time, this drawback is offset by the objective nature of the *DOT* ratings. In retrospective studies such as ours, in which people are asked to recall conditions of their lives before their depressive episode, one must be extremely careful in interpreting self-reports. Respondents may exaggerate negative aspects of their life circumstances and may forget positive aspects or relegate them to secondary importance in an effort to provide an accounting to themselves or others of reasons for psychological problems (Raphael 1987). Moreover, clinical features of depression may lead to a particularly negative view of one's life including one's ability to direct, control, and plan in an occupation. Because the *DOT* ratings of occupational conditions are made by objective observers, however, they are immune to these kinds of distortion.

In the *DOT*, the rating of direction, control, and planning is a dichotomy. We assign a value of "1" to occupations that the *DOT* rates as involving these activities and "0" to occupations that it does not. Only a minority (25.1%) of the occupations in our community sample are rated as involving DCP. In order to illustrate the types of occupations that received this rating, we list, in table 1, eight occupations reported by our sample respondents that the *DOT* includes in this category. For comparison we also selected eight occupations of roughly equal prestige (using Treiman [1977] scores) that do not involve direction, control, and plan-

TABLE 1
EIGHT OCCUPATIONS THAT INVOLVE DCP AND EIGHT OF ROUGHLY
EQUAL PRESTIGE THAT DO NOT

| Involve DCP | Do Not Involve DCP |
|--|-----------------------------------|
| Supervisor, garment manufacturing | Building inspector |
| Manager, industrial cafeteria | Booking manager |
| Civil engineer | Financial analyst |
| Industrial engineer | Aircraft inspector |
| Principal | Food products tester |
| Postmaster | Employment interviewer |
| Production superintendent | Editorial assistant |
| Director, community health nursing | Hospital insurance representative |

ning. As table 1 suggests, most occupations involving direction, control, and planning are relatively high in SES. In fact, 90% of such occupations in our full community sample are white collar and a majority of the incumbents (54%) are college graduates. The table also implies the importance of the "control over others" aspect of direction, control, and planning that distinguishes DCP from occupational self-direction. Note that most of the relatively high-prestige jobs that do not involve DCP are likely to allow occupational self-direction but do not involve control over others' work activities.

Our sample reveals no gender difference in exposure to occupations involving direction, control, and planning either in terms of a bivariate association (men 26%, women 25%) or when age, education, and prestige are controlled. This finding suggests that control over work cannot be tested in our sample as a possible explanation of the frequently observed gender difference in depression/distress.⁵

⁵ Given the importance of gender in depression rates, we examined this relationship in those jobs that involve DCP and in jobs that do not. We found no gender difference within occupations that involve DCP: 62.5% of the cases of major depression in these occupations and 50% of the highly distressed group were female, compared to 55.4% of the controls. This finding contrasts with a sharp gender difference among persons who do not hold occupations involving DCP: 76.3% of depressed cases and 72.4% of the highly distressed group were female, compared to 52.3% of the controls. Thus, the major part of the gender difference in depression in this sample occurs among people whose occupations do not involve DCP. In keeping with our theory about the beneficial effects of occupational control, this finding suggests that when women experience control in the occupational sphere, they are no more likely than men to experience major depression or high levels of distress. Let us recall, however, that in this sample there is no gender difference in rates of exposure to occupations involving DCP. Thus possible protective effects of occupational control cannot explain why

The relationship of DCP to other occupational measures.—In order to investigate the role of DCP, one must examine the degree of overlap between this and other measures of occupational conditions. If other occupational conditions are strongly related to this measure, they could account for its hypothesized relationship to major depression and psychological distress.

Cain and Treiman (1981) conducted a factor analysis using a 10% sample of *DOT* occupations. The analysis identified six factors that they called “substantive complexity,” “motor skills,” “physical demands,” “management,” “interpersonal skills,” and “undesirable work conditions.” We conducted a factor analysis using our respondents’ actual occupations and, with the exception of Cain and Treiman’s “interpersonal skills,” identified similar factors.⁶ In our factor analysis, the DCP rating had comparatively low loadings on the factors that emerged (the highest was .53 on the complexity factor), and only 45% of its variance was explained by all of the factors combined. In contrast, an item such as “complexity dealing with data” had a much higher loading of .86 on “complexity”; a full 77% of its variance was explained by the factors. Therefore, it would appear that, to a substantial degree, the direction, control, and planning rating measures something that differs from the phenomena included in the five factors we identified. Nevertheless, DCP is not unrelated to our factor-analytically derived measures. In the community sample, occupations entailing direction, control, and planning are less likely to be physically demanding ($r = -.286, P < .001$) and to involve noisome (undesirable) conditions ($r = -.138, P < .05$). They are much more likely to be substantively complex ($r = .565, P < .001$) and to involve the management of others ($r = .456, P < .001$).

In view of the significance of substantive complexity in the conceptualization of self-direction (as opposed to control over others) in Kohn and Schooler’s (1983) work, it is important to note that they developed a

women in this sample have higher overall rates of depression than men. It may be that men experience more control than women in their marriages and families. Thus, in contrast to women in low-control occupations, men in such occupations may have greater access to control and power in other domains.

⁶ We scored the factors by creating summative scales from items that had the highest loadings. The five factors consisted of the following (*DOT*) ratings: (1) substantive complexity = general educational development, intelligence, specific vocational preparation, complexity of function in relation to data, verbal aptitude, and numerical aptitude; (2) motor skills = finger dexterity, motor coordination, complexity dealing with things, manual dexterity, form perception, and seeing; (3) physical demands = climbing, eye-hand-foot coordination, working outside, lifting, and stooping; (4) management = dealing with people, talking, and scientific vs. business contact; (5) noisome work = extreme cold, extreme heat, humidity, noise, hazards, and atmospheric conditions.

different *DOT*-based measure of "substantive complexity" from the one derived by Cain and Treiman. They used the *DOT* ratings of complexity dealing with data, people, and things to predict their survey-based measure of substantive complexity in a multiple regression analysis. We used the scoring scheme that they derived from this analysis to construct a second measure of substantive complexity (Kohn and Schooler 1983, p. 75). Like the Cain and Treiman measure of substantive complexity, the measure derived from Kohn and Schooler correlated significantly with DCP ($r = .347$, $P < .001$). The relatively strong correlations between DCP and other features of occupations underscore the need to control for these related features in our analysis.⁷

Dependent variables.—We have already described how we defined the case groups of individuals with either first episode or recurrent major depression. Our other dependent variable is a continuously scored 27-item measure of psychological distress or demoralization ($\alpha = .93$; see Dohrenwend, Shrout, et al. 1980). This measure includes symptoms experienced during the past six months such as helplessness-hopelessness, low self-esteem, sadness, psychophysiological problems, and other features commonly included in scales of psychological distress. We use this composite of seemingly different symptoms because research reported by Dohrenwend, Shrout, et al. (1980) has shown that scales measuring these subcomponents are highly correlated with each other in community samples. In fact, they frequently correlate as highly as their reliabilities permit, which suggests that very little unique variance is associated with these subcomponents. Consistent with this is Shrout et al.'s (1986) report, which is based on a subsample of the same subjects used in this study. It shows that demoralization correlates .78 with the widely used Center for Epidemiological Studies Depression (CES-D) scale (about as highly as the reliabilities of the two measures permit).

The demoralization scale, like other measures of psychological distress, can change with changing circumstances, but it also shows considerable stability in samples of people measured at two or more points in time

⁷ Management of others is conceptually distinct from DCP. It is possible to have responsibility for managing others but little opportunity to direct, control, or plan the conditions under which such management occurs. Also, from our vantage point, simply managing others without occupational control actually could be stressful and certainly would not protect against depression. Examples of this type of occupation that we encountered in our sample are salesperson, police officer, and receptionist. Occupations in our sample that involve DCP but do not call for the management of others are accountant and engineer. Thus the classification of occupations as high management without DCP and low management with DCP seems to make sense. As a result, our prediction about the predominance of DCP as a separate risk factor for depression is testable.

(e.g., Link et al. 1990). In this respect, the demoralization scale can be thought of as having "trait" and "state" components much like the measures of personality that we describe below. We include it as a dependent variable rather than as a personality variable because its conceptual and empirical overlap with major depression is much greater than the overlap of the personality variables considered below.

Sociodemographics and SES indicators.—When appropriate, we control for age, gender, ethnicity (black, Hispanic, and other), and education measured in years. We assess socioeconomic origin by asking respondents about their fathers' usual occupation. If the respondent had no knowledge of his or her father or was raised by his or her mother only, we used the occupation of the major caretaker. We defined first occupation as the first full-time job held for six months or more in order to exclude summer jobs and part-time jobs held while in school. We assigned Treiman (1977) prestige scores to father's occupation, respondent's first occupation, and respondent's most recent occupation.

Personality attributes.—We use three measures of personality attributes. Rotter's (1966) internal-external locus of control scale ($\alpha = .70$) asks respondents to choose which of two statements they believe to be more true. For example, they are asked to choose between the statement "What happens to me is my own doing" and the statement "Sometimes I feel that I don't have enough control over the directions my life is taking." The mastery orientation ($\alpha = .75$) and emotional passivity ($\alpha = .70$) scales are taken from the Spence-Helmreich (1978) measures of masculinity and femininity (see the Appendix for items included in these two measures).

These variables may operate as traits predisposing individuals to depression. Alternatively they may be regarded as changing with changing experiences and circumstances throughout adult life. For example, the work of Kohn and Schooler (1983) demonstrates that personality characteristics are influenced by social conditions in general and by occupational conditions in particular. Accordingly we recognize the possibility that our personality measures have both stable components and components that vary with changing situations, a point to which we will return.

Potential confounding variables.—These are antecedent factors indicating genetic vulnerability or early stressful experience (with SES held constant) that not only might limit the ability to obtain jobs involving direction, control, and planning but also might indicate a predisposition to depression. These would include family history of mental disorder, being raised in a household where only the mother was present and where there was no knowledge of the father, and remote life-threatening illnesses and injuries. Although this is not a comprehensive list of all possible confounding variables, previous research with this sample has

shown that these variables are strongly related to depression (Dohrenwend et al. 1986).

We generated the family history measure (Dohrenwend et al. 1986) by asking each respondent whether his or her first-degree relatives had ever had "serious mental or emotional problems such as problems with depression, suicide attempts, odd or violent behavior, or difficulties with drugs or alcohol." Using responses to this query and information about family members' psychiatric treatment, we made a dichotomous rating of family history present/absent based on the consensus rating of two psychiatrists. We also created a more refined measure of family history that included subcategories of affective disorder (e.g., depression, anxiety), psychotic disorder, and drug and alcohol problems. We use the more general family history measure in the analysis because the more refined measures do not significantly improve the prediction of depression or distress above and beyond the effect of the more general measure.⁸

Being raised in a mother-headed household with no knowledge of one's father is indexed by a dichotomy with "1" for yes and "0" for no. As McLanahan (1985) pointed out, such circumstances involve the likely loss of the father's contribution to family income, the lack of a male role model, and/or exposure to stressful circumstances associated with family discord or disruption.

The measure of remote life-threatening illnesses and injuries is created from a question that asks respondents to list the three most serious illnesses or injuries they ever had. They also were asked whether these illnesses still bothered them (if they did so, we excluded the conditions as not being "remote") and whether the health problems were "life threatening." Respondents with at least one remote life-threatening illness or injury were scored "1" whereas all other respondents were scored "0."

RESULTS

We start by examining the plausibility of the connections specified in model A (see fig. 1). Our first question is whether the inverse association between socioeconomic status and depression/distress is present in our samples.

⁸ Concerning the more refined measures, we note some evidence for specific effects. For example, a history of affective disorder (e.g., depression, anxiety) predicts depression and not schizophrenia spectrum disorder in our case-control study. Also, the dummy variable assessing a family history of affective disorder is the strongest predictor of depression and distress of the more refined measures. Most important, the DCP measure remains significant no matter which measure of family history is employed.

SES as a Risk Factor for Depression/Distress

In keeping with past research, psychological distress shows a statistically significant inverse association with both education ($r = -.183$, $P < .01$) and occupational prestige ($r = -.146$, $P < .01$) in the community sample. Major depression in the community sample also is associated inversely with both education (college graduate, 5.0%; less than high school, 7.5%) and occupational prestige (top third of prestige scale, 4.5%, to bottom third, 6.2%). Neither relationship is statistically significant, however, because statistical power is poor: only 24 cases of current major depression occur in the community sample.

Because current cases of major depression are rare in our community sample and recent first-episode cases are nonexistent, we rely on the patient cases from the case-control portion of the study. We compare three groups derived from community and patient samples: (1) individuals scoring in the top 20% on our measure of distress (excluding those with current DSM III major depression), (2) cases of major depression from the case-control study (including community cases), and (3) all other community residents.

Table 2 shows that depressed cases and those scoring high on distress have fewer years of education and lower occupational prestige than other community residents. These effects hold (results not shown) with controls on age, gender, and ethnicity. When the cases of major depression are broken into subgroups based on first-episode patient cases, repeat-episode patient cases, and community cases (none of whom were in their first episodes), we find results that are consistent with this overall picture. Although the smaller groups do not always differ significantly from the group of community "nondistressed" persons, they are always lower on

TABLE 2

ASSOCIATION BETWEEN DEPRESSION/DISTRESS AND INDICATORS OF SES

| Groups | N of Cases | Mean Years of Education | Mean Treiman Prestige Scores |
|--|------------|-------------------------|------------------------------|
| Cases scoring in top 20% on distress but not meeting DSM III criteria for major depression | 65 | 11.4 (3.9) | 39.2 (13.0) |
| Cases meeting DSM III criteria for major depression, including 24 community cases | 114 | 12.4 (3.8) | 40.6 (12.5) |
| | | 13.3 | 43.5 |
| Community control group | 331 | (3.2) | (13.0) |

NOTE.—Groups connected by brackets are significantly different at $P < .05$. Numbers in parentheses are SDs.

both SES indicators. Therefore the distributions of major depression and distress according to SES indicators in our groups pose the problems of explanation that have grown out of the larger literature.

SES and Occupational DCP

If model A is to be plausible, SES must be related to occupations involving DCP. This appears to be the case in the community sample. Consider first the association between prestige of father's (or major caregiver's) occupation and respondent's tenure in occupations involving direction, control, and planning. Respondents whose fathers' occupations are in the top third of the prestige continuum are one and one-half times more likely (35% vs. 23%) to have occupations involving direction, control, and planning than respondents whose fathers are in the bottom third. The respondent's own SES is associated even more strongly with direction, control, and planning: college graduates are three times as likely as noncollege graduates (48% vs. 16%) to have occupations involving this element. Respondents whose occupational prestige places them in the top third of the distribution are more than four times as likely as those in the bottom half (49% vs. 11%) to have occupations involving DCP. Stated in terms of correlations with occupational control, the coefficients are .302 and .371, respectively, for years of education and for prestige. Although substantial, these correlations suggest that DCP does not overlap with SES to a degree that would preclude examining it as a separate factor. Thus, DCP is an SES-linked but empirically distinct occupational characteristic.

Occupational DCP and Depression/Distress

Model A also portrays a relationship between direction, control, and planning and major depression/distress. In keeping with this model, figure 3 shows that only 15% of the community residents who are highly distressed and 14% of the persons with major depression have jobs that involve occupational control compared to 27.8% of the nondistressed community residents.

Because most of the cases in figure 3 are patients, we first investigate whether these results are a function of selection into treatment. In the bottom half of figure 3, major depressed cases are separated into three groups: cases from the community sample, repeat-episode cases, and first-episode cases. The community depressed group and the distressed group were not selected from treatment settings. These groups are similar to the first- and the repeat-episode samples of patient cases in percentages

Role of Occupations

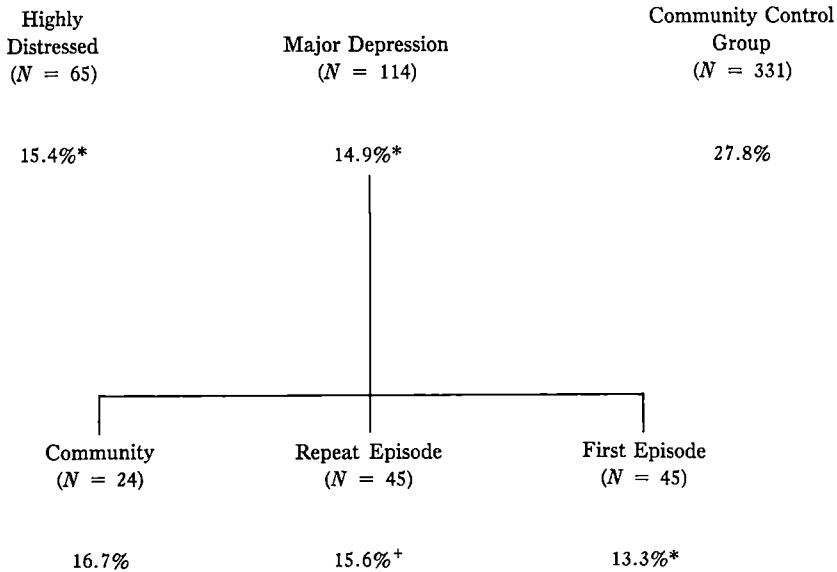


FIG. 3.—Percentage of respondents holding occupations involving DCP in depressed, highly distressed, and control groups. (+ = group significantly different from low-distressed, nondepressed at $P < .10$; * = groups significantly different from low-distressed, nondepressed at $P < .05$. No other pairs of groups are significantly different from one another.)

having occupations involving DCP and are dissimilar to the community control group. Selection into treatment therefore is not a parsimonious explanation of the pattern of results in figure 3.

Is it possible, however, that the association is present because major depression is a severely disabling disorder? Perhaps, as selection theory would predict (see fig. 1, model B), people who have had a major depressive episode cannot function in jobs involving DCP; consequently they would leave or would not enter such jobs. If this is so, we would expect to find a strong difference in occupational control between repeat- and first-episode cases, in which the first-episode cases would seem more like the controls. Note, however, that repeat- and first-episode cases do not differ in their involvement in occupations that call for DCP. Even when these persons never experienced an earlier major depressive episode, the occupations they held before they developed the disorder were far less likely to involve DCP than were those of controls. This finding is inconsistent with the selection explanation depicted in model B. Thus, as an explanation of our results, model C is left as the most plausible alternative to model A.

SES, Control over Work, and Depression

Our next concern is to investigate whether experience in generally high SES occupations that involve DCP accounts for the association between SES and depression/distress as suggested by model A. If this were so, we would expect the inverse association between SES and depression/distress to be diminished when the DCP variable is held constant. In conducting this test, we perform separate analyses with case-control and with community cross-sectional samples. In the first analysis, we use logistic regression to assess the effect of risk variables on the odds of being a case of major depression ("1") as opposed to being a control ("0"). In the second, we use multiple regression to take advantage of the continuous nature of the outcome variable of distress.

As table 3 shows, the results of the two analyses provide consistent support for the hypothesized explanatory role of occupational control. The SES indicators always have smaller regression coefficients when DCP is controlled; in two of the four instances the association goes from

TABLE 3

DOES DCP "EXPLAIN" THE ASSOCIATION BETWEEN SES AND DEPRESSION/DISTRESS?^a

| | CASE-CONTROL DESIGN PRESTIGE (N = 444) | | COMMUNITY CROSS- SECTIONAL DESIGN PRESTIGE (N = 420) | |
|--------------------------|--|--------------------|---|--------------------|
| | Alone | Controlling DCP | Alone | Controlling DCP |
| Prestige | -.018* (.009) | -.013 (.010) | -.007** (.003) | -.005* (.003) |
| DCP | . . . | -.721* (.311) | . . . | -.184* (.077) |
| | YEARS OF EDUCATION | | YEARS OF EDUCATION | |
| | Alone | Controlling DCP | Alone | Controlling DCP |
| Years of education | -.081* (.035) | -.059 (.037) | -.035** (.010) | -.029** (.010) |
| DCP | . . . | -.715* (.308) | . . . | -.178* (.075) |

NOTE.—Numbers for case-control design are logistic regression coefficients; for community cross-sectional design, OLS regression coefficients are shown. Numbers in parentheses are SEs.

^a Because the sample was stratified by ethnicity, this variable is controlled. Its nonsignificant effects are not shown.

* $P < .05$.

** $P < .01$.

statistical significance to nonsignificance. The percentage decline in the magnitude of the coefficients ranges from 29% for prestige (community cross-section) to 17% for education (community cross-section). The hypothesized intervening variable—DCP—remains significant in each analysis with SES held constant.

Alternative Explanations for Connections between SES, DCP, and Depression/Distress

Explanations based on selection.—According to the theory portrayed in model C, some people perform poorly in status attainment and are predisposed to depression because they have characteristics (e.g., genes, physical handicaps) or have been exposed to unfortunate circumstances that were not caused by SES (e.g., a disfiguring or disabling accident). The result is a spurious association between current SES and depression/distress. We have two direct tests of the plausibility of this model. Both use factors that precede current occupational conditions and current depression/distress and therefore fit the causal order proposed in model C.

In the first test we make an assumption, consistent with selection theory (model C), that respondents who achieve lower current status than one would expect on the basis of parental SES and their own early attainments (education and prestige of first job) are handicapped with respect to dispositional factors that may cause depression. By contrast, persons who obtain occupations of higher than expected prestige are assumed to be relatively advantaged with respect to these dispositions. Then we ask whether status attainment as an indirect indicator of the dispositional factors can explain our principal finding concerning the association between depression/distress and occupational DCP.

In the second test, we use direct measures of some of the dispositional factors depicted in model C and enter them as control variables. Specifically we include measures of family history of mental disorder (with father's prestige held constant), remote life-threatening illness or injury, and being raised by a mother alone. Insofar as model C is correct, we would expect that controlling these variables would diminish sharply the association between depression/distress and DCP.

Table 4 shows the outcome of the two tests for each study design. Columns 1 and 4 show the unadjusted effect of DCP on major depression in the case-control design and on distress in the cross-sectional design, respectively. Columns 2 and 5 add variables that control for SES and (more important) the tendency to achieve higher or lower than expected current occupational prestige (i.e., SES change). We include in the "SES and SES change" component all of the available measures relevant to this domain: father's or caretaker's occupational prestige, prestige of

TABLE 4
THE EFFECT OF DCP ON DEPRESSION/DISTRESS, CONTROLLING FOR STATUS ATTAINMENT AND VARIABLES CONSISTENT
WITH A SOCIAL SELECTION MODEL^a

| | CASE-CONTROL DESIGN (N = 444) | | | COMMUNITY CROSS-SECTIONAL DESIGN (N = 420) | | |
|--|--|---|-----------------------------------|---|---|-----------------------------------|
| | DCP Controlling Ethnicity (1) | Controlling SES and SES Change (2) | Full Set of Controls (3) | DCP Controlling Ethnicity (4) | Controlling SES and SES Change (5) | Full Set of Controls (6) |
| Possible causation mechanism: | | | | | | |
| DCP | -.822** (.301) | -.676* (.317) | -.922* (.364) | -.229** (.073) | -.179* (.078) | -.183* (.075) |
| SES and SES change: | | | | | | |
| Father's occupational prestige | | .009 (.009) | .006 (.010) | | -.000 (.002) | .000 (.002) |
| Years of education | | -.066 (.042) | -.035 (.052) | | -.031** (.012) | -.031** (.012) |
| Prestige of first full-time occupation | | .005 (.011) | .013 (.013) | | .005 (.003) | .003 (.003) |

| | | | | |
|--|-----------------|--------------------|-----------------|-------------------|
| Current occupational prestige | -.009 (.011) | -.014 (.013) | -.004 (.003) | -.003 (.003) |
| Other control variables: | | | | |
| Age (in years) | | -.019 (.011) | | -.007** (.003) |
| Sex ^b | | .889** (.292) | | .238** (.062) |
| Marital status ^b | | -1.298** (.277) | | -.107 (.060) |
| Remote life-threatening illness and injury | | .837** (.270) | | .137* (.064) |
| Raised by mother alone | | 1.064* (.537) | | .110 (.158) |
| Family history of mental disorder | | 1.909*** (.277) | | .217*** (.062) |
| χ^2 , <i>df</i> | 9.56, 3 | 14.15, 7 | 172.11, 13 | |
| R^2 | | .031 | .063 | .155 |

NOTE.—Numbers for case-control design are logistic regression coefficients; for community cross-sectional design, OLS regression coefficients are shown.

^a Because the sample was stratified by ethnicity this variable is controlled. Its nonsignificant effects are not shown.

^b Sex is scored "1" for female, "0" for male; marital status is scored "1" for married, "0" for other.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

respondent's first full-time occupation, respondent's education, and prestige of respondent's current or last occupation. These controls incorporate and thus hold constant the effects of background SES, starting position, current position, and change between these points in the status attainment process (cf. Kessler and Greenberg 1981). Therefore they provide the strictest test we can devise to determine whether SES and the tendency to do better or worse than expected in the status attainment process can explain the association between depression/distress and direction, control, and planning. Finally, columns 3 and 6 show results that add family history of mental disorder, remote illness and injury, and being raised by a mother alone; these variables, according to model C, should lead away from occupations involving DCP and toward depression. Age, gender, and marital status also are added at this step to ensure a model that has controls that are as complete as possible.

The key result in table 4 is that the effect of direction, control, and planning on major depression remains significant and relatively constant across these controls. Even with the full battery of controls (col. 3), the case-control results show that a person whose occupation involves DCP is much less likely to be depressed than a person without such an occupation (adjusted odds ratio = .398, antilog of $-.922$). The ordinary least squares (OLS) regression coefficients for occupational control on distress in columns 4–6 also remain significant in each step. The distress score of a person with an occupation involving DCP is more than one-quarter of a standard deviation (SD) lower ($-.183$) than that of a person without such an occupation.

The association between depression/distress and occupations involving DCP was not explained by either approach to assessing the importance of dispositional factors depicted in model C—that is, neither by a general tendency to do better or worse than expected in status attainment nor by the effects of family history, remote illness and injury, or being raised by a mother alone. If the outcome had been explained in one of these ways, it would have been convincing evidence for model C because both of the approaches that we employed indexed factors that precede current occupational conditions and any current episode of major depression. These findings exhaust the relatively clear-cut tests of model C, tests that would have led us to choose model C over the causation approach depicted in model A. As we will see, however, certain results that are not interpreted so easily still could support model C and prevent us from ruling it out definitively.

We checked the robustness of the occupational control effect reported in table 4 by varying some aspects of the analysis. The result holds when we exclude from the sample people who were not employed—that is, people whose reported occupation was their most recent occupation

rather than their current occupation. The result also holds in the case-control part of the study in which we include distressed community residents in the control group. Finally, we find no significant difference between the effect of DCP on depression/distress in males and in females. In both genders the regression coefficients are negative, an indication that a job involving occupational control is associated with a reduced risk of depression/distress for males and for females.

Competing occupational characteristics.—It is possible that DCP is associated with SES and depression/distress because it is linked closely to other aspects of occupations that play a larger role in depression. To investigate this possibility, we examine the degree of physical demands, noisome conditions, motor skills, management, and two measures of substantive complexity (Cain and Treiman 1981; Kohn and Schooler 1983). In no case did the entry of one of these occupational variables reduce the DCP coefficient to nonsignificance ($P > .10$); in 10 of the 12 instances tested (two outcome measures and six occupational variables) the coefficient was significant ($P < .05$). If all the occupational variables are entered together into an equation containing only those control variables that significantly predict depression/distress, DCP is significant for depression in the case/control design ($P < .10$) and for distress in the community cross-sectional design ($P < .05$).⁹ More important, with only one exception, there was no occupational variable other than DCP that approached significance in predicting depression/distress in these analyses. The exception was that people in occupations requiring motor skills were more likely to be in the depressed group in the case-control design. Unlike the situation for DCP, we found no evidence that this effect was consistent for distress as well as for major depression; the motor skills variable was entirely unrelated to distress in the cross-sectional design. Direction, control, and planning is the only occupational characteristic we measured that is related consistently to depression/distress independent of other occupational characteristics.

Occupations Involving DCP, Personality Variables, and Depression

The foregoing analyses show that exposure to occupational conditions involving DCP may help explain why SES is related to major depression.

⁹ While the regression coefficients for DCP decline in magnitude somewhat when the other occupational conditions are controlled, much of the decline in significance is due to the increase in the SE when collinear variables are entered. For example in the case-control design the coefficient for DCP without controls is $-.822$. With all significant controls and the other occupational conditions added, it is $-.647$, a decline of 21.3%. For the same comparison the SE increases by 33.6%, from .301 to .402.

Even so, we have not investigated the full set of relationships portrayed in model A. Specifically, we should be able to identify personality characteristics and orientations that might be affected by occupational conditions and that might be related, in turn, to depression/distress. To do so, we investigate whether personality characteristics substantially reduce the direct (unique) effect of DCP on depression/distress. If direct effects are reduced, we have evidence for an explanation that is consistent with the data and that tells us, at least in part, why DCP is related to depression/distress. The same pattern of results, however—direct effects that decline with the entry of personality variables—is potentially consistent with model C as well.

Table 5 shows the effect of entering mastery (Spence and Helmreich 1978), internal-external locus of control (Rotter 1966), and emotional passivity (Spence and Helmreich 1978) on the association between depression/distress and DCP. For both the case-control and the cross-sectional studies, these three variables sharply diminish the regression coefficient of DCP on depression/distress, rendering it nonsignificant in both instances. In the case-control design, the coefficient for DCP drops by 39% (from $-.922$ to $-.562$); in the community cross-section it decreases 44% (from $-.183$ to $-.102$).

Although these results are consistent with the social causation explanation shown in model A, they also are consistent with a selection explanation in which personality is postulated as an antecedent factor that (1) determines both access to and tenure in occupations involving DCP and (2) simultaneously places individuals at risk for depression/distress (see fig. 2, model C). Unlike the situation in our previous tests of model C, in which we examined factors antecedent to depression/distress and current occupation, the personality factors investigated here cannot be placed clearly in time before occupational conditions and depression. As a result, there is no reason to choose selection (model C) over causation (model A). In the discussion, we consider in detail the plausibility of a causation explanation (model A) in light of this ambiguity.

DISCUSSION

The Meaning of the Findings from a Social Causation Perspective

Our results are consistent with the social causation processes portrayed in model A but do not confirm them fully. People at the low end of the socioeconomic continuum are less likely to hold occupations that involve DCP; in turn, the absence of this occupational characteristic is associated with major depression and distress. Moreover, when we statistically held constant DCP, the inverse association between SES and depression/dis-

TABLE 5

THE EFFECT OF MASTERY, PASSIVE EMOTIONALITY, AND INTERNAL/EXTERNAL LOCUS OF CONTROL ON THE ASSOCIATION BETWEEN DCP AND DEPRESSION/DISTRESS^a

| | Case-Control Design (<i>N</i> = 444) | Community Cross-Sectional Design (<i>N</i> = 420) |
|--|---|---|
| Possible causation mechanism: | | |
| DCP | -.562 (.400) | -.102 (.068) |
| Control variables: | | |
| Father's occupational prestige | .007 (.011) | -.001 (.002) |
| Years of education | -.021 (.057) | -.021 (.011) |
| Prestige of first full-time occupation | .008 (.014) | .004 (.002) |
| Current occupational prestige | -.012 (.014) | -.001 (.003) |
| Age (in years) | -.014 (.013) | -.007** (.002) |
| Sex ^b | .235 (.330) | .048 (.059) |
| Marital status ^b | -1.555*** (.317) | -.126* (.054) |
| Remote life-threatening illness and injury | .731* (.300) | .089 (.058) |
| Raised by mother alone (no knowledge of father) | 1.437* (.606) | .033 (.143) |
| Family history of mental disorder | 1.789*** (.305) | .202*** (.056) |
| Personality attributes: | | |
| Mastery | -1.433*** (.269) | -.307*** (.049) |
| Passive emotionality | .771*** (.211) | .201*** (.041) |
| External locus of control | 1.466 (.916) | .818*** (.168) |
| χ^2 , <i>df</i> | 180.51, 16 | ... |
| <i>R</i> ² | ... | .327 |

NOTE.—Numbers for case-control design are logistic regression coefficients; for community cross-sectional design, OLS regression coefficients are shown. Numbers in parentheses are SEs.

^a Because the sample was stratified by ethnicity, this variable is controlled. Its nonsignificant effects are not shown.

^b Sex is scored "1" for female, "0" for male; marital status is scored "1" for married, "0" for other.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

tress declined. This finding suggests that occupational DCP explains the SES/depression association, at least in part. Further, the DCP variable was unique among the variety of occupational characteristics in showing an independent relationship to both major depression and distress. Finally, our data were consistent with an explanation of the association between depression/distress and DCP that holds that occupational control influences personal feelings of control, which in turn influence depression/distress. This connection clarifies the association between depression/distress and DCP by focusing on variables closer to depression in the hypothesized causal chain to explain why SES and occupational conditions have the effects they have.

Generally, social causation arguments focus on adverse social situations that propel lower-status people toward mental illness. By contrast, DCP is conceived most appropriately as a protective factor that reduces the risk of developing depression and/or distress among incumbents of some high-status occupations. Accordingly this factor calls attention to positive features of the social environment, at least in regard to inoculation against depression. Moreover if this reasoning is correct, it may help explain the finding that higher-status persons are less likely than lower-status persons to become distressed when they are exposed to life events (Brown and Harris 1978; Langner and Michael 1963; Turner and Noh 1983). Perhaps social conditions such as occupational control foster the development of a personal sense of mastery that facilitates adaptive coping in the face of stressful events. Conceiving of occupational control as a protective factor that is consistent with a social causation approach opens this and other avenues for future research.

Explanations Based on Selection

We undertook three tests of the selection-based explanations depicted in models B and C (figs. 1 and 2). In the first, we assessed the plausibility of model B, which suggests that disability due to depression causes downward mobility or a failure to rise in occupational status; thus it hampers movement into or causes movement out of occupations involving DCP. According to this model, the association between major depression and DCP should exist only for repeat episode cases who have experienced the disabling effects of major depression. We found, however, that the antecedent occupations of first-episode cases were similar to those of repeat-episode cases in being considerably less likely than the occupations of the control group to involve DCP.

The second and third tests concerned model C. In one of these tests we controlled a respondent's tendency to achieve higher or lower than expected occupational status in order to determine whether such a control

could explain the association between depression/distress and DCP. In the other test we controlled for potent antecedent risk factors for depression/distress that might render spurious the association between depression and DCP. We found, however, that the association was unchanged as a consequence of these controls. Any one of these three tests could have led us to reject the social causation explanation depicted in model A. The fact that none did so means that we found no compelling evidence to reject this explanation.

Alternative Explanations Involving Personality Measures

In sharp contrast to the other variables we controlled to test model C, the personality variables cannot be placed unambiguously before the attainment of occupation and the occurrence of depression. Thus the personality variables can be used from either a social causation or a social selection viewpoint to explain the association between depression/distress and DCP. We address this ambiguity by seeking ancillary evidence on the plausibility of the alternative models. Of particular concern is the plausibility of concluding that at least some of the association between SES and depression/distress arises through social causation processes (model A).¹⁰

Personality variables as concomitants of major depression.—One possible cause of ambiguity about the personality variables is that when people are severely depressed their personality orientations may change; they show decreased mastery and heightened emotional passivity, external locus of control, and distress. Insofar as this situation occurs these problems can be viewed as part of the depressive syndrome. If they are in fact part of the depressive syndrome, we cannot claim to have identified personality factors as the mechanism explaining why DCP affects depression. Moreover, controlling for personality characteristics in an equation predicting depression/distress would lead to an underestimation of the effect of DCP on depression. The social causation explanation set forth in model A would not be undermined, but our reasoning about the mechanism explaining why occupations affect depression/distress would be proved incorrect.

Some evidence bearing on this issue is available from an eight-year follow-up of 52 of the cases of depression used in this study. Analysis of

¹⁰ In evaluating these alternative models we do not mean to imply that one is the "right one" and must entirely account for the association between depression/distress and DCP. In fact, it is likely that some combination is involved, with personality characteristics as concomitants, antecedents, and consequences of occupational characteristics.

these data suggests that the personality measures are in part concomitants of depression and in part predictors of depression.¹¹ This latter finding leads us to consider whether personality is more likely a confounder in the association between depression/distress and DCP (model C) or an intervening variable that explains the mechanism through which DCP influences depression/distress (model A).

Personality as a factor in social selection.—We could increase our confidence in the personality measures as antecedent factors that render spurious the association between depression/distress and DCP if the personality measures were linked to clearly antecedent selection factors. In particular, we can ask whether the personality variables explain the potent effects exerted on depression/distress by variables such as family history, remote life-threatening illness and injury, or being raised by a mother alone. Specifically, if the regression coefficients for these variables are reduced when the personality variables are entered, the reduction would suggest the possibility that the personality variables explain why the antecedent selection factors have such strong effects on depression. Such results would add to our confidence in model C. When we compare the effects of family history, of remote illness and injury, and of being raised by a mother alone in tables 4 and 5, we see that adding the personality variables (in table 5) has little impact on the size of the coefficients of these variables. Only one of six associations goes from significance to nonsignificance (remote illness and injury for distress), whereas four remain significant and of roughly the same magnitude. Thus the personality variables do not appear to be part of the causal chain linking potent selection variables to depression. Although model C is not ruled out by these results, it is supported less strongly than it might have been.

In addition, as we showed, access to occupations involving DCP is determined in part by socioeconomic origins; persons of high-status origin

¹¹ Persons who were episode-free eight years later ($N = 13$) were significantly higher on mastery, significantly lower on passive emotionality, and somewhat less external on the Rotter scale at follow-up than they had been at baseline. Much less change occurred among persons who were symptomatic on follow-up ($N = 40$): unlike initially depressed persons whose episode status changed, no significant changes in mastery or passive emotionality occurred, with the exception of a marginally significant decrease in external orientation. The fact that personality scores tend to change when a respondent is not in an episode, but change less and on different variables when the respondent's episode status has not changed, supports the interpretation that the personality variables are concomitants of depression. Using logistic regression, however, we also found that baseline assessments of personality predict significantly whether subjects who are depressed at baseline are in episodes at follow-up. Although all three variables are in the expected direction, the Rotter scale is by far the most powerful: the odds of being in an episode at follow-up increase 2.8 times with each SD-unit increase in externality. This finding suggests a possible causal role for these variables, particularly external locus of control.

(top one-third in parental prestige) are 1.5 times as likely as persons of lower-status origin to currently hold an occupation involving DCP. This finding suggests that at least one major social factor antecedent to respondents' personality determines selection into occupations involving direction, control, and planning.

Personality as a mechanism of social causation.—In order to rule out social causation entirely, we would have to account for all of the associations between occupational control and personality by confirming that personality causes access to, and tenure in, occupations involving DCP. Let us recall, however, the studies we described that have a bearing on this issue. Experimental research, which fixes the direction of cause, has shown that positions that allow (or deny) control, such as the boss (or worker) role, can influence perceived personal control dramatically. Kohn and Schooler's program of nonexperimental longitudinal research leads to the same conclusion. These studies, which estimate the reciprocal effects of occupational conditions and personality, have found consistently that some of the effect is from occupational conditions to personality (Kohn and Schooler 1983). Because strong research indicates that occupations can affect personality, it seems highly unlikely that all of the association between depression and DCP is the spurious result of personality causing both of these variables.

CONCLUSION

At the beginning of our article, we noted that a central theoretical proposition of sociology is that social structural positions influence life chances. In keeping with this assumption, we presented a theoretical model linking SES, occupational direction, control, and planning, personality factors, and depression. Our analysis supports the plausibility of this model. First, our empirical findings were consistent with the model: the SES/depression association was explained by DCP and the DCP/depression association was explained by personality factors. Second, several plausible alternative explanations that could have led us to reject the causation model failed to do so. The only remaining challenge—the possibility that personality causes both occupational conditions and depression—was ambiguous; it hinged on the relative effects of occupation on personality and of personality on occupation. Here, we argued, the preponderance of the evidence allowed us to conclude that, to some degree, occupational DCP protects against depression.

A clear direction for future research is to conduct studies of occupational control and depression that use prospective designs and employ more detailed measures of DCP. Prospective studies, particularly those in which cohorts of young people are assessed before they enter the job

market and then again after they have been exposed to occupations (e.g., Jenkins 1987), will allow the assessment of reverse or reciprocal causation. In addition, future research would do well to disaggregate and measure separately the components of DCP that are related to depression/distress, a process that is not possible with the present highly aggregated *DOT* rating. Additional inquiries such as these will test further the validity of the place we have assigned to occupations involving DCP as a protective factor in SES-related processes that contribute to the causation of depression.

APPENDIX

TABLE A1

OPPOSITE ADJECTIVES IN THE SEMANTIC DIFFERENTIAL FORMING THE MASTERY
AND PASSIVE EMOTIONALITY SCALES

| Low | High |
|--|------------------------------------|
| Mastery: | |
| Not at all independent | Very independent |
| Very submissive | Very dominant |
| Very passive | Very active |
| Not at all competitive | Very competitive |
| Has difficulty making decisions | Can make decisions easily |
| Gives up very easily | Never gives up easily |
| Not at all self-confident | Very self-confident |
| Feels very inferior | Feels very superior |
| Goes to pieces under pressure | Stands up well under pressure |
| Passive emotionality: | |
| Not at all emotional | Very emotional |
| Not at all excitable in a major crisis | Very excitable in a major crisis |
| Indifferent to others' approval | Highly needful of others' approval |
| Feelings not easily hurt | Feelings easily hurt |
| Never cries | Cries very easily |
| Very little need for security | Very strong need for security |

SOURCE.—Spence and Helmreich (1978).

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Minority Proximity to Whites in Suburbs: An Individual-Level Analysis of Segregation¹

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A novel method for locational analysis at the individual level is used to analyze the determinants of proximity to non-Hispanic whites separately for Asians, blacks, Hispanics, and for non-Hispanic whites themselves. The resulting regression analyses, for which the percentage of non-Hispanic whites in a community serves as the dependent variable, reveal how the familiar *P** segregation measure is generated through locational patterns that map racial/ethnic-group members with specific personal and household characteristics into communities with specific majority-group proportions. The analyses are developed from two complementary theoretical models—spatial assimilation and place stratification—and applied to the suburban communities of the nation's largest metropolitan region, surrounding New York City, as of 1980. Consistent with the place-stratification model, proximity to non-Hispanic whites is very different for members of the white and black groups and little affected by their individual characteristics other than race. By contrast, Asians and Hispanics appear more consistent with the spatial-assimilation model.

Residing in proximity to whites is a critical indicator of assimilation for minorities, yet much evidence indicates that there is, in the aggregate, substantial minority segregation from whites in the United States (e.g., Massey and Denton 1987). A traditional perspective views residential segregation as entailing a dearth of informal contact between minority

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and majority populations, which promotes prejudice and is itself a *prima facie* sign of incomplete assimilation (e.g., Gordon 1964; Williams 1964). In addition, segregation is necessary to the stratification processes that inhere in such locational differentials as unequal schools serving different neighborhoods or unequal levels of services provided by different localities; these in turn are implicated in racial and ethnic inequality (Farley and Allen 1987; Logan and Schneider 1984; Massey, Condran, and Denton 1987). By this reasoning, an analysis of the conditions under which segregation breaks down should offer important insights into the processes of minority incorporation and assimilation.

In broad terms, suburbanization appears to be linked with a decline in segregation for American minorities—namely, Asians, blacks, and Hispanics—although the degree of reduction is quite variable across groups and metropolitan areas (Massey and Denton 1988). This linkage is tied to important shifts in the spatial patterning of minorities, who are increasingly found in suburbs rather than central cities, the classical settings of ethnic enclaves (Alba and Logan 1991; Frey and Speare 1988; Long and DeAre 1981; Waldinger 1989). Thus, changing residential patterns can be expected to result in lower overall segregation, although it is only for Asians and Hispanics that the level of suburban segregation is typically in the low to moderate range (Massey and Denton 1988). For blacks, the level of suburban segregation remains relatively high, even if it is, on average, notably lower than in central cities.

As the black case suggests, much remains to be learned about the precise connection between suburbanization and residential desegregation. It is clear from studies of black suburbanization that suburban residence is not inevitably linked to substantially greater proximity to whites: black suburbanites are so often found in suburbs with high proportions of nonwhites (as well as with other distinctive characteristics) that the term *reghettoization* might be applied (Guest 1978; Lake 1981; Logan and Schneider 1984; Stearns and Logan 1986a). But it is not clear under what conditions suburbanization for blacks means greater proximity to whites rather than continued segregation from them. There is even less knowledge about this issue for Asians and Hispanics.

One way to approach the issue is through an analysis of *locational attainment* within suburbia. We mean the term to denote an analysis that would focus on how the characteristics of individuals—in this case, of minority-group suburbanites—are linked to some feature of their communities—in this case, the percentage of non-Hispanic whites among residents. Such an analysis reveals how members of different groups convert such characteristics as income or English-language proficiency into proximity to the majority group and thus identifies the conditions under which suburbanization brings desegregation. Although this ap-

proach has much to recommend it, it has been difficult to implement in the study of desegregation because the most complete data about small areas, those of the U.S. Census of Population and Housing, are only available in aggregate form (see Massey and Denton 1988).

In this article, we rely on a novel method of constructing individual-level ordinary least squares (OLS) regressions, in part by using aggregate data. Applying this method, we analyze the process determining proximity to non-Hispanic whites separately for Asians, blacks, and Hispanics (as well as for non-Hispanic whites themselves, who are included for comparative purposes). Our analysis is conducted with 1980 census data for the suburban portion of the New York City Consolidated Metropolitan Statistical Area (CMSA), the largest metropolitan region in the nation as well as one of the most ethnically and racially diverse.

THEORETICAL BACKGROUND AND PREVIOUS RESEARCH

Two complementary theoretical models inform our analysis. The model of *spatial assimilation*, a continuation of the ecological tradition, views the spatial distribution of groups as a reflection of the state of their assimilation, broadly construed (Massey 1985). The basic tenets of the model are that residential mobility follows from the acculturation and social mobility of individuals and that residential mobility is an intermediate step on the way to more complete—that is, structural—assimilation (Massey and Mullen 1984). As members of minority groups acculturate and establish themselves in American labor markets, they attempt to leave behind less successful members of their groups and to convert socioeconomic and assimilation progress into residential gain, by “purchasing” residence in places with greater advantages and amenities. This process disperses minority-group members, opening the way for increased contact with members of the ethnic majority, and thus for desegregation.

In the spatial-assimilation model, entry into relatively advantaged suburban communities that contain many whites is a key outcome. It is seen principally as the outgrowth of individual-level processes: as individuals acquire human capital and acculturate, improving their ability to interact socially with members of the majority, their likelihood of achieving residence in advantaged suburbs increases. Thus, the locational process should be described well by individual-level variables, such as income, education, and English-language ability. In this way, the model subsumes a common, admittedly partial explanation for segregation; namely, that groups differ on average in the characteristics, especially income, that determine location (for the debate about this explanation in the black case, see Clark [1986, 1988] and Galster [1988]).

The model of *place stratification* addresses directly the hierarchical differentiation within suburbia and its reflection in the mapping of groups across suburban space. The model posits that places are ordered hierarchically and consequently are associated with more or less favorable life chances and quality of life for the people who reside in them (Logan 1978). This is as true within suburbia as it is between central cities and suburbs, on average. In addition, the hierarchy of places is seen as a means by which more advantaged groups, for example, affluent non-Hispanic whites, seek to preserve social distance from less advantaged ones. The place-stratification model thus subsumes frequently stated explanations for segregation that focus on mechanisms of public and private discrimination (see Farley and Allen 1987, pp. 150–55; Galster 1988). These mechanisms are complex and include a variety of individual and institutional actions, ranging from acts of violence against the pioneer minorities in a community to restrictive zoning (Logan and Stearns 1981; Rieder 1985; Shlay and Rossi 1981). These generate racially segmented housing markets (Foley 1973; Berry 1975; Stearns and Logan 1986*a*), which make it difficult for minorities to enter some communities and add to the cost they must pay for housing.

The place-stratification model thus envisions that racial and ethnic minorities are sorted by place according to their group's relative standing in society, and this limits the ability of even the socially mobile members of these groups to reside in the same communities as comparable whites. The model obviously indicates that group membership must be taken into account in analyzing locational processes. In addition, it implies that the members of some groups are not able fully to convert socioeconomic and assimilation gains into residence in the same communities as the majority; in other words, the "returns" on individual achievements, such as income and English-language ability, may differ substantially across groups. In effect, it "costs" members of some groups more to achieve desirable locational outcomes, if they are able to achieve them at all. We would expect this extra burden to be found especially in racially stigmatized groups, such as blacks and, among Hispanics, Puerto Ricans (on the higher segregation of Puerto Ricans compared to other Hispanics, see Massey and Bitterman [1985]).

These are not the only theoretical conceptions that might be entertained in a study of segregation. Another possibility, which our analysis cannot directly address, is that minority segregation is, at least in part, voluntary—that, even in suburbs, minority-group members seek out communities with disproportionate numbers of fellow group members. Support for this notion is found in Schelling's (1971) argument that high levels of racial segregation can arise from more modest group differences

in preferences for neighborhood composition (see also Clark 1991). Support can also be inferred, although not without ambiguity, from the model of the enclave economy (Portes and Bach 1985; cf. Sanders and Nee 1987; Portes and Jensen 1987), which envisions connections among ethnic solidarity, spatial concentration, and economic success. Nevertheless, although we cannot directly test the voluntary conception of segregation here, it is hard to imagine that a rigorous test of this or any explanation of residential patterns is possible without a method for accurately estimating individual-level effects. We will demonstrate such a method, which can be applied with widely disseminated U.S. census data sets.

Previous studies of segregation patterns, which have been formulated usually at the aggregate level and have focused only rarely on the segregation within suburbia, can be read as supporting both assimilation and stratification models. The key is that the models vary in their applications to different groups. In analyses at the metropolitan-area level, for example, Massey and Denton (1987, 1988; see also Denton and Massey 1988; Hwang et al. 1985; White, Biddlecom, and Guo 1991), find important differences among the segregation patterns of Asians, blacks, and Hispanics. In all regions of the country, in cities and in suburbs, blacks are most segregated from whites, while Asians, despite the recency of their immigration, tend to be least segregated (see also Farley and Allen 1987). Beyond these differences in its levels, segregation appear to respond differently to assimilation and socioeconomic variables in the three groups. Black segregation is resistant to socioeconomic improvement, in contrast to the more varied profile of Asian and Hispanic segregation. The segregation of the latter groups has been found to be affected by both assimilation and socioeconomic characteristics (Massey and Denton 1987, pp. 819–21).

However, the precise configuration of effects in past research appears to be sensitive to equation specification and segregation measure. For instance, in contrast to the findings reported above, Hwang et al. (1985) find only modest support for the proposition that Hispanic segregation is reduced by socioeconomic improvement. This sensitivity is likely to be, at least in part, a result of problems associated with ecological inference, that is, a function of the aggregate level of past analysis and the resulting difficulties inherent in inferring individual-level effects. The only published analyses that avoid these inferential difficulties are by Villemez (1980), Massey and Denton (1985), and by Gross and Massey (1991), but, while they are at the appropriate, that is, individual, level, these analyses also employ a limited range of independent variables. A limited number of independent variables is also characteristic of aggregate-level analyses and is probably attributable to the multicollinearity existing among ag-

gregate variables, which constrains variable choice. For example, no model we have encountered has included home ownership among the independent variables, even though it is obvious that ownership, and the wealth it reflects, may be implicated in the locational patterns behind spatial segregation (Clark 1986).

METHOD

The Locational-Attainment Model

The two theoretical models outlined earlier can best be addressed by a model of the following form:

$$Y_{ij} = a + b_1X_{1ij} + b_2X_{2ij} + b_3X_{3ij} + \epsilon_{ij}, \quad (1)$$

where Y_{ij} is a community-level measure of racial composition for community j (say, the percentage of non-Hispanic whites among residents) and thus is assumed to be constant for all i in the same j (i.e., for all individuals in the same community); X_{1ij} is the household income (or another human-capital variable) for individual or household i in community j ; X_{2ij} is English-language ability (or another assimilation measure); and X_{3ij} represents other (possibly control) variables, such as household configuration or age. The model is estimated for the individual or the household as the unit and separately by racial/ethnic group. Each b coefficient, then, establishes the degree of conversion of a minority individual's or household's characteristics, such as income, into a residential setting with low segregation (i.e., a community with many whites or, alternatively, many nongroup members). Estimating effects of this sort has, in fact, been a rather consistent goal of research on residential processes (see, e.g., Villemez 1980; Massey and Denton 1985; Massey et al. 1987; White et al. 1991).

Under the assumption that high scores on the dependent variable mean low segregation (true when that variable is the percentage of non-Hispanic whites in a community), the spatial-assimilation model would lead one to expect that both X_1 and X_2 have positive effects (i.e., $b_1, b_2 > 0$). In its ideal-typical form, it also supports the expectation that these effects are quite similar across groups. The place-stratification model, on the other hand, calls attention to variations in the intercept or, alternatively, in predicted values from the models, since it posits that individuals who have the same human-capital and assimilation values will be located in settings of markedly different segregation in accordance with their racial/ethnic group membership. This model also leads to expectations about differences in b coefficients. In an ideal-typical reading of the model (e.g., an apartheid system), racial/ethnic group membership is determinative of segregation, and therefore other personal characteristics of group

members should have little effect on their exposure to the majority population. One would, therefore, expect the b coefficients to express small effects and, at the limit, none at all.

The alternatives just stated are extremes. Another possible pattern is that the intercept—and the predicted y values for low x values (e.g., low incomes)—are smaller for a minority group than for the majority, but that at least some of the minority's b coefficients are larger. Viewed from one perspective, this situation would appear to fit an empirically plausible scenario for assimilation: although some members of a minority, especially those with poor English, low incomes, and so on, are disadvantaged compared with similar whites (as indicated by the difference in intercepts and predicted y values when x values are low), minority-group members with improved personal characteristics begin to catch up (implied by their greater returns on variables such as language and income). Viewed from another perspective, however, this situation can be interpreted in terms of the stratification model. In brief, even though some whites may be disadvantaged in terms of income or other characteristics, they are locationally advantaged by virtue of their racial/ethnic membership (i.e., they have better locational outcomes than similar minority-group members). For minority-group members to achieve the same locational outcome as whites, they generally must have superior individual characteristics—in other words, it costs them more. In the abstract, both these interpretations seem plausible. Empirically, perhaps the question can be settled on the basis of whether minorities achieve *equal* outcomes with similar whites for some subset of values on language, income, and other characteristics. If so, then one would favor the assimilation model, if not, if whites are always more advantaged in locational terms than their minority peers, then the stratification model should get the nod. This, in fact, is the criterion we will follow in drawing conclusions.

The Dependent Variable: Percentage of Non-Hispanic Whites in a Community

The dependent variable, a measure of racial/ethnic composition, can be understood in two ways. The first is as one of a series of community characteristics that influence the life chances and quality of life of residents (in which case, the assumption above of constancy for all individuals in a community is unproblematic). This view is particularly congenial to the place-stratification model, according to which a community's racial and ethnic composition is a factor that tends to affect its chances in the competition over resources (e.g., tax base), in maintaining desirable living conditions (e.g., low crime rate), and in attracting new residents (especially affluent whites). Such a view gains credibility from the re-

search by Logan and Schneider (1984) that demonstrates that suburban communities with high percentages of black residents generally have a high population density, are proximate to the central city, have a weak property tax base and limited services, and experience residential instability and further growth in the minority population. Other research shows that suburbs with large minority populations also tend to have high crime rates (Stahura, Huff, and Smith 1980).

In the second view, community-wide racial/ethnic composition is a measure of the proximate composition of the neighborhood in which an individual resides. In this case, the assumption that Y_{ij} is constant for individuals in the same community is obviously more problematic, since, at a micro level, segregation varies within communities, not just across them. Nevertheless, we believe that a community-wide measure is tenable as an approximation to the composition of smaller neighborhoods. One way of demonstrating this is to change the level of aggregation and observe if the results are affected, and this we have done. The results we report here are based on a measure of racial/ethnic composition at the level of the suburban place (more will be said below about this level of aggregation); but we have also computed analyses using an areal unit about 40% as large in population terms, the census tract, and found largely similar results (reported in Logan, Alba, and Fisher [1992]).

We have chosen the percentage of non-Hispanic whites as the measure of racial composition to analyze here because it links the analysis to a well-known measure of segregation, ${}_xP_y^*$, the asymmetric exposure index (Lieberman and Carter 1982), which is generally interpreted as the probability that members of group x have residential "contact" with group y . The exposure index plays an important role in the literature on segregation because, as Massey and Denton (1987, p. 806) point out, it measures "the extent to which minority and majority members must physically confront one another by virtue of sharing a common tract of residence."²

² As is well known, the P^* family of indices is sensitive to group size in a region (Stearns and Logan 1986b). One reviewer has accordingly raised with us the possibility that any differences we detect in the locational patterns of groups may be a function of group size. The reviewer reasons that, if all groups have a similar tendency to form residential clusters, the clusters of larger groups will exert a more powerful influence on our dependent variable and thus on the results of our modeling procedure. Of course, if group size in a region does influence the pattern of segregation, this may be an interesting result, not a confounding influence. The point suggests two strategies of attack. The first is to analyze a number of different metropolitan regions in which the same groups are of different sizes and then test whether the regression results vary in accordance with group size. We are following this strategy in the larger project of which this article is a part (ultimately, our analysis will encompass the 11 largest metropolitan regions in the nation). A second strategy is to vary the level of aggregation at which the dependent variable is calculated. As that level becomes smaller, the

In our case, group y is identified with non-Hispanic whites. Further, the mean of our dependent variable, when calculated at the individual (not aggregate) level for the members of a particular group, is the value of P^* (multiplied by 100) across communities and can be interpreted as the average exposure of members of that group to non-Hispanic whites; this follows directly from the standard formula for ${}_xP_y^*$.³ When whites are the group in question, then the mean is the same as the so-called isolation index. A multivariate analysis taking the form above can thus be viewed as an analysis of the P^* index in terms of its individual-level determinants for different groups. As such, it also directly addresses an issue fundamental to both theoretical models that has mostly been addressed indirectly or at an aggregate level in segregation research: To what extent can segregation be explained by compositional, or "endowment," differences among groups? (See, e.g., Jaynes and Williams 1989, pp. 144–46; the term "endowment," which refers here to a group's average level of location-relevant characteristics such as income, is taken from Jones and Kelley [1984].) If endowment differences account for segregation, in whole or in part, then differences among intercepts, or among predicted segregation levels based on the model above, should be smaller than differences among P^* values for different groups.

Estimating the Model

One feature of the analytical model that must be recognized in order to address the difficulties involved in estimating it is that it requires variables from different levels—aggregate and individual. The dependent variable is a community characteristic; the independent variables, however, are at the individual level or the household level. To be sure, other familiar sociological models also have such a cross-level character: this is true, for example, of the status-attainment model (Blau and Duncan 1967), since occupational prestige, the dependent variable in the model,

clusters of smaller groups should have a greater effect on the values of the dependent variable, and the results obtained for these groups should increasingly resemble those obtained for larger ones. As already noted, we have carried out an analysis based on the census tract, a smaller population unit than we use here. The results are quite similar to those we report here, suggesting that group size is not a confounding factor.

³ The standard formula (Massey and Denton 1987, p. 806) is:

$${}_xP_y^* = \Sigma(x_i/X)(y_i/t_i),$$

where x_i and y_i are the numbers of members of groups x and y , respectively, in community i , X is the total population size of group x , and t_i is the population size of community i . Note that this formula amounts to the weighted average of the proportion of group y in each community, where the weighting factor is the community size of group x .

is a characteristic of an occupational group and is predicted by the characteristics of individual incumbents. The theoretical reasoning that connects the dependent variable to the independent variables is also similar in both cases: the individual characteristics are presumed to determine access to the groups (occupations or communities) by some market-based sorting or allocation process. In the status-attainment model, education affects occupational access because some occupations require a certain level of education and, in other cases, employers prefer to hire individuals with higher levels of educational attainment. In a locational-attainment model, education is construed as a measure of "permanent income," which determines access to communities through the housing costs and other costs associated with living in them, and also as a measure of various "tastes" that are associated with choice of a community, such as the tastes for a community that has certain cultural amenities (or is at least near them) or for a school system that prepares students to enter prestigious colleges.

However, the cross-level character of locational-attainment models has made them difficult to estimate in past research. The critical stumbling block—which is found in much research on spatial processes—is that, in general, no single data set provides data in the form required for direct model estimation.⁴ In particular, census data, which offer the most complete geographical coverage as well as a wide array of community and individual characteristics, are available either in an aggregate form or an individual one, which, in the past, forced analysts into an uncomfortable choice between the two. The aggregate form, the summary tape files (STFs; U.S. Bureau of the Census 1982, 1983*a*), allows analysts to distinguish among small geographical units (e.g., census tracts) but does not allow individual records to be linked to them. The alternative, the Public Use Microdata Samples (PUMS; U.S. Bureau of the Census 1983*b*), provides individual-level data but describes their geography only in terms of very large units (counties or county groups), thus precluding

⁴ For the dependent variable under investigation here, there is in fact an unusual census data set at the individual level, the F version of the PUMS, prepared for Douglas Massey and Nancy Denton. This data set represents a 1% sample of households and individuals in the 50 largest metropolitan areas. Each household is tagged with a small set of tract characteristics, including percentage of non-Hispanic whites, and a small random error has been added to each tract variable to protect confidentiality. A key limitation of this data set from the point of view of an analysis of segregation is that the metropolitan areas are not identifiable. Since the metropolitan context unquestionably plays an important role in segregation and in residential patterns in general (see Massey and Denton 1987; Alba and Logan 1991), it is important either to take metropolitan characteristics into account or to analyze segregation separately by metropolitan area. Nevertheless, the PUMS F file does offer an alternative route of investigating segregation (see Gross and Massey 1991; White et al. 1991).

insight into community differentiation. For the most part, analysts have chosen the finer spatial distinctions afforded by aggregate census data, thus risking the ecological fallacy in drawing conclusions about the locational processes affecting individuals and households. The magnitude of this risk remains unknown, despite a few attempts with unusual data sets to create parallel analyses with both levels of data (e.g., Massey and Denton 1985; Gross and Massey 1991).

Our results in this article are based on a novel method that uses both aggregate-level and individual-level data files to construct regression analyses that bridge the two. This method can be used to analyze locational processes in terms of a wide range of community characteristics, not just the percentage of non-Hispanic whites, which we analyze here. It can also be used to include community characteristics as independent variables, that is, as contextual influences, although we do not so employ them in this analysis. It is, finally, applicable at different levels of aggregation, such as the place and census tract, as long as the requisite tables (see below) are available. We will only outline the method here because we have described it at greater length elsewhere (Alba and Logan 1992). The method derives from a fundamental principle of OLS regression: the estimation of regression coefficients requires only pairwise (i.e., correlations or covariances) and univariate (i.e., means and standard deviations) information for all variables (Hanushek and Jackson 1977). The key element is a matrix of correlations or covariances, and this we construct by combining data from STFs and PUMs.

First, from STFs (mainly the 1980 STF4B file [U.S. Bureau of the Census 1983a], which reports tables by racial/ethnic group) we calculate the correlations between the aggregate dependent variable and the individual-level independent variables. This is straightforward as long as one can find STF tables that show the distributions of the various independent variables by racial/ethnic group in each community.⁵ Since the per-

⁵ Of course, these distributions must all be based on the same unit, and this requirement can necessitate a thorough search of census tabulations. For our analysis, we have selected tables (distributions) for individuals (rather than, say, households), in part because these tables offer the widest range of characteristics. In only one case, that of household income, did we have to resort to a tabulation for households. But the table we selected presents income categories by size of household and is thus readily converted to a table whose counts represent individuals. Partially missing data, or the coverage of tables, give rise to another issue. Many census tabulations do not cover all residents of a place—for example, household-income and home-ownership tabulations include only individuals residing in households, ignoring those in institutions, and language tabulations include only persons more than four years old. To be usable in our analysis, these and similar tables must be completed by adding appropriate categories to contain the remaining cases. This, in turn, generates a novel feature in our variable construction: variables that share the same category. As

centage of non-Hispanic white residents is also known for each community (from the STF3A file [U.S. Bureau of the Census 1982]), it follows that the x and y values are known for all members of a group and thus a correlation (or covariance) can be calculated. For instance, the correlation between individual-level nativity, represented as a dummy variable (foreign birth = 1), and the community percentage that is non-Hispanic white is readily calculated from tables that show the number of group members who are native born, the number who are foreign born, and the percentage of non-Hispanic whites for each community. In any community, $x = 0$, and y is the community percentage of non-Hispanic whites for every group member who is native born; $x = 1$, and y is the same percentage for every group member who is foreign born. Standard computing formulas for the correlation coefficient apply to this situation. The same principles hold, moreover, when the x variable is a scale, rather than categorical.

Second, from the PUMS data, we calculate the correlations among the independent variables at the individual level. It is not necessary to know about the specific communities in which individuals reside to do this, but it is necessary to make the boundaries of the region extracted from the PUMS data coincide quite closely with that mapped by the communities extracted from the STF data.⁶ Third, the correlations derived from the two different sources are assembled into a single matrix, which is then submitted to a standard regression program such as SPSS-X, along with means and standard deviations of all variables (which are readily calculated from the community-specific distributions of the STFs).

Two issues arise at the outset in the construction of these models: Which is the unit of analysis, the individual or the household? What is the geography of the analysis? Practical considerations determine our decision on the first issue. The key lies in the availability of tables for specific independent variables at the appropriate level, for our method cannot function without such tables. Even though the household is prob-

table 1 will show below, several variables contain a group-quarters category (which, to be sure, represents the same population in each case). Interpretation of the effects of these shared categories must reflect their involvement in several variables (see n. 17 below).

⁶ Since metropolitan regions and the geography of the PUMS data are both defined in terms of counties, it is not hard to make the metropolitan boundaries coincide in the two data sources. A small divergence arises, however, because our study is focused on suburbs and thus requires us to exclude central city residents. This is easily done in data from the Summary Tape Files, but several small central cities (such as Sayreville, New Jersey, with a population under 30,000 in 1980) are not identifiable in the PUMS data we are using (A geography). Their residents are therefore included in our PUMS calculations, creating an overall error of approximately 2.3% in the sample definitions.

ably the more appropriate unit for analyses of spatial location, theoretically critical variables (such as English-language ability and generation in the United States) are available in STF's only for individuals. Distributions of these variables for household heads cannot be simply assumed without great risk of error. Therefore, to avoid the risk associated with the omission of theoretically important variables, we have elected to base the analyses that follow on individuals, and thus have included all residents of the region under study (regardless of age or status in a household). To investigate the possibility of bias as a result of this decision, we have also run some parallel analyses at the individual and household levels for a limited set of variables available at both levels in STF's and found that there was a reassuringly close correspondence between the two sets of results.⁷

With respect to the second issue, we have focused our analysis on locational processes among suburban communities (for the New York City CMSA, as already noted).⁸ Our decision to focus on suburbs is justified by the likely heterogeneity of spatial processes in suburbs and central cities, especially where segregation is concerned, and is supported by some practical considerations as well. Previous research (e.g., Massey and Denton 1988) demonstrates that, for all minority groups, segregation is lower in suburbs than in central cities. Indeed, because minorities are few in number in many suburban areas, the probability of coresidence with whites usually is greatly enhanced by movement to suburbs (see Massey and Denton 1988, table 3). In other words, given two minority-group members with similar personal characteristics other than residence, it is highly plausible that the suburban one lives in a community with a greater proportion of whites than does the central city denizen. Thus, it does not seem appropriate to combine suburban and central city areas in the same analysis, since the effects of individual-level characteristics are likely to be dissimilar in the two types of social space. Further, to combine them would risk confusing the process of suburbanization *per se* with what we see as the more interesting process determining location

⁷ These results, which closely parallel the analysis described in Alba and Logan (1992), are available from Richard Alba.

⁸ The description in the text refers to the New York–New Jersey portion of the CMSA identified by the Census Bureau as the “New York–Northern New Jersey–Long Island CMSA.” For convenience, we excluded the small Connecticut portion of the CMSA from our analysis; it contains just 5% of the region’s suburban residents and even smaller percentages of its minority suburbanites. The New York–New Jersey portion of this CMSA includes eight primary metropolitan areas: Bergen-Passaic, N.J.; Jersey City, N.J.; Middlesex-Somerset-Hunterdon, N.J.; Monmouth-Ocean, N.J.; Nassau-Suffolk, N.Y.; New York, N.Y.; Newark, N.J.; and Orange County, N.Y.

within suburbia (on the differences among groups in the attainment of suburban residence, see Alba and Logan [1991]). In effect, the problem here is the reverse of the now familiar problem of selection bias (Berk 1983; cf. Stolzenberg and Relles 1990). Because, for most minority groups, suburban residence is rather strongly affected by such individual characteristics as household composition and English-language proficiency (Alba and Logan 1991), an analysis that includes both central city areas and suburban areas risks confounding the determinants of suburban rather than central city residence with those channeling suburbanites into a location in the hierarchy of suburbs.

In addition, our definition of a suburban community is formulated in terms of jurisdictional boundaries, for we view communities as positioned in a hierarchy of advantage, engaged in a competition to preserve or enhance their advantages, and able to wield political and legal means (e.g., restrictive zoning) in this struggle. Jurisdictional boundaries are obviously critical to the identification of these collective actors, and therefore many of the communities on which our study is based are incorporated places (e.g., villages, towns). It would make little sense to include a much larger central city as just another place in this scheme. An alternative would be to conduct the analysis separately within central city and suburban areas, at the census-tract level. Although there is obvious value in this alternative, it does run afoul of the practical problem that, in census data (at least in 1980), tract data do not permit a clean separation of the major racial/ethnic groups. Tables for whites and blacks include Hispanics (this is not the case for the place-level tables we employ here), and this obviously muddies a comparison of coefficients across equations.⁹ (Elsewhere, however, we [Logan et al. 1992] present separate

⁹ In data terms, the difference between what we can accomplish here with place-level data and what could be accomplished with tract-level data can be understood in the following way: For the current analysis, we use the 1980 STF4B file, which provides place-level tables for as many as 38 different racial/ethnic groups, including non-Hispanic whites and blacks, but does not provide data for areas within places. Thus, large cities cannot be decomposed into neighborhoods, and it makes little sense to retain central cities in the analysis. An alternative is to use the STF4A file, which reports tables at the census-tract level, but only for six broad and overlapping racial/ethnic categories. As noted, the STF4A tables for whites include Hispanics, and the same is true for blacks. Tables are also provided for Hispanics, regardless of race. There is no way of disentangling these populations to construct tables for non-Hispanic whites, for instance. But it is obviously desirable to have mutually exclusive racial/ethnic categories insofar as this is possible. In effect, 1980 census data files force one to choose—either drop central cities or use substantially overlapping racial/ethnic categories (e.g., whites and Hispanics). Here, we have chosen the former, and we focus on whether the process of placement *within* suburbia is conducive to racial/ethnic integration or segregation. This data incompatibility, it appears, may be corrected in the 1990 census.

central city and suburban analyses by using tract-level variables for community characteristics.)

Data and Measures

The suburban region under analysis encompasses 674 suburban communities. Not all of these are incorporated places; some are so-called census-designated places, and others are constituted by the residual portions of larger entities (towns in New York, townships in New Jersey) after nested places have been subtracted.¹⁰ Further, group-specific data may be missing, or "suppressed" in census terminology, for some communities, forcing us to combine places in the same town (or township) to arrive at a unit for which the necessary data can be inferred.¹¹ Consequently, the specific number of communities varies from group to group, from 616 for non-Hispanic whites to 353 for Asians.

From the 5% 1980 PUMS data, we have extracted separate files by group, containing all group members residing in the suburban portion of the New York City CMSA (i.e., outside of central cities). The numbers of cases in these files in effect determine the degrees of freedom for our analysis, because correlations derived from the PUMS data are necessarily based on a smaller number of sampled cases than the correlations derived from the STFs. In general, we have limited the number of cases for a group to 10,000, to prevent variations in group sizes from influencing the results.¹²

¹⁰ Census-designated places are defined on the basis of recognized locality names although they are not incorporated as municipalities. Our method requires us to include these and other nonincorporated areas, since their residents cannot be distinguished in one of our data sources, the PUMS. In any event, in the thickly settled New York metropolitan region, all such areas are parts of fairly small political entities, i.e., towns, and thus fall within the scope of the place-stratification model.

¹¹ To overcome the suppression of data, we have derived group counts wherever possible from higher-level information. For example, if group data are suppressed for only one place in a town, then the data can be inferred by subtracting the counts for all other places from those of the town (reported as a different level of geography by the Census Bureau). When two or more places in a town have missing data and must be combined, the group data for the combined area can be generated by the same subtraction process.

¹² In fact, the correlations from the PUMS are based on as many cases as were available, but we have fixed the *N* at 10,000 in estimating regression results. For non-Hispanic whites and Asians, the number of cases is smaller than 10,000—they are 7,321 and 6,148 respectively. In the case of whites, the smaller *N* results from our use of the .1% sample for the majority group; in the case of Asians, the smaller *N* is simply a reflection of this group's size in the suburban population (and thus the 5% sample).

In the analysis to follow, we estimate a separate regression equation for each of four major racial/ethnic groups (non-Hispanic whites, non-Hispanic blacks, Hispanics, and Asians). The *dependent variable*, to repeat, is the percentage of a community's residents who are non-Hispanic white. The equations include a number of demographic, socioeconomic, and assimilation variables characterizing individual group members. Because of the categorical format of census summary tables, which are obviously pivotal for our data construction, we have represented each independent variable as a set of dummy variables. This construction also has the advantage of giving the intercept a simple, empirically meaningful interpretation (as will become clear in our subsequent discussion). The independent variables and their means by racial/ethnic group are presented in table 1. In addition, we discuss here the rationale for each variable's inclusion in our model.

1. *Assimilation*.—English-language ability (those who speak only English at home, who speak English well, and who do not speak English well), along with nativity and immigration status (native born, immigrated before 1975, and immigrated 1975 or later) are the measures included. The spatial-assimilation model would anticipate less desirable spatial outcomes, and thus less proximity to non-Hispanic whites, for recent immigrants and more desirable locations for those who speak English well. In other words, some locational payoff is expected from assimilation status.

2. *Socioeconomic status*.—This includes home ownership, household income, and education. A central tenet of spatial-assimilation theory is that, controlling for other relevant characteristics, minority-group members who have achieved higher socioeconomic standing will gain access to desirable locations equal to those of majority-group members of the same status. The theory implies a positive effect of socioeconomic variables on proximity to the majority. (We would also expect to find this relationship among non-Hispanic whites, although its slope may be lower because even less affluent whites reside in communities that are largely white.) By contrast, the stratification hypothesis is that the coefficient linking income to spatial advantage (i.e., the "return to earnings") will be lower for members of those minority groups that experience discrimination in the housing market.

We will be especially attentive to the coefficients for home ownership. This is another innovation in our modeling strategy, since it has been recognized that the failure of income alone to explain segregation may be due to the omission of any measure of wealth. Home ownership is, in part, a reflection of wealth, and is a virtual prerequisite for residing in many high-status suburban communities.

3. *Demographic composition*.—The variables include *age*, in five cate-

TABLE 1
INDEPENDENT VARIABLE CONSTRUCTION AND MEANS BY GROUP

| VARIABLES | SOURCES | | MEANS | | | |
|--------------------------------------|----------------------|-------|------------------------|------------------------|-----------|--------|
| | Summary Tape File | Table | Non-Hispanic Whites | Non-Hispanic Blacks | Hispanics | Asians |
| Age | STF4B | PB5 | | | | |
| Under 5 years old† | ... | ... | .054 | .075 | .088 | .115 |
| 5-17 years old† | ... | ... | .203 | .266 | .255 | .234 |
| 18-24 years old† | ... | ... | .112 | .124 | .128 | .070 |
| 25-64 years old* | ... | ... | .509 | .471 | .483 | .546 |
| 65 years old and older | ... | ... | .122 | .063 | .046 | .036 |
| Household structure | STF4B | PB19 | | | | |
| Married couple | ... | ... | .795 | .556 | .736 | .869 |
| Other family | ... | ... | .100 | .294 | .173 | .059 |
| Nonfamily | ... | ... | .088 | .116 | .072 | .052 |
| Group quarters (nonhousehold)† | ... | ... | .018 | .034 | .019 | .020 |
| Home ownership | STF4B | HB3 | | | | |
| Owner-occupied unit | ... | ... | .785 | .501 | .454 | .626 |
| Renter-occupied unit* | ... | ... | .197 | .466 | .527 | .353 |
| Group quarters† | ... | ... | .018 | .034 | .019 | .020 |
| Household income | STF4B | HB45 | | | | |
| Under \$5,000* | ... | ... | .038 | .104 | .096 | .033 |
| \$5,000-\$9,999 | ... | ... | .066 | .127 | .122 | .039 |
| \$10,000-\$14,999 | ... | ... | .086 | .136 | .137 | .070 |
| \$15,000-\$19,999 | ... | ... | .113 | .129 | .147 | .090 |
| \$20,000-\$29,999 | ... | ... | .263 | .230 | .251 | .239 |
| \$30,000-\$39,999 | ... | ... | .187 | .140 | .126 | .202 |
| \$40,000-\$49,999 | ... | ... | .102 | .057 | .056 | .125 |

| | | | | | |
|---|------|------|------|------|------|
| \$50,000–\$74,999..... | ... | .088 | .037 | .036 | .121 |
| \$75,000 and over | ... | .037 | .006 | .011 | .062 |
| Group quarters† | ... | .018 | .034 | .019 | .020 |
| Educational attainment..... | PB48 | | | | |
| Grammar school* | ... | .083 | .090 | .164 | .045 |
| Some high school | ... | .078 | .101 | .079 | .028 |
| High school graduate..... | ... | .234 | .197 | .162 | .094 |
| Some college..... | ... | .095 | .083 | .067 | .076 |
| College graduate..... | ... | .141 | .064 | .057 | .339 |
| Under 25 years old† | ... | .369 | .465 | .471 | .419 |
| English language ability | PB11 | | | | |
| Speaks only English* | ... | .843 | .883 | .188 | .187 |
| Speaks English well or very well..... | ... | .092 | .038 | .522 | .593 |
| Speaks English not well or not at all..... | ... | .011 | .005 | .202 | .105 |
| Under 5 years old† | ... | .054 | .075 | .088 | .115 |
| Immigration status..... | PB10 | | | | |
| Native born* | ... | .916 | .919 | .587 | .273 |
| Immigrated pre-1975 | ... | .078 | .059 | .337 | .423 |
| Immigrated 1975 or later | ... | .006 | .022 | .077 | .303 |
| Ethnicity among whites | 28 | | | | |
| British..... | ... | .039 | ... | ... | ... |
| French | ... | .005 | ... | ... | ... |
| German..... | ... | .066 | ... | ... | ... |
| Irish | ... | .086 | ... | ... | ... |
| Italian..... | ... | .166 | ... | ... | ... |
| Polish..... | ... | .043 | ... | ... | ... |
| Other single ancestries and mixed ancestries* ... | ... | .595 | ... | ... | ... |
| Ethnicity among Hispanics | 13 | | | | |
| Mexican | ... | ... | ... | .031 | ... |
| Puerto Rican | ... | ... | ... | .374 | ... |
| Cuban..... | ... | ... | ... | .204 | ... |
| Other* | ... | ... | ... | .392 | ... |

TABLE 1 (Continued)

| VARIABLES | SOURCES | | MEANS | | | |
|------------------------------|----------------------|-------|------------------------|------------------------|-----------|--------|
| | Summary Tape File | Table | Non-Hispanic Whites | Non-Hispanic Blacks | Hispanics | Asians |
| Race among Hispanics..... | STF3 | 14 | | | | |
| White* | ... | ... | ... | ... | .770 | ... |
| Black | ... | ... | ... | ... | .037 | ... |
| Other | ... | ... | ... | ... | .194 | ... |
| Ethnicity among Asians | STF3 | 12 | | | | |
| Japanese | ... | ... | ... | ... | ... | .126 |
| Chinese | ... | ... | ... | ... | ... | .258 |
| Korean | ... | ... | ... | ... | ... | .116 |
| Filipino | ... | ... | ... | ... | ... | .173 |
| Asian Indian | ... | ... | ... | ... | ... | .269 |
| Vietnamese | ... | ... | ... | ... | ... | .018 |
| Other Asian* | ... | ... | ... | ... | ... | .040 |

NOTE.—“Sources” reports the tables used in the construction of correlations between the dependent and independent variables. Correlations between independent variables are taken from PUMS data. In subsequent regressions, all independent variables are expressed as dummy variables; hence, their means here are proportions.

* Omitted category.

† Category shared among variables (see n. 5).

gories identifying group members under the age of 18 (i.e., children) as well as those who are 65 years old and older, and *household composition*, distinguishing people in married-couple households, other family types, nonfamily households, and institutional settings (known as "group quarters" in census terminology).¹³ Both variables have previously been shown to be independently associated with community characteristics and with suburban residence. Their effects are widely understood to indicate a preference for, or greater willingness to invest in, location by married couples with children. This suggests that members of married-couple households and children may be more likely than others to be in communities with high percentages of non-Hispanic whites. This interpretation is compatible with (but not critical to) the spatial-assimilation model. In any event, these variables are implicated in the construction of others, and it is necessary to control for variables that are likely to be associated with locational outcomes (see table 1).

4. *Ethnic subgroup composition*.—For all groups except blacks, this is a series of dummy variables to indicate membership in more specific categories of origin, such as Chinese or Japanese among Asians. The assimilation model provides little reason to expect differences among subgroups in locational outcomes, net of other predictors. Differences between Chinese and Japanese might be interpreted, for instance, in terms of the recency of arrival of the subgroup in the United States, but there should be no further differentiation. Subgroup differences are more important to the stratification model, which would predict locational disadvantages for those subgroups which experience the most severe discrimination. Among non-Hispanic whites, these might include those from the periphery of Europe, that is, Poles, Italians, and, perhaps, Irish. Among Hispanics, they would especially affect Puerto Ricans and possibly Mexicans; race would be expected to have an additional effect on Hispanic residential patterns (Denton and Massey 1989). Among Asians, the least advantaged might be Filipinos, while the Japanese, Koreans, and Chinese (except for those from the People's Republic of China) might be relatively advantaged because of the current power of the Japanese, Korean, Taiwanese, and Hong Kong economies.

In constructing these categories among whites, we have been cognizant of the broad decline of ethnic differentiation in the white population (Alba 1990; Lieberman and Waters 1988). Therefore, we have distinguished only

¹³ Some other age categories are necessary for the proper construction of other variables. Thus, the under-five-years-old category is required for completion of the English-language variable, which is tabulated in the census only for persons five years old and older; the 18–24 category completes a set of categories for those less than 25 years old, required for the completion of the educational attainment variable, which is tabulated for persons 25 years old and older.

categories of single ancestry (e.g., Italian ancestry only), on the grounds that any ethnic differences should be most detectable for such categories. Individuals with mixed ancestry are classified in the omitted category, as are individuals with single ancestries who come from smaller ethnic groups (e.g., Swedish).

FINDINGS

Mean Differences among Groups

Table 2 shows the means in community percentage of non-Hispanic whites by major racial/ethnic group and, within each group, by ethnic subgroup. The values in the table are means across individuals; they have been generated by weighting the percentage of non-Hispanic whites in a community by the number of members of a given group who reside there. According to the formula for P^* (see n. 3 above), the values therefore represent the exposure or proximity to whites (P^*) of the average group member in the suburbs surrounding New York City. (Correlation ratio, or η^2 , values are also presented; they are taken from analyses of variance between group or subgroup membership and the community percentage variable.)

The table shows substantial and expected differences among the major racial/ethnic groups and, to a limited extent, within these groups. Aside from non-Hispanic whites themselves, Asians are the group with the greatest residential proximity to whites: the average Asian suburbanite resides in a community that is about 85% non-Hispanic white. Blacks, on the other hand, have surprisingly low residential proximity to whites in light of the fact that this suburban region is heavily non-Hispanic white (in 1980, whites composed 86% of its population). The average black suburbanite is in a community that is more than 40% minority. Hispanics fall midway between the black and Asian proximities.

There is only slight variation in proximity according to ethnic subgroup among whites and Asians. Even though the differences among whites are statistically significant, the range of variation is minute, from 88.1% (for persons of French ancestry) to 90.0% (for those in the residual category). The dispersion is slightly wider among Asians, from 82.9% for Asian Indians to 87.9% for Chinese.

Substantively more important differences are found among Hispanics. At one end of the spectrum are Mexicans, who approach the Asian groups in terms of residence in communities with many non-Hispanic whites. At the other end are Cubans, who are equal to blacks in their tendency to reside in communities with relatively high proportions of minority residents. In between come Puerto Ricans and the other Spanish groups

TABLE 2
MEAN PERCENTAGE OF NON-HISPANIC WHITES (or P^*) IN SUBURBAN COMMUNITIES
OF RACIAL/ETHNIC GROUPS

| Racial/Ethnic Group | % (P^*) |
|--|-------------|
| Major groups: | |
| Non-Hispanic whites | 89.7 |
| Non-Hispanic blacks | 57.8 |
| Hispanics | 70.2 |
| Asians | 85.4 |
| η^2 | .283 |
| White national-origin groups: | |
| English | 89.4 |
| French | 88.1 |
| Germans | 89.9 |
| Irish | 89.7 |
| Italians | 88.5 |
| Polish | 89.0 |
| Other single ancestries and mixed ancestries | 90.0 |
| η^2 | .002 |
| Hispanic origin groups: | |
| Mexicans | 80.5 |
| Puerto Ricans | 72.4 |
| Cubans | 57.5 |
| Other Spanish | 74.0 |
| η^2 | .084 |
| Hispanic racial groups: | |
| Whites | 71.7 |
| Blacks | 59.8 |
| Others | 66.3 |
| η^2 | .017 |
| Asian groups: | |
| Japanese | 86.9 |
| Chinese | 87.9 |
| Koreans | 87.8 |
| Filipinos | 83.5 |
| Asian Indians | 82.9 |
| Vietnamese | 83.0 |
| Other Asians | 83.7 |
| η^2 | .022 |

NOTE.—All analyses of variance are statistically significant. Values in the table are weighted means, where the weight is the number of group members in each suburb.

(the "other" category mixes Caribbean groups, such as Dominicans, with Central and South Americans). There is also substantial variation by race for Hispanics, with black Hispanics tending to reside in communities with the lowest percentages of non-Hispanic whites and white Hispanics in communities with the highest percentages. The range of these differences rivals that by ethnic origin.

Locational Analysis Results

How are such differences generated? Do different groups convert their human-capital and other personal characteristics into location in similar ways? And are there conditions under which minorities achieve parity with their white peers? Regression analyses that address these questions, produced by the method described earlier, are reported in table 3.

Overall, the analyses reveal quite different patterns of attaining residential proximity to whites. But there are important resemblances between the patterns for blacks and whites, even though their proximity levels mark the extremes. Two of the coefficients in the table give initial insight into the patterns for these two groups, the intercept and R^2 . Because of the construction of the independent variables as categorical, the intercept here has a more compelling interpretation than is usually true in regression analysis. Specifically, given our choice of omitted categories, it can be interpreted as the predicted percentage of non-Hispanic whites in the community of residence of a group member with a particular set of characteristics: someone of working age (25–64-years old), who resides in a nonfamily, renter household with less than \$5,000 in annual income, speaks only English and was born in the United States, and has only a grammar school education.¹⁴ (For whites, membership in the residual ethnic subgroup is also assumed.) According to the intercept for whites, a white with these characteristics would be expected to reside in a suburb 83% of whose residents are also non-Hispanic white. This value

¹⁴ Because the independent variables are represented as dummy variables, the categories in the text also represent the categories of comparison for the coefficients (and associated statistical tests) reported in the table. The reader will note that, in the case of the socioeconomic variables, these omitted categories occur at the low end in each case and therefore, according to the assimilation model, the predicted sign of the coefficients in the table should be positive (increases in SES produce increases in proximity to whites). In the case of the assimilation variables, we have not followed this convention, however. The reason is that it would lead us to choose omitted categories for these variables (poor English and recent immigration) that are quite rare among suburban whites and blacks. This would, in turn, lead to awkwardness in discussing the intercepts, and so we chose the typical categories (speaks only English and native born), instead. The assimilation model would therefore predict negative coefficients for these variables.

TABLE 3

INDIVIDUAL-LEVEL REGRESSION ANALYSES OF COMMUNITY PERCENTAGES OF NON-HISPANIC WHITES

| Independent Variables | Non-Hispanic Whites | Non-Hispanic Blacks | Hispanics | Asians |
|-----------------------------------|---------------------|---------------------|---------------|---------------|
| Age: | | | | |
| Under 5 years old ^b | .76 (.86) | -2.73* (1.38) | -6.49* (1.03) | 4.78* (1.25) |
| 5-17 years old ^b | 1.08 (.68) | -1.45 (1.12) | .23 (.72) | 3.55* (1.10) |
| 18-24 years old ^b | .59 (.73) | -3.07* (1.23) | .94 (.79) | 2.71* (1.28) |
| 25-64 years old ^a | 0 | 0 | 0 | 0 |
| 65 years old and older | -.50 (.51) | 1.86 (1.20) | 2.93* (.97) | 1.15 (1.09) |
| Household structure: | | | | |
| Married couple | 1.50* (.55) | 1.57 (.89) | -.94 (.79) | -.33 (.84) |
| Other family | .37 (.65) | -.80 (.93) | -1.14 (.88) | -1.17 (1.05) |
| Nonfamily ^a | 0 | 0 | 0 | 0 |
| Group quarters ^b | 3.49* (1.30) | 28.39* (1.77) | 21.60* (1.65) | 10.29* (2.01) |
| Home ownership: | | | | |
| Owner-occupied unit | 5.81* (.38) | .14 (.61) | 9.82* (.47) | 7.81* (.47) |
| Renter-occupied unit ^a | 0 | 0 | 0 | 0 |
| Household income: | | | | |
| Under \$5,000 ^a | 0 | 0 | 0 | 0 |
| \$5,000-\$9,999 | .64 (.89) | 1.08 (1.08) | 1.26 (.82) | 5.56* (1.50) |
| \$10,000-\$14,999 | 1.05 (.88) | 2.05 (1.08) | 3.17* (.84) | 3.81* (1.34) |
| \$15,000-\$19,999 | .69 (.82) | 1.95 (1.12) | 4.53* (.83) | 3.00* (1.40) |
| \$20,000-\$29,999 | .80 (.80) | 2.05* (1.04) | 5.25* (.79) | 6.48* (1.28) |
| \$30,000-\$39,999 | 1.11 (.84) | 3.13* (1.17) | 6.92* (.90) | 6.62* (1.32) |
| \$40,000-\$49,999 | .85 (.85) | 5.42* (1.47) | 8.25* (1.11) | 8.42* (1.34) |
| \$50,000-\$74,999 | 1.08 (.89) | 5.87* (1.67) | 10.67* (1.27) | 8.29* (1.37) |
| \$75,000 and over | 1.31 (1.03) | 15.07* (3.57) | 9.00* (2.00) | 10.38* (1.48) |

TABLE 3 (Continued)

| Independent Variables | Non-Hispanic Whites | Non-Hispanic Blacks | Hispanics | Asians |
|---|---------------------|---------------------|---------------|--------------|
| Education: | | | | |
| Grammar school ^a | 0 | 0 | 0 | 0 |
| Some high school | .17 (.73) | -1.84 (1.24) | 1.56 (.86) | 1.33 (1.43) |
| High school graduate | .50 (.63) | -3.03* (1.11) | 4.28* (.71) | 2.01 (1.13) |
| Some college | 1.00 (.71) | -1.98 (1.30) | 4.72* (.93) | 1.76 (1.18) |
| College graduate | .67 (.68) | 1.00 (1.41) | 7.06* (.99) | 4.22* (1.05) |
| English language ability: | | | | |
| Speaks only English ^a | 0 | 0 | 0 | 0 |
| Speaks English well or very well | -1.95* (.56) | .88 (1.41) | -8.45* (.57) | .95 (.56) |
| Speaks English not well or not at all | -4.49* (1.40) | -2.44 (3.88) | -13.48* (.77) | 1.83* (.81) |
| Immigration status: | | | | |
| Native born ^a | 0 | 0 | 0 | 0 |
| Immigrated pre-1975 | -.76 (.62) | 1.13 (1.15) | -4.27* (.63) | -1.29* (.59) |
| Immigrated 1975 or later | -.77 (1.83) | 1.31 (1.79) | -3.89* (.87) | -.60 (.61) |
| Ethnicity among whites: | | | | |
| British | .19 (.73) | | | |
| French | -1.10 (1.90) | | | |
| German | .51 (.57) | | | |
| Irish | -.37 (.51) | | | |
| Italian | -.64 (.39) | | | |

| | | | |
|---|--------------|---------------|---------------|
| Polish..... | -.06 | (.69) | |
| Other single ancestries and mixed ancestries ^a | 0 | | |
| Ethnicity among Hispanics: | | | |
| Mexican..... | 2.52* | (1.17) | |
| Puerto Rican..... | -2.51* | (.57) | |
| Cuban..... | -13.76* | (.57) | |
| Other ^a | 0 | | |
| Race among Hispanics: | | | |
| White ^a | 0 | | |
| Black | -16.29* | (1.04) | |
| Other | -3.34* | (.51) | |
| Ethnicity among Asians: | | | |
| Japanese | | | 3.81* (1.09) |
| Chinese..... | | | 2.55* (1.00) |
| Korean | | | 2.75* (1.10) |
| Filipino..... | | | -2.14* (1.05) |
| Asian Indian..... | | | -1.17 (1.00) |
| Vietnamese..... | | | -1.22 (1.60) |
| Other Asian ^a | | | 0 |
| Intercept | 82.65* (.97) | 55.35* (1.35) | 69.37* (1.87) |
| R ² | .060 | .041 | .119 |
| Adjusted R ² | .056 | .038 | .115 |

NOTE. — SEs are reported in parentheses.

^a Omitted category.

^b Category shared among variables.

* Statistically significant at the .05 level (two-tailed).

is not much below the mean in table 2. The intercept for blacks shows a very different residential experience in suburbs, since the community of a black with these characteristics is predicted to be just 55% non-Hispanic white (the white and black intercepts are significantly different: $t = 16.42$, $p < .001$).¹⁵ A comparison of whites and blacks who are equal in the ways just specified has done little to reduce the gap between the two groups in table 2.

The low R^2 values suggest a reason for this. For both groups, individual characteristics offer scant explanation for residential proximity to whites. For non-Hispanic whites, this evidently means that residence in a largely white community is typical for individuals with rather differing family, socioeconomic, and assimilation characteristics. For blacks, the conclusion suggested is that residence in a disproportionately minority community is typical under a wide variety of individual circumstances.

These conclusions are further borne out by an examination of regression coefficients (whose standard errors appear in parentheses). Just a few variables are statistically significant in the equation for non-Hispanic whites. Home owners reside in communities with higher proportions of non-Hispanic whites than renters do (by a margin of six percentage points). Those who speak a language other than English at home reside in communities with modestly higher minority proportions, and the magnitude of this difference varies according to ability to speak English (i.e., those who do not speak English well are in communities with the highest minority proportions). This last finding indicates that the least acculturated, or most ethnic, whites are the ones most residentially proximate to minorities, which is consistent with spatial-assimilation theory but contradicts a finding from aggregate-level data that white ethnic communities tend to exclude minorities (Logan and Stearns 1981). Other than these effects, there is little else to explain proximity. Noteworthy is that household income has no direct effect on proximity (once ownership status is controlled), nor does ethnic suborigin (once English-language proficiency is controlled). The last remaining significant coefficients are associated with household structure. Individuals in married-couple households reside in communities with slightly higher proportions of non-

¹⁵ The statistical significance of differences between coefficients across equations is assessed, here and subsequently, using a well-known t -test, whose formula is given by

$$t = \frac{b_1 - b_2}{\sqrt{SE_1^2 + SE_2^2}}.$$

The necessary SEs are reported in parentheses in table 3; we consistently determine the sign of t by comparing groups in left-right order in the table. Except where stated otherwise, we also evaluate significance with two-tailed tests.

Hispanic whites than do individuals in nonfamily households. A somewhat larger difference favors individuals in institutional settings, indicating that the communities in which most institutions (such as college dormitories, hospitals, and nursing homes) are found are more homogeneously white than suburbs are, on average.

Similarly, the set of variables that plays a role in the black equation is also small, but it is not the same as that in the white equation. Most important, home owning does not improve the ability of blacks to live near whites, despite our expectation that ownership should give members of minorities access to more privileged communities. Instead, higher household income is associated with greater proximity to whites, but in a nonlinear way. Little change in proximity occurs up to the income level of \$40,000, and there is a considerable leap upward in proximity to non-Hispanic whites for individuals in the highest income category, \$75,000 and over.¹⁶ In both respects, the black equation is significantly different from the white one (the *t*-value for the difference in home-ownership coefficients is 7.89, $p < .001$; significant differences in the income coefficients first appear for the \$40,000–\$49,999 category [$t = -2.69$, $p < .01$] and continue into the higher income categories).

Other than household income, the only remaining effects of note among blacks are associated with household structure. One is indirectly revealed through age, that is, blacks less than 25 years old reside in communities with lower white proportions, suggesting that families with children are disadvantaged in achieving coresidence with whites. Also, there is a large effect associated with residence in institutional settings: the communities in which most of the institutions are found result in blacks in these settings living in much greater proximity to whites than does the average group member.¹⁷

¹⁶ That these income steps do lead to successively greater proximity to whites can be demonstrated by varying the omitted category of income in the black equation, since it serves as the reference point for the remaining income coefficients and associated statistical tests. For example, when the omitted category is set as \$30,000–\$39,999, then a significant increment in proximity appears for the next higher income category ($t = 1.85$, $p < .05$, one-tailed) and subsequent ones. Likewise, the highest income category significantly increases proximity compared to the next lower one, \$50,000–\$74,999 ($t = 2.42$, $p < .05$). Analogous procedures, whose details we do not report here, demonstrate that, for Asians and Hispanics also, the pattern of proximity increasing with income, evident in table 3, is statistically meaningful. We have also used this approach to verify the ordinality of the effects of language among whites and Hispanics and that of education among Hispanics.

¹⁷ To understand the large magnitude of the group-quarters effect, it is also necessary to take into account the role this category plays in several variables. As table 1 shows and as n. 5 above describes, this variable is shared among three variables. Given the omitted categories of these variables, the group-quarters coefficient reports the ex-

The differing profile of effects in the white and black equations implies that the comparison between members of these groups whose personal and household characteristics other than race are similar depends on the precise characteristics one posits. To evaluate the differences between the races in proximity to whites, then, we cannot rely only on the comparison of intercepts discussed earlier. Accordingly, we have calculated predicted values of proximity for six types in each racial/ethnic group, which we label "poor natives," "poor immigrants," "average SES natives," "average SES immigrants," "affluent natives," and "affluent immigrants." Each type represents a distinct combination of socioeconomic and assimilation characteristics, with the values of other variables held constant at the omitted categories in table 3. A "poor" individual is defined as one with a grammar school education who lives in a renter household with less than \$5,000 in annual income (these are the omitted categories in table 3); an "average SES" individual is a home owner with a high school diploma whose household income lies between \$20,000 and \$29,999 (these are the categories of the average suburbanite in this region in 1980); and an "affluent" individual is a college graduate who is a home owner and has an annual household income of \$75,000 or greater. A "native" is a native-born individual who speaks only English at home; an "immigrant" arrived in the United States after 1974 and does not speak English well. Calculating the predicted values for any of these types is straightforward and involves only adding the intercept and the coefficients of those characteristics that depart from the omitted categories. Thus, the predicted percentage of non-Hispanic whites in the community of residence of a black, average SES native is 54.51 ($55.35 + .14 + 2.05 - 3.03$). The remaining predicted values are presented in table 4.

The comparison of the predicted values for various types of whites and blacks in table 4 establishes what table 3 implies: only the most affluent blacks, with incomes of \$75,000 and higher (a category that represented less than 1% of suburban blacks in 1980), gain substantially in proximity

pected difference between someone in an institutional setting and someone who resides in a nonfamily, rental household with less than \$5,000 in income. These latter characteristics (income, in particular) are associated with low proximity to whites, enhancing the size of the group-quarters coefficient. It should also be noted from table 1 that only a small proportion of each group resides in group quarters (nevertheless, this subgroup cannot be removed from the analysis because its members are indistinguishable in some of the aggregate tables from which the analysis is constructed). Across all racial/ethnic groups, about 28% of the group-quarters population is in homes for the aged, 26% in college dormitories, 13% in mental hospitals, and 5% in military quarters. Inmates of correctional institutions account for a further 16% of the total. The remainder, including residents of rooming houses, is 13%.

TABLE 4
PREDICTED PERCENTAGE OF NON-HISPANIC WHITES IN COMMUNITY OF RESIDENCE
FOR SIX TYPES FROM EACH RACIAL/ETHNIC GROUP

| | Non-Hispanic Whites (%) | Non-Hispanic Blacks (%) | Hispanics (%) | Asians (%) |
|-------------------|-------------------------------|-------------------------------|------------------|---------------|
| Natives: | | | | |
| Poor | 82.7 | 55.4 | 74.2 | 69.4 |
| Average SES | 89.8 | 54.5 | 93.6 | 85.7 |
| Affluent..... | 90.4 | 71.6 | 100.0 | 91.8 |
| Immigrants: | | | | |
| Poor | 77.4 | 54.2 | 56.8 | 70.6 |
| Average SES | 84.5 | 53.4 | 76.2 | 86.9 |
| Affluent..... | 85.2 | 70.4 | 82.7 | 93.0 |

NOTE.—The six types represent distinct combinations of the socioeconomic and assimilation variables, with all other variables set at their omitted-category values. Definitions are as follows: poor = renter, income under \$5,000, grammar school only; average SES = home owner, \$20,000–\$29,999 income, high school graduate; affluent = home owner, income of \$75,000 and over, college graduate; native = U.S. born, speaks only English; immigrant = arrived after 1975, does not speak English well. Predicted values for these types are calculated from the coefficients in table 3 according to the method described in the text. The values for the “poor” category under “native” are the same as the intercepts in that table.

to whites. Even so, they do not close the gap with their white peers: an affluent native black is predicted to reside in a community that is 71.6% non-Hispanic white, while a similar white resides in one that is 90.4% white. Indeed, by this measure, an affluent black does not achieve residential parity with a poor white, who is predicted to reside in a community that is 82.7% white, if he or she is native, and 77.4% white, if an immigrant.

In contrast to the white and black equations, both the Asian and Hispanic equations show a much greater variety of effects. For these groups, there is a closer nexus between individual characteristics and residential proximity to whites than is found in the preceding equations, and this is borne out in higher R^2 values, especially for Hispanics. Nevertheless, although some similarities are found between the Asian and Hispanic equations, there are also some prominent differences.

Principal among the similarities are the large socioeconomic effects evident for both groups. For both Asians and Hispanics, owning a home gives access to communities with substantially higher white proportions than is true for renters. The difference associated with ownership is significantly higher among Hispanics than among Asians, but it is higher in either group than among non-Hispanic whites (the t -values for the Hispanic-Asian, white-Hispanic, and white-Asian comparisons are, re-

spectively: 3.02, $p < .01$; -6.63, $p < .001$; and -3.31, $p < .001$). In further contrast to the equation for whites, household income contributes to proximity for both minorities. In the Hispanic equation, proximity climbs with improvements in income, until an income level of \$50,000 is attained; the Asian pattern is a bit harder to characterize, but proximity appears to move upward through several plateaus, with a sizable difference found between the lowest income level (under \$5,000 a year) and all subsequent ones. (For Hispanics, significant differences from the white equation start with the coefficient for \$15,000-\$19,000 [$t = -3.29$, $p < .01$] and continue into higher income categories; for Asians, the systematic differences from the white pattern begin with the coefficient for \$20,000-\$29,999 [$t = -3.76$, $p < .001$].) Finally, education has a role in the Asian and Hispanic equations. Again, the pattern for Hispanics is one of a rather steady rise in proximity to whites as educational attainment increases. For Asians, the difference in proximity lies between college graduates and all others.¹⁸

A major difference between the two equations occurs for the assimilation variables. These are very central in determining proximity to whites for Hispanics, but almost inconsequential for Asians. Hispanics who speak Spanish at home reside in communities with higher minority proportions, all the more so if they do not speak English well. The difference made by language assimilation is considerably larger than that among whites. Immigration status adds to it, for Hispanics born abroad also reside in communities with higher minority proportions. This pattern, anticipated by spatial-assimilation theory, is not matched among Asians, however. Persons who speak Asian languages at home are not at all disadvantaged in terms of proximity to whites. The foreign-born (in particular, those who immigrated prior to 1975) are just slightly so. These findings demonstrate that recently arrived Asian immigrants who do not speak English well are able to attain proximity to whites largely according to their socioeconomic characteristics. This is in strong contrast to the potent role of assimilation among Hispanics and also to elements of spatial-assimilation theory.¹⁹

¹⁸ The locational returns to a college education are significantly higher for Hispanics than for Asians (e.g., the t -value for a comparison of the coefficients for the some college category is 1.97, $p < .05$). Further, virtually all the significant education coefficients for both groups are also significantly higher than the corresponding coefficients for whites and blacks. For instance, the t -value for the high school graduate coefficients in the white-Hispanic comparison is -3.98 ($p < .001$). The only nonsignificant comparison is between black and Asian college graduates ($t = -1.83$).

¹⁹ The differences between Hispanic and Asian coefficients are consistently significant. For instance, the t -value for the difference in coefficients for individuals who speak English well is -11.76 ($p < .001$), and the t -value for the coefficients for individuals

Hispanics and Asians differ further in the part played by ethnic suborigins. There are some modest differences among the Asian groups: Japanese, Chinese, and Koreans have moderately greater proximity to whites than members of the other groups. The differences among Hispanic groups are much more profound; the mean differences among them in table 2, then, are not simply due to different socioeconomic or assimilation endowments. Mexicans have the greatest proximity to whites, followed by those in the "other" category and Puerto Ricans, with a gap of approximately five percentage points separating the first and last of these groups. Cubans trail considerably behind the others, about 16 percentage points behind Mexicans. Hispanics are also sharply differentiated according to racial origin. Black Hispanics in particular have much lower proximity to whites, although there is also a significant difference between white Hispanics and those who define themselves as neither white nor black. Needless to say, white Hispanics are most proximate to white non-Hispanics.

Finally, there are differences between these two major groups in terms of the effects of age and household composition. Among Hispanics, as among blacks, there is some disadvantage associated with the presence of children in the household; this is specifically evident for children less than five years old.²⁰ Among Asians, by comparison, children, and by implication the households containing them, have greater proximity to whites than working-age adults. For both groups, residence in institutional settings means residence in communities with greater than average proportions of non-Hispanic whites.

In further contrast to the black case, the equations for Asians and Hispanics imply that certain characteristics tend to bring members of these groups into suburban communities with the same racial/ethnic composition (i.e., percentage of non-Hispanic whites) as the communities of residence of their white peers. (Note, however, that one cannot infer directly from our analysis that the Asians and Hispanics in question

who arrived in 1975 or later is -3.10 ($p < .01$). In general, the Hispanic coefficients are also significantly different from those for whites and blacks. The only exception occurs for the white-Hispanic comparison for immigrants who arrived in 1975 or later ($t = 1.54$).

²⁰ Caution is warranted here because this effect is confounded with that of the language variable. As table 1 shows, the under-five-years-old age category is shared by age and English-language ability (defined only for persons five years old and older in census data). By the way the variables are constructed, the coefficient of this category compares young children to adults in English-speaking households. Since speaking English at home adds considerably to Hispanic proximity to whites, it is possible that the negative coefficient for young children reflects simply an uncontrolled language effect in this group.

reside in the same suburbs as their white peers.) This result, which is evident in the predicted values in table 4, is in accord with the spatial-assimilation model, especially because favorable socioeconomic characteristics achieve this outcome for Asian groups and the combination of favorable socioeconomic and assimilation characteristics (U.S. birth, speaking English at home) do the same for *most* Hispanics. For Asians in the other Asian category (which, as the omitted ethnic subgroup, is assumed in table 4), even average socioeconomic characteristics raise proximity to whites almost to the level of their white peers. For the Chinese, Japanese, and Koreans, who are more residentially proximate to whites (according to table 3), parity with their white peers is attained with the achievement of average socioeconomic characteristics, as it is for affluent Asians (of any subgroup). Being an immigrant, rather than a native, has only a small effect.

The story among Hispanics is more complex because it depends more on subgroup membership. For Hispanics who are white and in the other ethnic Spanish subgroup (the omitted categories), parity with whites occurs for natives with average or affluent socioeconomic characteristics.²¹ The same statement holds for Mexicans, Puerto Ricans, and Hispanics in the other race category, since these groups all have small coefficients in table 3 and thus deviate only slightly from predictions for the omitted categories. For immigrant Hispanics in the same subgroups, near parity is achieved by those with affluent socioeconomic characteristics. But all these calculations are changed markedly for black Hispanics, which once again demonstrates the powerful influence of black race on residential outcomes, and for Cubans. According to table 3, the predicted values for black Hispanics are 16 percentage points below those in table 4; in other words, black Hispanics do not achieve parity with whites under any conditions (although affluent natives come close). Approximately the same holds for Cubans.

²¹ The value in table 4 for affluent native Hispanics is impossibly high, although it clearly justifies the conclusion that they have attained parity with whites. The occurrence of such extreme values can be avoided by application of a logistic transformation to the dependent variable. We have not employed such a transformation here because it would remove the transparent connection of the analysis to the P^* segregation measure, which serves as an important motivation for our analysis. (A minor statistical inconvenience would also result, because some whites live in 100% white communities, but the logit transformation is not defined for the value 100.) However, as a check, we have repeated our analysis with the logit transformed variable. Aside from the obvious change in scale, the results are extremely similar to those in table 3, specifically in terms of the statistical significance and the relative size of coefficients. For the record, the predicted percentage of non-Hispanic whites in the community of an affluent native Hispanic is 95.8% in the logit results. The conclusion from table 4 is thus unaltered.

Despite this evident heterogeneity among Hispanic subgroups, the residential parity between some Hispanics and their non-Hispanic white peers suggests that the sizable aggregate difference between the two groups in table 2 is due to endowment differences to an important degree. This is also demonstrated when we decompose the mean differences in table 2, using the regression coefficients in table 3 and the group means for the independent variables (for the method we employ, see Jones and Kelley [1984]). In this decomposition, the sources of the difference in means (89.7% for non-Hispanic whites versus 70.2% for Hispanics) are partitioned into three types: group specific (including discrimination), endowment (or group composition), and the interaction between these two.²² For the white-Hispanic difference in means, the group-specific and endowment portions are nearly equal (at 15.5% and 13.6%, respectively, while the interaction portion is -9.5%). By comparison, very little of the white-black difference in means (89.7% vs. 57.8%) originates in endowment differences. Application of the decomposition formula reveals that group-specific factors constitute the greatest source by far (amounting to 30.1%, compared with 1.5% for the endowment portion and .4% for the interaction). Also, very little of the small white-Asian difference (89.7% vs. 85.4%) is compositional—in this case, however, because Asian suburbanites are as well endowed socioeconomically as whites.

CONCLUSION

In this article, we have examined segregation by analyzing locational patterns for individuals in terms of the racial/ethnic, socioeconomic, assimilation, and other characteristics that shape them. Our analysis has been guided by two major theoretical models—spatial assimilation and

²² The standard formula for the decomposition is (see Jones and Kelley 1984, p. 326):

$$Y_w - Y_m = (a_w - a_m) + \Sigma X_m(b_w - b_m) + \Sigma b_m(X_w - X_m) + \Sigma(b_w - b_m)(X_w - X_m),$$

where the *w* and *m* subscripts designate non-Hispanic whites and a minority group (Hispanics), respectively, *X* and *Y* the means of these variables, and Σ is a summation over the independent variables. Jones and Kelley show that the distinction between the first two terms on the right-hand side is not meaningful (unless all independent variables have a true zero point, something that is not true here). Both terms are classified (Jones and Kelley 1984, p. 338) as constituting the “unexplained differences” between groups, although we prefer to call them “group-specific factors” (including discrimination); the third term is the difference due to endowments, and the last is the interaction. One peculiarity in the setup of the current paper is the presence of subgroup terms, which are specific to each group. We have classified their contribution to the difference in means with the group-specific portion of the decomposition.

place stratification—and focused on the locational patterns in suburbia, where segregation might be expected to be attenuated. Our dependent variable—a community-level measure that we have described throughout in terms of proximity to whites—measures, precisely stated, residence in the same suburban community as whites. This is, of course, a form of proximity, or spatial and social nearness, but it need not imply that whites and minorities have much social contact with each other or even that they are neighbors on the same block or adjacent ones. (To be sure, none of the conventional segregation measures, which rely on aggregate data, can demonstrate actual contact across racial/ethnic lines or even neighboring, unless they are measured at a fine level of aggregation.) Residence in a community with whites obviously reflects only the possibility of contact with them. More important perhaps, it places minorities in a “community of fate” with whites; since numerous inequalities are rooted in communities, the presence of many members of the majority tends to preserve a community’s position of relative advantage. Our measure also links directly to a well-known measure of segregation, P^* , and one way of viewing our results is that they reveal how this measure of segregation is generated through individual-level locational patterns that map minority-group members with specific characteristics into suburban communities with specific majority-group proportions.

Although there is less segregation in suburbs than in central cities, we have nevertheless found salient differences across racial/ethnic groups in the level of segregation from non-Hispanic whites and, equally important, in the determinants (and, by implication, the processes) behind segregation. These differences can be understood in terms of our theoretical models if one grants that the models can vary in their applicability to different groups. The place-stratification model seems the more appropriate one for describing the locational patterns of whites and blacks. Our analysis shows that, for members of both groups, residential proximity to whites is determined substantially by race and little affected by other individual characteristics. For whites, this conclusion is perhaps to be anticipated from the demographic composition of suburbia, which is largely white and non-Hispanic. Yet the notion of place stratification still applies, for most whites appear to have little difficulty in avoiding those suburbs with disproportionate numbers of minority residents, even when they themselves have limited socioeconomic resources. Such resources also do little to help blacks achieve greater coresidence with whites: Only high income (or, less important for most blacks, residence in an institutional setting) allows entry into communities with greater proportions of whites.

The assimilation model, on the other hand, clearly applies to the patterns for Asians and Hispanics, but not identically. As other studies

of residential patterns have also found (e.g., Massey and Denton 1987; Waldinger 1989), Asians seem to be something of an exception to the classical ecological model, for many Asians are bypassing, or rapidly leaving, the initial stage of settlement in ethnic enclaves, typically in central cities. Consequently, individual assimilation status has little bearing on residential proximity to whites, according to our analysis. Socio-economic status has substantial relevance, however. Asian proximity to whites is increased by home ownership, high income, and a college diploma. We do not believe that these results are attributable to any distinctive characteristics of the Asians who reside in the New York area, for example, the possibility that an unusual number of them are employees of foreign companies and thus affluent sojourners. While this characterization does appear to fit many suburban Japanese in the New York region, the Japanese represent just a small part of the suburban Asian population, and other Asian groups do not fit this profile. Moreover, in analyses conducted separately for each Asian group, we find results similar to those we report here (Alba, Logan, and Leung, *in press*).

Hispanics show the most varied profile of effects. Both assimilation and socioeconomic variables play important roles. The assimilation variables, which indicate that foreign-born Hispanics and those who do not speak English at home are less proximate to whites, suggest the presence of suburban Hispanic enclaves to which the less assimilated members of the group attach themselves. There are elements of a stratification pattern, too. As expected, the racial appearance of Hispanics is associated with large residential differences. Hispanics also reveal the strongest national-origin effects, with Mexicans most likely to reside in proximity to whites and Cubans—unexpectedly—least likely.

As striking as the differences we have found across groups appear to be, they cannot fully resolve the tension between different theoretical models. There could be voluntaristic aspects to the patterns we have described as fitting the stratification model. For blacks, a well-known argument holds that segregation results from the differing preferences of whites and blacks with respect to the racial composition of their communities (Schelling 1971; see also Farley et al. 1978; Clark 1991). Most blacks wish to live in integrated communities where other blacks form a substantial portion of the residents, while most whites, though willing to live in the same communities as blacks, prefer a small black presence. Without data about preferences, we cannot be sure of how much black preferences for residing in communities with other blacks lie behind the patterns we have uncovered. But, on the basis of our findings, we are skeptical that preferences alone can explain these patterns. The highest-income black suburbanites should have the greatest ability to realize their community preferences; as we have seen, they reside in communities with

substantially greater white proportions than do other black suburbanites, suggesting that other blacks might reside in such communities if they could. There may also be an element of voluntarism in the unexpectedly high segregation of Cubans from non-Hispanic whites. In the New York region, many Cuban immigrants have settled in a few suburban communities—such as West New York and Union City, both in New Jersey—which thus resemble urban ethnic enclaves, but in a suburban location. These settlements have grown through the chain migration of family and friends, which has built upon initial cores created by a government refugee resettlement program in the early 1960s (Rogg and Cooney 1980, p. 12).

There may, however, be discriminatory elements in the patterns among Hispanics that appear to be consonant with the assimilation model. We have found that Spanish-speaking suburbanites reside in communities with higher minority proportions, a result consistent with the voluntaristic assumptions of the assimilation model (i.e., the less assimilated prefer, or are more comfortable in, ethnic communities). However, we cannot rule out the possibility that these individuals are excluded from largely majority communities.

There are also issues for future research with regard to the metropolitan context of suburban segregation. Our study has examined locational patterns in the large suburban region of the New York City CMSA, which is of special interest because of its great racial/ethnic diversity and because it receives a disproportionate share of the nation's immigration (Kraly 1987). Its segregation levels are, to be sure, not typical of the nation's metropolitan areas as a whole, since previous research shows segregation levels to be higher in larger metropolitan areas and in the older areas of the Northeast and Midwest (Massey and Denton 1987, 1988). Nevertheless, the New York levels appear to be roughly on a par with those found in other large, racially and ethnically diverse metropolitan regions, such as Los Angeles (see, e.g., Massey and Denton 1987, table 3). It is therefore of interest to know whether the locational patterns associated with segregation look different in metropolitan areas where minorities form a smaller presence than they do in New York. Where whites have less need to establish social distance from minorities because the latter are few in number, the stratification model may have less applicability. Likewise, it is of interest to know whether locational patterns for specific groups differ in metropolitan areas where they form a greater presence than they do in New York. Mexicans in New York City suburbs appear to be more proximate to whites than do other Hispanic groups, but they do not constitute a large minority there. Would the same finding be obtained for Mexicans in Los Angeles? Perhaps the stratification model would better fit Mexicans in Los Angeles than it does those

in New York. To take fully into account such contextual factors as a group's percentage of the population, it is, of course, necessary to repeat our analysis for New York with a substantial number of metropolitan areas. This must await future research.

Our concluding remark highlights our method. In investigating the role of metropolitan context in suburban segregation or indeed a host of other locational issues concerned with the character of the communities in which individuals reside, whether that character is measured by home-ownership level, average incomes, crime rate, or school performance, the method we have employed here offers, we believe, an important new tool. It is suited for studying locational processes at an appropriate level of detail, with individuals as the unit of analysis and with models specific to different metropolitan areas. Our hope is that it will facilitate a new vein of comparative community research.

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The Structure of Intergenerational Exchanges in American Families¹

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Intergenerational support is analyzed using data from the National Survey of Families and Households. The authors find evidence that a systematic latent structure of intergenerational exchange characterizes the giving and receiving of support. Overall, one-half of Americans do not routinely engage in giving or receiving relationships with their parents and only about one in 10 are engaged in extensive exchange relationships. Parents are assisted more often in situations of poor health, and more often receive assistance when they have young children. Assistance in time of need is not uniform and is rarely extensive. Intergenerational assistance is constrained by family structure and the needs and resources of each generation. African-Americans are consistently less likely than whites to be involved in intergenerational assistance. In each generation, men receive as much altruistic support as women; higher levels of giving and receiving of aid among American women are due to their greater involvement in exchange.

INTRODUCTION

Patterns of kin support, especially exchanges between parents and their adult children, have been described in many studies (see Mancini and Blieszner [1989] for a recent review). Despite this extensive research, the nature of kin support and its determinants remain elusive. In part, this is because much of the research has focused on patterns of discrete types of support (e.g., money, child care, emotional support) and has ignored the linkages between different domains. Previous research also shows a significant bias toward examining support systems for the elderly. Finally, by focusing only on support rendered, much previous work has

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failed to appreciate the extent to which this support is part of a larger exchange process involving receiving as well as giving.

In this study the nature of giving and receiving support between generations in American families is examined in light of these issues. We use data from the 1987–88 National Survey of Families and Households, a nationally representative sample of American adults with dependent children and aging parents, which contains a rich set of questions on exchanges of assistance. We begin by addressing the question of whether there is an underlying structure to the array of discrete types of commodities given and received by American adults and their aging parents. This structure will indicate the extent to which persons are involved in the routine giving or receiving of assistance and the patterned exchange of particular types of support. Gender, family status, and ethnic differentials in the structure of kin assistance are then established, and we examine the extent to which these differences in support persist in the context of each generation's constraints, opportunities for support, and resources. Finally, we address the degree to which that support results from altruism or the rational exchange of resources.

BACKGROUND

Changes in the American Family

American families have undergone many changes since the 1960s (Bumpass 1990). The formation of new nuclear families has slowed as age at marriage has increased. A growing proportion of men and women do not marry at all. An increasing number of young adults establish premarital residence independent of the parental family as the final step in their transition to adulthood (Goldscheider and Goldscheider 1989). Many women have delayed marriage and postponed childbearing until their thirties (Modell 1989); other women who are unmarried bear children in their teens or early adult years (Hogan 1987; Rindfuss, Morgan, and Swicegood 1988). Increases in separation and divorce have reduced the perceived permanence of marriage, while remarriage has led to more complex "blended" families (Furstenberg, Nord, Peterson, and Zill 1983).

These changes in family processes have made the early life course of American children both less stable and less economically secure. Single parents, especially mothers, often cannot achieve levels of earned income that meet their children's economic needs. As the proportion of children in single-parent families has grown, many more children grow up in persistently poor families (Eggebeen and Lichter 1991; Garfinkel and McLanahan 1986; Lichter and Eggebeen 1993). For both single-parent

and two-parent families, the underemployment and low wages now routinely encountered by many Americans have reduced economic stability (Sawhill 1988). Many married couples secure a middle-class life-style for their family by having both the mother and father work outside the home. Such a life-style is precarious in view of the currently high levels of marital instability.

These pressures on the traditional family with their consequences for the economic well-being of children are particularly pronounced among American blacks. This is partially connected to the composition of black families and households and to their somewhat larger numbers of children (Eggebeen and Lichter 1991). But the precarious economic well-being of black families is also connected to worsening employment prospects (Lichter 1988; Wilson 1987) and the decline in inflation-adjusted income transfers to poor families during the 1980s (Bane and Ellwood 1989).

While both black and white families have fewer children than in previous generations, the cost of raising a child has increased relative to the greater emphasis on child quality. Coincidentally, the government has shifted many of the costs of child rearing back onto parents (Preston 1984) by reducing assistance to families with young children and offering more modest tax benefits (calculated in constant dollars) to families.

Family Support Networks

These family and institutional changes form the context for recent theoretical and empirical attention to patterns of resource exchanges between generations by sociologists, economists, anthropologists, and demographers. The central thrust of much of this work is the development of models that link variations in kin networks to variations within and between societies in reproductive behavior, the care of children, and the care of dependent elderly (Lancaster and Lancaster 1987; Turke 1989). In high-fertility societies, extended kin networks disperse the costs of child rearing. Extended kin networks permit older children and the aged to enhance families' reproductive potential by assisting in the nurturance of children (siblings and grandchildren, respectively) who share their genetic heritage.

In more modern societies, social and economic resources are procured outside the family. Persons who limit their family size generally attain greater educational and socioeconomic success; these persons, therefore, may invest more heavily in their children's socioeconomic success. To focus their investment on fewer children, modern parents must reduce their investments in extended kin. The parents, in turn, will have less access to assistance from extended kin, thereby increasing the cost of

their own children and promoting a further reduction in fertility. The disappearance of sibling-provided child care accelerates this process.

In small-family societies, the parents' central concern is concentrating resources to ensure their children's social and economic success. Since children represent the major focus of the parents' reproductive heritage, parental investment in these children is expected to continue well into adulthood and will accelerate with the birth of grandchildren. Evolutionary theory suggests that parental investment will be diluted when a larger number of children compete for resources and will be more heavily concentrated on children who bear grandchildren. These hypotheses about intergenerational exchange in a small-family society are supported empirically by recent analyses (Eggebeen and Hogan 1990).

Economists have pointed to altruism (concern for the survival and well-being of progeny) as one factor leading to parental transfers to children (Becker 1974). An alternative view suggests that assistance is given to motivate reciprocal exchange, either concurrently or over the life course (Bernheim, Shleifer, and Summers 1985; Cox 1987; Hill 1970). The exchange value of kin assistance may diminish with the establishment of modern credit institutions and the increased difficulty in guaranteeing reciprocity by children (Willis 1982). Cox and Rank (1992) find that routine inter vivos transfer behavior in the United States is more consistent with the exchange model than with the altruism model. Similarly, Spitze and Logan (1989) infer that women's investment in care giving and kin keeping activities in their early and middle life course may create obligations in men and children that lead to assistance in later life.

By examining intergenerational assistance mainly in the context of the needs of the frail elderly, gerontological research has tended to present a one-sided view of intergenerational support (Mancini and Blieszner 1989). In both altruistic and exchange situations, the balance of resource transfers will be from the elderly parents (with no reproductive potential) to their children and grandchildren (Kotlikoff and Summers 1981), partially off-setting public transfers that favor the aged. This is true even in families in which the grandparents are old but not disabled (Eggebeen 1992; Eggebeen and Hogan 1990).

That kin play a strategic role in assisting families and individuals in need over the entire life course is well documented (Eggebeen and Hogan 1990; Rossi and Rossi 1990). Of all kin ties, however, the parent-child relationship remains the most important "stem" in the kin support network (Wellman and Wortley 1990), in part because this kin relationship has the most emotional salience for individuals over their life course (Rossi and Rossi 1990). Furthermore, other research shows that parent-child exchanges of support are most common when dependent grandchild-

dren are present (Eggebeen and Hogan 1990). Thus, the most appropriate focus for research on intergenerational support is on lineages that contain grandchildren.

While prior research has emphasized the "safety net" character of kin support, focusing on *potential* sources of assistance in time of need and on sources and types of assistance during a *crisis*, the neglect of routine, noncrisis assistance between adults and their aging parents has obscured the extent to which assistance during a crisis represents a fundamentally new behavior or is the result of a preexisting exchange strategy. In addition, the neglect of routine patterns of assistance has concealed the extent to which kin support typically is reciprocal at any point in time or over the life course. Understanding this structured linkage of giving and receiving is essential to a full understanding of the altruistic motives or exchange strategies that lead to intergenerational support.

Factors Affecting Intergenerational Support

Systematic differences in intergenerational giving and receiving have been observed by age, gender, ethnicity, family structure, socioeconomic status (SES), and proximity to kin. According to some research, the receipt of aid is greatest in childhood and old age, while giving behavior predominates among the middle-aged (Hill 1970). Others emphasize the continuity of support offered by aging parents to their children (Morgan 1982; Troll, Miller, and Atchley 1979). Rossi and Rossi (1990) find that parental help to children declines over time, but children's help to parents continues at the same level. Other data suggest that both receiving and giving decline with age overall, taking into account the life-cycle stage of the respondents and the age and health of their parents (Eggebeen 1992; Eggebeen and Hogan 1990; Cooney and Uhlenberg 1992).

Having a daughter is key to receiving assistance in old age (Spitze and Logan 1990). Sons are more likely to give financial or household assistance to their parents while daughters are more likely to provide advice and homecare. Daughters more often receive advice, household assistance, and child care (Adams 1968; Brody and Schoonover 1986; Eggebeen and Hogan 1990; Hagestad 1986; Lee 1979; Rossi and Rossi 1990; Stoller and Earl 1983; Taylor 1988; Troll et al. 1979).

Because of their documented involvement in mutual support activities, blacks, Mexican-Americans, and other Hispanic groups often are assumed to have stronger kin networks than whites, even though the studies from which these conclusions were drawn do not include comparable data for whites (Aschenbrenner 1973; Bastida 1979; Hays and Mindel 1973; Markides, Boldt, and Ray 1986; Martin and Martin 1978; Mindel 1980; Stack 1974; Taylor 1986, 1988; Wilson 1986). Studies that systemat-

ically compare kin assistance among representative samples of minority (black and Hispanic) and white families have not found superior support networks among minority families (Eggebeen 1992; Eggebeen and Hogan 1990; Hofferth 1984; Hogan, Hao, and Parish 1990).²

Single-parent families are more likely than two-parent families to depend on support from extended kin (Gibson 1972; Tienda and Angel 1982). However, evidence of the extent of reliance on kin by single-parent families shows that substantial numbers of female-headed families do not participate in kin support networks, even among blacks (Hofferth 1984; Hogan et al. 1990). This finding contrasts with past ethnographic work but is consistent with ongoing ethnographic studies of multigenerational black families (Burton 1991), suggesting that the ability of black families to support kin in need may have deteriorated as a result of the cumulation of economic and social disadvantage among a sizable segment of the black population (Wilson 1987).

Assistance involving face-to-face interactions, such as child care or the performance of household tasks, diminishes with physical distance. Some researchers note that other forms of assistance such as money, gifts, and advice or emotional support appear less affected by distance (Litwak and Kulis 1987), although Rossi and Rossi (1990) find that geographical distance reduces the incidence of all types of help between generations. Among the poorest families, young adults and aging grandparents are especially likely to reside with middle-aged parents. As family socioeconomic resources improve, coresidence becomes less common, in part because of the greater interhousehold resource flows possible in more educated and higher income groups (Goldscheider and Goldscheider 1989).

RESEARCH OBJECTIVES

As the review above demonstrates, earlier research has typically considered giving and receiving of particular types of aid (e.g., money and material resources, personal care to the elderly, child care for young parents, household assistance, or companionship and advice) individually. Several key research questions about the overall structure of kin support therefore are left unanswered: (1) Do the different forms of assistance each represent a separate dimension of aid, or are they multiple

² The lack of evidence for especially strong kin support in minority families is not an artifact of population coverage, sample design, or questionnaire construction. These studies have used three different nationally representative sample surveys: the National Survey of Families and Households (NSFH), the National Longitudinal Survey of Labor-Market Experiences—Youth Cohort, and the Panel Study of Income Dynamics (PSID).

indicators of some common underlying dimension of giving or receiving?³ (2) Are particular sorts of assistance commonly linked (reflecting the needs and resources of each generation)?⁴ (3) Do these various forms of assistance commonly come together so that giving is paired with receiving?⁵

The purpose of this article is to answer these questions using recent, nationally representative survey data on assistance between American adults with children age 18 or younger and their noncoresidential aging parents.⁶ We address questions 1 and 2 by examining the possibility that the eight dimensions of exchange measured here can better be represented by a few underlying constructs that classify families according to their overall pattern of exchange. We then describe sex, family status, and ethnic differentials in the structure of kin assistance. We also explicitly examine these differentials in the context of the variations in each generation's resource levels, their need for assistance, prior patterns of assistance, their affective relationships, and geographic constraints. Finally, we address question 3 by comparing individuals characterized as being extensively involved in exchanges with those who are primarily receivers of assistance.

DATA AND METHODS

The data for this study are drawn from the 1987–88 National Survey of Families and Households (NSFH 1988). This survey includes interviews with a representative national sample of 13,017 respondents age 19 and older. The NSFH respondents consist of a main sample of 9,643 respondents plus double samples of minorities (blacks and Hispanics), single-

³ Virtually all analyses of the giving and receipt of different forms of assistance have treated each form of aid as a unique dimension of assistance, either estimating multiple models to analyze the factors that affect the giving or receiving of each type of assistance, or estimating a model of the number of dimensions on which the giving, receiving, or exchange of aid occurs. The former procedure is likely to result in findings that appear statistically significant, but in fact are the chance result of multiple simultaneous tests. The latter procedure is almost never based on any measurement theory that would justify treating each form of aid as being equal and makes little sense if each form of aid is not an independent dimension.

⁴ For example, does the receipt of financial assistance tend to be linked with household assistance or companionship? Does the receipt of child care tend to be linked to giving an elderly parent a place to live?

⁵ Such information is necessary to distinguish altruistic assistance from assistance provided as part of an ongoing exchange relationship.

⁶ To avoid confusion about the generation being described here, respondents who provided this data will be referred to as "adults" or "parents," and the parents who are assisting or receiving assistance from them will be referred to as either "aging parents" or "grandparents."

parent families, families with stepchildren, cohabiting couples, and recently married couples.

A total of 5,071 respondents had at least one living parent, were living with a child 18 years old or younger, and either coresided with a parent or provided information about patterns of intergenerational exchange. These included 964 blacks, 3,575 nonblack, non-Hispanics (hereafter called "whites"), 338 Mexican-Americans, and 180 other Hispanics.⁷

The NSFH used personal interviews and supplemental self-administered questionnaires to gather detailed information on the respondents' family and socioeconomic life histories and on their kinship and social networks. The definitions, means, and SDs for the variables used in this analysis are shown in table 1. Key variables included on the basis of prior research include the respondents' ("parents' ") characteristics (age, gender, ethnicity, family structure, and measures of needs and resources), the respondent's aging parents' (the "grandparents' ") characteristics (who survives, education, and health status), and measures of the parent-grandparent ties (frequency of contact, distance, past care giving, quality of relationship).⁸

Of particular importance for this study are the definitions of assistance. Four dimensions of assistance are distinguished—monetary and material resources, care, work around the household, and companionship and advice. For each of the four dimensions of assistance, giving support and receiving support were measured separately (see table 1). Respondents were classified as exchanging monetary or material support with their aging parents if they reported giving (receiving) a gift or loan worth \$200 or more during the previous five years (excluding any help with a mortgage).⁹ Care assistance may assume two forms—child care or paren-

⁷ Ethnic origins could not be determined for 14 of the eligible respondents.

⁸ The NSFH includes relatively good measures for the characteristics of the parents and for their relationship with their aging parents, but includes only crude measures of grandparents' resources (education) and needs (health scale).

⁹ The NSFH questionnaire did not ask about small cash gifts or loans of less than \$200. We recognize that in some populations (such as the very poor) much more modest amounts of money might routinely be exchanged to help with groceries, to buy medicine for a sick child, or to "get by" until payday or welfare payment day. This weakness in the NSFH is not unique—the specially designed "transfers supplement" to the PSID, e.g., uses a \$100 floor. This feature of the questionnaire design is not likely to result in the misclassification of persons in the latent structure model as long as those persons giving or receiving small, unrecorded money transfers also engage in other forms of kin support. The ethnographic studies that point to such small money transfers as an important element of kin support record such transfers as only one element of kin support, of which care giving and help around the house also are a part. Someone giving and receiving on these dimensions would be recorded as a "high exchanger" in our latent structure models even if small money exchanges were missed.

TABLE 1
VARIABLE DEFINITIONS, MEANS, AND STANDARD DEVIATIONS

| Variable | Mean | SD |
|---|-------|------|
| Parents' characteristics: | | |
| Gender (1 = male) | .42 | .46 |
| Ethnicity: | | |
| Black | .12 | .30 |
| Mexican-American | .06 | .22 |
| Other Hispanic | .03 | .17 |
| Age in years | 35.10 | 7.42 |
| Married | .81 | .36 |
| Preschool-age child | .45 | .46 |
| Poverty (family income below U.S. poverty line) | .12 | .30 |
| Education (years of schooling completed) | 12.88 | 2.52 |
| Siblings (no. of brothers and sisters) | 3.69 | 2.38 |
| Health (1 = very poor, . . . , 5 = excellent) | 4.13 | .69 |
| Residence: | | |
| Suburban (metropolitan area outside of central city) | .18 | .36 |
| Nonmetropolitan | .27 | .41 |
| Grandparents' characteristics: | | |
| Number and gender: | | |
| Grandmother only alive | .25 | .40 |
| Grandfather only alive | .08 | .25 |
| Education (years of schooling completed by best-educated surviving grandparent) | 11.68 | 3.04 |
| Health (of sickest surviving grandparent, 1 = very poor, . . . , 5 = excellent) | 3.70 | .81 |
| Parent-grandparent ties: | | |
| Contact (frequency of visits, letters, phone calls between parent and most-contacted grandparent, 1 = not at all, . . . , 6 = several times a week) | 4.88 | 1.06 |
| Distance (log of distance between parent and nearest noncoresidential grandparent) | 3.56 | 2.44 |
| Home care in past (parent has provided coresidence to grandparent) | .04 | .19 |
| Quality of relationship (if both grandparents alive, lowest score is used, 1 = very poor, . . . , 7 = excellent) | 5.32 | 1.60 |
| Missing data: | | |
| Missing value on poverty (POVMISS) | .18 | .36 |
| Missing value on contact (CONTMIS) | .14 | .32 |
| Missing value on grandparent health (PHLTHMIS) | .03 | .15 |
| Missing value on grandparent education (PEDMISS) | .07 | .24 |
| Missing value on distance (DISTMISS) | .04 | .18 |
| Missing value on number of parents (PARMIS) | .07 | .24 |
| Missing value on siblings (SIBMISS) | .05 | .20 |
| Assistance:* | | |
| Gave money (parent gave or loaned grandparent at least \$200 in past five years) | .04 | .19 |

TABLE 1 (*Continued*)

| Variable | Mean | SD |
|--|------|-----|
| Received money (parent received gift or loan of at least \$200 from grandparent in past five years) | .16 | .34 |
| Received advice (parent received advice, encouragement, moral or emotional support from grandparent in past month) | .29 | .42 |
| Gave advice (parent gave advice, encouragement, moral or emotional support to grandparent in past month) | .26 | .41 |
| Gave care (parent gave help with grandparents' children or babysitting or parent cared for disabled or seriously ill grandparent in past 12 months) | .03 | .15 |
| Received care (parent received help from grandparent with child care for preschool child in past week or babysitting of any age child in past month) | .26 | .41 |
| Gave assistance (parent gave help to grandparents with transportation, repairs to house or car, or work around the house) | .31 | .43 |
| Received assistance (parent received help from grandparents with transportation, repairs to house or car, or work around the house) | .18 | .35 |

* Limited to parents with at least one noncoresident grandparent.

tal care. Child-care assistance was indicated by (a) help with babysitting or child care from the respondent's aging parents in the last month, or (b) if grandparents provide care for the young child of a working mother. Parental care was defined as providing home care for a disabled or seriously ill parent during the past 12 months. Household assistance occurred if the respondent indicated giving or receiving transportation, repairs to the home or car, or other kinds of work around the house during the past month. Finally, emotional support or advice is recorded for respondents who indicated giving (receiving) "advice, encouragement, moral or emotional support" to (from) their aging parents during the past month.¹⁰

The first step in the analysis was to determine the principles of kin support that underlie the observed patterns of giving and receiving on each dimension of assistance. Specifically, respondents were classified according to their status (yes/no) in regard to giving and to receiving for each of the four dimensions of exchange. The cross-classification of these eight variables provides 256 different cells (combinations of giving and receiving on the various dimensions) with each respondent assigned to a

¹⁰ In this article, sampling weights have been applied in the production of all descriptive statistics to correctly characterize the U.S. population of persons with one or more children under age 19 who have at least one surviving grandparent. Sampling weights were not applied in the multinomial logistic regression models, however. These models include controls for variables on which sampling stratification occurred, so it is not necessary to incorporate sampling weights in these models (Agresti 1990).

single cell. Latent class models (Clogg and Goodman 1985; McCutcheon 1987) were used to determine the minimum number of unique groups ("latent classes") needed to represent the association among items.

The latent class model characterizes a categorical latent variable (X) that corresponds in this setting to ideal-types of exchange. We describe this model supposing that just three items were under consideration, say, items A , B , and C , whose levels are indexed by i , j , and k , respectively. The model with an arbitrary number of items can be written in an analogous fashion. Let π_{ijk} denote the probability that a respondent is in cell (i, j, k) of the cross-classification of the three items. The latent class model with T latent classes is, then,

$$\pi_{ijk} = \sum_{t=1}^T \pi_X(t) \pi_{A|X=t}(i) \pi_{B|X=t}(j) \pi_{C|X=t}(k),$$

where $\pi_X(t)$ is the probability that $X = t$, $\pi_{A|X=t}(i)$ is the conditional probability that item A takes on level i , given that latent variable X is at level t ; the other conditional probabilities are defined similarly. Estimates of these model parameters, for the case in which there are eight items, are reported below. The model assumes that the items are mutually independent given X (the so-called axiom of local independence), which is an assumption analogous to that typically employed in factor analysis. (In common factor analysis, it is assumed that the correlations among the items are zero given the values of the latent factor or factors). The conditional probabilities are somewhat analogous to factor loadings in common factor analysis because they describe the association between a particular item and the latent variable under the model. In contrast to factor analysis, the latent class model does not require continuity or normality assumptions, does not rely on interitem correlations, and does allow characterization of the latent variable in a nonparametric way (i.e., in a way that does not assume continuity, normality, or sufficiency of means and variances). The latent class model is often portrayed as a method for characterizing ideal-types, which are categorical by nature; the common factor model, based on an assumption of one or more continuous factors, is difficult to reconcile with the perspective of ideal-types or discrete latent classes. Up to limits imposed by identifiability, we can consider latent class models with different numbers of latent classes (e.g., $T = 2$, $T = 3$, $T = 4$, etc.) without encountering the rotation problem in factor analysis (e.g., the two-factor model is not identified without imposing restrictions of some sort). It can be noted also that when $T = 1$, the conventional model of independence is obtained, and this model forms a natural baseline (see table 2). The similarities and differences

TABLE 2
FIT STATISTICS FOR ALTERNATIVE LATENT CLASS MODELS
OF INTERGENERATIONAL EXCHANGE

| Number of Latent Classes | Likelihood- Ratio χ^2 | <i>df</i> | <i>P</i> value | Index of Dissimilarity |
|-----------------------------|-------------------------------|-----------|----------------|---------------------------|
| 1 (independence) | 4,145.5 | 247 | .000 | .366 |
| 2 | 728.4 | 238 | .000 | .115 |
| 3 | 538.8 | 231 | .000 | .095 |
| 4 | 253.5 | 222 | .068 | .048 |
| 5 | 213.6 | 213 | >.5 | .042 |

between latent class models and factor models are taken up in McCutcheon (1987) and in references cited there.

Respondents were assigned to latent classes using estimates of the model parameters to calculate the probability that latent variable X takes on level t given that the observed items are at particular levels. For the three-item example used immediately above, we evaluate $P(X = t | A = i, B = j, C = k)$, for $t = 1, \dots, T$, and assign all respondents in cell (i, j, k) to the latent class that is modal for this response pattern. The predicted latent classes were then used in models that characterize gender, family status, and ethnic group differentials in the structure of kin assistance. Whether these differentials persist (taking into account group variations in each generation's resource levels, need for assistance, prior patterns of assistance, frequency of contact, and geographic constraints) was determined using a multinomial logistic regression model (Hoffman and Duncan 1988). Finally, a second set of multinomial logit models, in which "high" exchangers were the comparison group, was estimated in order to determine whether support involves altruistic giving or an exchange strategy.

The NSFH instructions for the questions eliciting information about assistance directed respondents to answer only for parents who did not reside with them. Only 167 (3.3%) of the study population resided with at least one parent; of these, 103 answered the support questions for another parent resident elsewhere. In order to make full use of the available data, we first analyze the latent structure of patterns of giving and receiving on each dimension of aid based on all respondents who answered those questions. Since coresidence can be a key form of intergenerational assistance, we wished to include persons in the multivariate analysis with a coresident aging parent, taking into account exchanges with that coresident aging parent, whether or not the parent responded to the exchange questions. We first do this by including a separate

"coresident" category in describing group variations in the latent class of intergenerational exchange.

Our *a priori* assumption is that such coresident parents and children engage in a high level of exchange. The NSFH did not include the questions necessary to test this assumption directly. However, respondents who had at least one child 19 years old or older were asked an analogous series of questions on giving and/or receiving assistance on a "regular basis" with each of their adult children.¹¹ This included assistance on several dimensions—listening to problems, providing family news, helping with household tasks, providing financial assistance, providing companionship, and child care. We randomly selected one coresident aging parent-adult child dyad from the interviews of the aging parents ($N = 969$). Most of these coresiders did engage regularly in giving and receiving assistance, indicating that it is reasonable to treat each person with a coresident parent as a "high exchanger" in the multivariate analysis.¹²

RESULTS

Latent Class Analysis: Types of Exchange

As described above, the 256 cells representing unique combinations of giving and receiving behaviors on the eight dimensions of exchange were subjected to a latent structure analysis. This first step in this analysis is determining the number of latent classes needed to characterize the data. As indicated in table 2 above, a model with four latent classes provides a substantially better fit for the data than a model with only three latent classes. This model provides an adequate fit to the data ($L^2 = 253.5$, 222 *df*; $P = .07$), while misallocating only 4.8% of cases. Adding a fifth latent class to the model significantly improves the statistical fit of the model, but this increased complexity buys little in terms of the correct classification of cases (4.2% misallocated) (Clogg and Sawyer 1981).¹³ Because of this, we decided to proceed with the more parsimonious model, which contains four latent classes. On the other hand, note that

¹¹ These respondents correspond to an independent sample of the aging parents of respondents whose experiences are represented in this article.

¹² Only 10% of the coresident parents reported not receiving aid from their adult child, 8% reported never giving any aid, and 4% reported neither giving nor receiving aid.

¹³ The large number of sample cases makes it relatively easy to find a statistically significant improvement in fit in the absence of much improvement in the correct assignment of cases. The Bayesian information criterion (BIC) index (Raftery 1986) is $-1,414.9$ for the three-class model, $-1,624.1$ for the four-class model, and $-1,587.9$ for the five-class model, which also indicates that the four-class model is preferred.

Intergenerational Exchange

TABLE 3

CONDITIONAL PROBABILITIES OF ITEM RESPONSES FOR EACH LATENT CLASS
FOR DIMENSIONS OF INTERGENERATIONAL EXCHANGE

| Dimensions of Exchange | Low Exchangers (1) | (Advice) Givers (2) | High Exchangers (3) | Receivers (4) |
|---------------------------|--------------------------|---------------------------|---------------------------|------------------|
| Give assistance | .079 | .440 | .789 | .469 |
| Receive assistance | .000 | .083 | .706 | .393 |
| Give care | .004 | .042 | .105 | .036 |
| Receive care | .024 | .197 | .799 | .570 |
| Give advice | .004 | .979 | .795 | .000 |
| Receive advice | .055 | .507 | .886 | .352 |
| Give money | .033 | .057 | .078 | .031 |
| Receive money | .099 | .029 | .279 | .225 |
| Latent class proportions: | | | | |
| Unweighted | .504 | .155 | .126 | .215 |
| Weighted* | .532 | .169 | .113 | .186 |

* The latent class models were estimated on unweighted data. The "unweighted" proportions were obtained by first estimating the latent class for each respondent, say X , and applying case weights to obtain the weighted proportions on the i th class of X .

models with two or three latent classes are not acceptable in terms of either goodness of fit or prediction of latent class.

Table 3 displays the maximum-likelihood estimates of the latent class proportions for the four-class model, and the conditional probabilities of item responses for each latent class for each of the eight dimensions of intergenerational exchange. These conditional probabilities, in combination with the latent class assignment of each of the 256 combinations into which all cases were classified (not shown), suggests a ready interpretation for each latent class.

Fifty-three percent of the U.S. population falls into the first latent class, which is associated with a low probability of giving and receiving on each of the four dimensions of exchange. Of persons in this "low exchange" category, the majority (72%) were not involved in *any* type of giving or receiving. The second latent class is associated with a high likelihood that the parents give support (particularly advice) to the grandparents, but have a low likelihood of receiving support from the older generation. Seventeen percent of American adults are (advice) givers. The complementary situation in which assistance received from aging parents predominates over assistance given defines the fourth latent class, which includes 19% of American parents. The third, and smallest, latent class contains 11% of the parents. This select group is highly involved in exchanges with grandparents, both giving and receiving support on several dimensions. The intensity of this exchange is noteworthy—these

"high exchangers" are more involved in giving support than those in the "giver" category (with the exception of advice) and are more likely to receive support than those in the "receiver" category.

Differentials in the Structure of Exchange

The distribution of persons grouped by sex, family SES, and ethnicity across these latent classes of kin support (with the addition of a fifth "coresider" class) indicate key differentials in the structure of kin support (see table 4). A majority of the fathers (60%) and nearly half of the mothers (46%) do not engage routinely in intergenerational exchange. Fathers are more likely than mothers to receive support. Mothers more often receive support from their aging parents, which they reciprocate (high exchangers). Mothers are almost three times as likely as fathers to coreside with an aging parent. Combining the high exchangers with the coresiders, 10% of the fathers compared to 19% of the mothers are involved in a high level of intergenerational exchange. Of all persons receiving assistance, 38% of the fathers and 51% of the mothers reciprocate.¹⁴

Sixty percent of the parents whose youngest child is of school age are in the low exchange group, compared to 42% of those with a preschool-age child.¹⁵ The presence of a preschool-age child especially is associated with being a receiver of support (24% vs. 12%) or being a high exchanger (15% vs. 7%), which suggests that the key supporting role is played by aging parents as their adult children begin to reproduce. Even so, 46% of the parents of a preschool-age child provide reciprocal support to the grandparents, compared to 48% of those parents whose youngest child is 6 years old or older. This finding reflects the exchange strategy that underlies kin assistance.

Smaller proportions of whites are low exchangers compared to other ethnic groups. Mexican-Americans are less involved in giving or receiving assistance than are whites or blacks. Other Hispanic groups are largely (77%) uninvolved in intergenerational assistance. Whites are more likely to be receivers of support than other ethnic groups; however, this might be partially a function of the higher average number of siblings (who are competitors for parental resources) of blacks and Hispanics.

¹⁴ This is calculated as the percentage of persons in the receiver, high exchanger, or coresider categories who are either high exchangers or coresiders.

¹⁵ We experimented with several different measures of the age and number of children less than 18 years old in the home. It appears that the only characteristic of children related to intergenerational assistance is whether or not at least one child is a preschooler. Number of children, number of preschool-aged children, and number of older children were not related to exchanges with grandparents.

TABLE 4

GROUP DIFFERENTIALS IN THE STRUCTURES OF INTERGENERATIONAL EXCHANGE

| POPULATION GROUP | χ^2 | LATENT CLASS OF EXCHANGE (%) | | | | | N |
|-------------------------------------|----------|------------------------------|--------------------|-----------|--------------------|-----------|-------|
| | | Low Exchangers | (Advice) Givers | Receivers | High Exchangers | Coreiders | |
| Gender | 101.0*** | | | | | | |
| Father | | 59.7 | 14.1 | 16.3 | 7.8 | 2.1 | 1,591 |
| Mother | | 45.8 | 17.8 | 17.8 | 12.5 | 6.0 | 3,120 |
| Ethnicity | 179.8*** | | | | | | |
| White | | 48.7 | 18.3 | 18.3 | 11.8 | 3.0 | 3,363 |
| Black | | 57.4 | 10.2 | 15.2 | 6.8 | 10.5 | 875 |
| Mexican-American | | 66.0 | 6.3 | 12.9 | 6.0 | 9.0 | 307 |
| Other Hispanics | | 77.1 | 8.4 | 7.6 | 1.7 | 5.2 | 156 |
| Gender and race | 240.7*** | | | | | | |
| White fathers | | 57.0 | 15.3 | 17.3 | 8.6 | 1.9 | 1,219 |
| White mothers | | 42.2 | 20.7 | 19.0 | 14.3 | 3.8 | 2,144 |
| Black fathers | | 69.9 | 8.8 | 14.5 | 6.2 | .6 | 227 |
| Black mothers | | 51.1 | 10.9 | 15.5 | 7.1 | 15.4 | 648 |
| Parent of preschool-age child | 246.8*** | | | | | | |
| Yes | | 42.1 | 13.4 | 24.1 | 15.3 | 5.1 | 2,204 |
| No | | 59.7 | 18.6 | 11.5 | 6.6 | 3.8 | 2,507 |
| Unmarried mother | 18.4*** | | | | | | |
| White | | 38.9 | 15.0 | 19.1 | 12.6 | 14.4 | 756 |
| Black | | 45.4 | 7.9 | 16.9 | 5.9 | 23.9 | 446 |
| Close family ties | 223.6*** | | | | | | |
| Yes | | 33.2 | 15.0 | 25.7 | 16.5 | 9.6 | 1,042 |
| No | | 56.6 | 16.6 | 14.9 | 8.9 | 3.0 | 3,669 |

*** $P < .001$.

When those who are residing with parents are combined with those who are high exchangers, whites, blacks, and Mexican-Americans appear to be equally involved in exchanges of support. Forty-five percent of whites receiving support reciprocate, compared to 53% of blacks and Hispanics.

Black males are substantially more likely than white males to be low exchangers (70% vs. 57%). These fathers are almost as likely as the same-race mothers to receive unreciprocated assistance. Overall, however, the fathers receive considerably less support than the mothers, with the mothers being much more likely to reciprocate. Thirty-seven percent of the white mothers receive support from their parents, with half of them reciprocating. Black mothers are equally likely to receive support (38%), but more often reciprocate it than do whites (59% vs. 49%). In part, these differences reflect racial differences in the number of surviving parents. In these data, only 49% of black males have two living parents, compared to 61% of black females and nearly 66% of whites (tabulations not shown).

Unmarried mothers of both races are likely to have a greater need for kin support and they, in fact, more often receive support (46% of whites and 47% of blacks). Even so, it is noteworthy that less than half of the single mothers receive significant parental support. Reciprocity is common even among these single mothers (59% of whites and 64% of blacks). It is surprising that 15% of the single white mothers report that they give, but do not receive, intergenerational assistance. This pattern is only half as common among blacks.

Are these modest levels of ongoing intergenerational support the by-product of life in a highly mobile society in which family members live far apart and have only infrequent contact or poor parent-adult child relationships, or is it an essential element of American family life? To address this question, we identified the 21% of respondents with close family ties (live within 10 miles of their aging parents, have contact with them at least once a week, and report their relationship to be 6 or 7 on a 7-point scale where 7 represents "excellent").¹⁶ Parents with close family ties to their aging parents are much more likely to be engaged in some form of intergenerational support (67% vs. 43%), mostly because they are more likely to receive aid from the grandparents (52% vs. 27%). Among those receiving aid, 50% with close family ties and 44% without close family ties reciprocate. The likelihood that these parents give unreciprocated aid to their aging parents is largely unaffected by lack/presence of close family ties. The relatively low level of support we observe

¹⁶ Even though coresiders by definition meet two of the three criteria for family closeness, only 50% were classified as having close family ties. Living together is no guarantee that parents will rate their relationship with their aging parents as of high quality.

is partly due to the lack of close ties among many American families; but it is also a feature of those families that have maintained close family ties.

This leads us to the more general question of the extent to which the American structure of intergenerational exchange is conditioned by the needs and resources of the parents and the grandparents. Do such aspects of family life produce the observed ethnic and gender differentials in kin support? Table 5 displays the results of a multinomial logistic regression model designed to address these questions. As discussed above, persons classified as high exchangers or coresiders in table 4 were combined into a single "high exchanger" category in table 5. Ethnicity and gender were key independent variables. The model included controls for resource levels and need for assistance in each generation, frequency of contact, and distance.¹⁷ The multinomial logistic regression model shows how the probability of being in a particular outcome category (advice giver, receiver, or high exchanger) versus the likelihood of being in the low exchanger group is modified by particular independent and control variables.¹⁸

Measures of the needs and resources of parents and grandparents are related in complex ways to the pattern of kin support. Parents in poverty are significantly less likely than persons with higher incomes to be involved in either the giving or receiving of support. Parents with more years of education are significantly more likely to be involved in giving advice or in a high exchange relationship with their aging parent. They also are somewhat more likely to receive unreciprocated support from the grandparents, although the magnitude of this effect is only about half as large as that associated with giving advice or being a high exchanger. More educated grandparents are also more likely to receive verbal support from the parents or to be involved in a high exchange relationship, but grandparents with better educations are no more likely to give unreciprocated support.

The pattern of kin support is only weakly related to the reported health status of each generation. Parents respond to the poor health of the grandparent by giving more advice. The pattern of intergenerational exchange is unaffected by the health of the parent.

Controlling for access to kin and for SES reveals few residential differ-

¹⁷ The model estimated also included controls for missing data on seven key variables, as shown in table 1 above. To simplify the presentation of this complex model, the coefficients for these variables are not shown in table 5 below.

¹⁸ The initial log likelihood of $-6,343.7$ is decreased to $-4,628.1$ for the model with all covariates included. This model decreases the proportion of cases misclassified by 9.0%.

ences in informal support. Parents in rural areas and in cities are equally likely to engage in intergenerational exchange, suggesting that neither a community's family culture nor its level of formal resources have much effect on informal kin support.¹⁹ The only significant difference across communities is the greater likelihood that urban residents will give verbal support to their aging parents compared to suburban residents.

The family structures within which these intergenerational relationships are worked out have an important impact on the nature of kin support. If only one grandparent is alive the parents are much less likely to receive a high level of support (either reciprocated or unreciprocated). Parents are about equally likely to give unreciprocated advice to a grandparent regardless of which survives. Parents with more surviving siblings are less likely to receive support (either reciprocated or unreciprocated) from their aging parents, but number of siblings has no effect on the likelihood that the parent will give regular advice to the grandparent. Grandparents are less able to help an individual child when they have had more children, indicating that the hypothesized trade-off between number of children and quality of relationship persists even into the later years of the family life. The greater investment by grandparents in more educated children also is consistent with this observation.

Net of the effect of poverty status and presence in the household of a preschool-age child, a married parent is more likely to give advice and less likely to receive support than a single and formerly married parent. In particular, a married person is only about 74% as likely as an unmarried person to be involved in a high exchange relationship with the grandparent. Grandparents are 85% more likely to provide support (both reciprocated and unreciprocated) to an adult child who is raising a preschool-age child. These findings confirm that grandparents frequently take an active role in providing resources to their progeny when parents face periods of especially heavy child costs or when they lack adequate human capital in the nuclear family to secure the future of their children. The absence of any similar increase in assistance to families in poverty more likely is a function of a lack of *resources* in the grandparent generation (something we could not measure) than to a lack of grandparents' *readiness* to invest in their descendants.

The pattern of intergenerational exchange is strongly influenced by the physical and emotional ties between the parents and grandparents. Net of level of contact and quality of relationship, distance has a modest but significant effect on the assistance provided by aging parents to their

¹⁹ While our measure of community services is crude, this finding does provide evidence with a national data set that confirms the lack of effect for formal community characteristics reported by Spitze and Logan (1990) for the Albany metropolitan area.

TABLE 5
MULTINOMIAL LOGISTIC REGRESSION MODEL OF THE STRUCTURE
OF INTERGENERATIONAL EXCHANGE

| INDEPENDENT VARIABLE | CONTRAST: MULTIPLICATIVE EFFECT | | |
|------------------------------------|---|-------------------------------------|---|
| | Advice Giver versus Low Exchanger | Receiver versus Low Exchanger | High Exchanger versus Low Exchanger |
| Parents' characteristics: | | | |
| Gender (male) | .540*** | .888 | .569*** |
| Ethnicity: | | | |
| Black | .567** | .672** | .477** |
| Mexican-American | .535** | .810 | .987 |
| Other Hispanic | .432* | .438* | .281** |
| Age | .987 | .947** | .945** |
| Married | 1.276* | .817 | .743* |
| Preschool-age child | .895 | 1.854** | 1.888** |
| In poverty | .765 | .593** | .447** |
| Education | 1.193** | 1.107** | 1.264** |
| Number of siblings | .970 | .957* | .954* |
| Health status | 1.078 | .979 | 1.057 |
| Residence: | | | |
| Suburban | .708* | .968 | .846 |
| Rural | .808 | 1.098 | .865 |
| Grandparents' characteristics: | | | |
| Number and gender of grandparents: | | | |
| Grandmother only alive | .846 | .618** | .507** |
| Grandfather only alive | .506 | .332** | .017** |
| Education | 1.047** | 1.020 | 1.057** |
| Health status | .800** | 1.055 | .974 |
| Parent-grandparent ties: | | | |
| Level of contact | 1.506** | 1.739** | 1.930** |
| Distance | .960 | .804** | .758** |
| Home care in past | 1.228 | 1.297 | 3.487** |
| Quality of relationship | 1.043 | 1.038 | 1.073* |
| Intercept | .010** | .069** | .004** |

NOTE.—A coefficient of 1.000 indicates that the independent variable has no effect. Coefficient > 1.000 indicates an increased likelihood of a given outcome relative to the likelihood of being a low exchanger. A coefficient < 1.000 indicates a reduced likelihood of a given outcome relative to the likelihood of being a low exchanger. Other variables in this model are POVMISS, CONTMISS, PHLTHMIS, PEDMISS, DISTMISS, PARMIS, and SIBMISS (see table 1 above).

* $P < .05$.

** $P < .01$.

adult children. Distance does not affect the likelihood that parents give (unreciprocated) advice and verbal support to their aging parents.²⁰ All forms of kin support are more likely when parents and grandparents maintain a higher level of contact.²¹ It is surprising, however, that the quality of the relationship appears to have little effect on the giving or receiving of verbal support, except for a modest association with the high exchange pattern.

Parents who have provided home care for a sick grandparent in the past year are 3.5 times more likely to be involved in a high exchange relationship with their aging parents at the time of interview. Estimation of a similar model that excludes parents and grandparents currently coresident from the high exchanger category showed that about half of this pattern of high exchange is due to the association of prior parental home care and current coresidence. Even excluding this association, parents and grandparents are significantly more likely to be involved in a high exchange relationship if the parent provided the grandparent with home care in the recent past. However, neither unreciprocated giving nor receiving of assistance is associated with prior home care. Apparently, the parents maintain a high level of support for frail grandparents after an incident of home care, and the grandparents try to reciprocate this support.

²⁰ Eugene Litwak suggested that the latent structure categories used here, while adequate for characterizing the overall structure of assistance, were not refined enough to provide a fair test of his theories. In an attempt to respond to this criticism and see whether a more detailed breakdown of patterns of assistance can shed further light on Litwak's hypothesis that distance limits mainly those forms of assistance that require interpersonal interaction, with other forms of assistance occurring through modern communication and transportation technologies, we estimated logistic regression models for the provision and receipt of each type of assistance. These models show that grandparents' advice, child care, and household assistance decline with distance. Grandparents at greater distance do not substitute financial assistance for these other forms of aid to their adult children. Distance is not a deterrent to home care by those American adults whose aging parents are seriously ill, but distance does deter them from providing routine assistance around the house. Adult children who live at greater distances are more likely to give financial assistance to aging parents, suggesting an attempt to substitute the purchase of services for their actual provision. Litwak's hypotheses are supported for the provision of assistance by American adults to their aging parents, but do not seem to apply to the giving behavior of the grandparents. This may, in part, reflect a greater facility of the younger generation with the use of modern communication and transportation technologies.

²¹ It is possible, as one reviewer pointed out, that contact presents an endogeneity problem because some forms of exchange inevitably involve visits, letters, and phone calls. Obviously, the cross-sectional nature of the data make it difficult to disentangle the relationships between these factors. To check on the robustness of our findings, however, we reran the multinomial models without the parent contact variables. The coefficients for the remaining variables remained virtually unchanged.

Controlling for factors relating to resources, needs, family structure, and grandparent-parent ties reveals persistent gender effects on the provision, but not the receipt, of aid. While one grandparent is less engaged in giving assistance or high exchange than two surviving grandparents, a sole surviving grandmother is 86% more likely to give assistance than a sole surviving grandfather and more than 19 times as likely to be engaged in high exchange. In contrast, the level of (unreciprocated) assistance provided by parents to a surviving grandparent does not differ significantly by the grandparent's gender. In a parallel fashion, fathers are equally as likely as mothers to receive (unreciprocated) support from their aging parents, but are less than 60% as likely to give assistance (either reciprocated or unreciprocated) to their aging parents.²² Because mothers are more often in an exchange relationship with their aging parents, they are more likely than fathers to receive aid (combining the reciprocated and unreciprocated groups).

Clearly, the strong gender differences in child nurturance persist over the life course. American men invest less often in their children both when they are young and when they are adults with their own children. But the children are about as likely to give advice and other verbal support, but not coresidence, to their aging fathers as to their aging mothers. Gender differences in giving persist across the generations—more often than not, a daughter rather than a son provides assistance to an aging parent.

Ethnic differences in the pattern of intergenerational exchange persist (and increase for blacks) when the effects of resources and needs, family structures, and parent-grandparent ties are taken into account. Mexican-Americans less commonly engage in giving advice to their aging parents than do non-Hispanic whites, but they are about equally likely to receive aid or to engage in mutual support. The observed lower participation of Mexican-Americans in any form of exchange relationship (table 4) is accounted for by the greater distances Mexican migrants live from their parents and the constraints this imposes on exchange. Other Hispanics are much less likely to be involved in any form of kin support, even controlling for distance. They are considerably less likely than Mexican-

²² We looked for but found no statistically significant interaction between gender of parent and gender of surviving grandparent on kin support. Neither fathers nor mothers have distinctive patterns of kin support depending on whether a sole surviving grandparent is male or female. We do not have data on assistance provided to fathers and mothers—only to “parents”—making it impossible to test whether distinct mother-daughter patterns of giving occur in those families in which both the aging father and aging mother are alive. Such data would be necessary for an exact replication of the findings of Rossi and Rossi (1990). The more limited replication for widows and widowers possible with the NSFH data is not consistent with their findings.

Americans to receive assistance or to be involved in a high exchange relationship.²³

Blacks are consistently less likely than whites or Mexican-Americans to be involved in any sort of intergenerational assistance. They are only 48% as likely as whites to be involved in a high level of intergenerational exchange, and only 67% as likely to receive (unreciprocated) support from parents. Black parents are only 57% as likely as whites to give advice to their aging parents.²⁴ The low level of intergenerational support among blacks is consistent with other recent studies (cited above) that use national data to compare kin support among blacks and whites. These results also are consistent with recent ethnographic research that suggests that multigenerational black families in need often lack the financial and human capital resources to meet the needs of all generations adequately (Burton 1991).²⁵ Older blacks especially find themselves facing significant demands with limited resources. We speculate that the ethnographic research of the 1960s and 1970s that portrayed strong and exceptionally effective kin support networks among multigenerational, matrifocal black families may represent a bygone era of limited relevance today. We believe that ethnographic studies should be conducted to determine the state of black communities in the 1990s.

The multinomial logistic regression model in table 5 contrasts the probability that persons with given characteristics are in each of the three types of intergenerational support with their probability of being in the

²³ One reviewer suggested that the relatively low level of advice and verbal support by both the Mexican-American and other Hispanic parents may reflect a cultural reluctance (rooted in filial piety) to give or acknowledge giving advice to aging parents. We cannot test this idea directly, but if it is accurate it seems likely that such a constraint would be stronger for adult daughters than for sons. We do find a statistically significant interaction consistent with this hypothesis—among both the Mexican-Americans and other Hispanics men were more likely than women to report that they give advice to their aging parents.

²⁴ Tabulations exploring race and gender differences in coresidence reveal that 14% of black mothers are coresident with a grandparent, compared to 4% of white females, 2% of white males, and less than 1% percent of black males. Logistic regression models indicate that this relationship remains after the introduction of extensive control variables. The lower probability of blacks being in the high exchange group in the model in table 5 occurs despite the classification of these coresiders as high exchangers. It is possible that the lower probability that blacks are recorded as high exchangers is an artifact of the pooling of black men and black women. To check this possibility we added a race-gender interaction term to the model in table 5; no statistically significant interaction is present.

²⁵ By human capital we mean sound physical and mental health, as well as educational credentials and labor-market experience. The high rates of drug use by poor young black mothers, and the accompanying social disorganization that accompanies a community's drug culture, seem to have had a markedly negative impact on the effective functioning of black families (Burton 1991).

TABLE 6

MULTINOMIAL LOGIT MODEL PREDICTING RECEIVERS RELATIVE TO HIGH EXCHANGERS

| Variable | Receiver |
|--------------------------------|----------|
| Parents' characteristics: | |
| Gender (male) | 1.561** |
| Ethnicity: | |
| Black | 1.409* |
| Mexican-American | .821 |
| Other Hispanic | 1.560 |
| Age | 1.002 |
| Married | 1.099 |
| Preschool-aged child | .982 |
| In poverty | 1.327 |
| Education | .876** |
| Number of siblings | 1.004 |
| Health status | .926 |
| Residence: | |
| Suburban | 1.144 |
| Rural | 1.269 |
| Grandparents' characteristics: | |
| Grandmother only alive | 1.220 |
| Grandfather only alive | 19.416** |
| Education | .964 |
| Health status | .921 |
| Parent-grandparent ties: | |
| Level of contact | .901 |
| Distance | 1.060** |
| Home care in past | .372** |
| Quality of relationship | .967 |
| Intercept | 16.406** |

NOTE.—The other contrasts (giver/high exchange, low/high exchange) are not reported. Other variables in this model are POVMISS, CONTMISS, PHLTHMIS, PEDMISS, DISTMISS, PARMISS, and SIBMISS (see table 1 above).

* $P < .05$.

** $P < .01$.

low exchange group. This model does not provide the information needed to assess differentials in the extent to which support from grandparents to parents is reciprocated. To obtain a comparative test of the altruistic and exchange models of giving, we reestimated the multinomial logit models selecting the high exchanger group as the contrast category. Table 6 contains just the contrast of interest: the receiver group relative to the high exchanger.

Each additional year of schooling increases the likelihood that parents will reciprocate assistance by the grandparents, but other measures of each generation's resources have no impact. With the exception of grand-

fathers who are widowers, the needs of each generation and family structure are unrelated to the likelihood that parents reciprocate grandparental aid. Parents who have, in the past, provided home care for a grandparent continue to reciprocate assistance from these aging parents. Speculatively, it seems that grandparents continue to have an exchange relationship with their adult children once such a pattern is established. Distance functions to increase the reception of support relative to the exchange of support. American fathers are considerably less likely than American mothers to reciprocate aid received from their aging parents. Thus, we find that women in each generation are more likely to give intergenerational assistance than are men, because they more often exchange aid even though they are not more often the beneficiaries of altruistic aid. Finally, blacks are significantly more likely than whites to be receiving aid rather than exchanging assistance.

CONCLUSIONS

Intergenerational support takes many forms among contemporary American families, including the giving and receiving of money and material resources, care, household assistance, and companionship and advice. Most studies of support between American adults with children and their aging parents have tended to regard these domains as discrete, focusing on the correlates and determinants of each type of assistance. As demonstrated here, these data can be much more simply analyzed by first determining the systematic latent structure of intergenerational exchange that characterizes American families.²⁶ We find evidence that different forms of assistance can be regarded as multiple indicators of an underlying construct of giving and receiving. Thus, with the exception of giving advice, the particular commodity (money, household assistance) matters less than whether one is a high exchanger or a receiver. Over half of all Americans were found to be uninvolved in exchanges and only about one

²⁶ Others have noted this difficulty in analyzing social support and have attempted to reduce the complexity of the data by searching for underlying patterns. Wellman and Wortley (1990), e.g., used factor analytic techniques to reduce 18 kinds of support rendered by network members to the respondents. They report that six factors are necessary to describe the patterns observed in their data set. They label the factors according to broad types of social exchange: emotional aid, small services, large services, financial assistance, and companionship. Their focus on support rendered (as opposed to both giving and receiving assistance), as well as on support drawn from all network members (rather than just between parents and adult children) undoubtedly contributes to the differences in the structure of support they report relative to that observed in our data. Consistent with our focus, however, they find that the parent/child bond is broadly supportive (e.g., this tie does not specialize in particular domains of assistance).

in 10 were extensively involved in giving and receiving support. However, these overall patterns varied considerably by demographic characteristics and the needs and resources of each generation. Finally, we find evidence consistent with both exchange and altruistic explanations of exchange patterns.

This analysis differs from earlier studies of family support by looking at the experiences of parents with children rather than at the experiences of the old and the disabled. This means that the intergenerational assistance studied here refers to grandparent–adult child dyadic relationships; studies of the elderly typically have focused on support from any of several children. This clearly is a critical difference: in any grandparent–adult child dyad, 53% are low exchangers. If we assume for the moment that this figure applies to every grandparent–adult child dyad, grandparents with two children would have only a 28% chance of being a low exchanger, with this figure diminishing to 15% among those with three children. Thus, while intergenerational support is not typical among American families, aging parents are likely to be involved in giving and/or receiving aid with at least one of their adult children. This distinction is especially important to keep in mind when making comparisons across ethnic groups. One reason that black and Mexican-American parents receive comparatively less assistance than whites from grandparents is because they have, on average, more siblings competing for this support. On the other hand, from the perspective of these minority grandparents, more adult children can mean higher levels of support (Eggebeen 1992).

This study also differs in its emphasis on actual kin support in populations rather than on support obtained during a crisis or on hypothetical sources of support during future periods of need. The incidence of intergenerational support at any particular time is much lower than the lifetime prevalence of support. As previous studies have supposed, family support can provide an important security net for family members (both old and young) during periods of high need (e.g., preschool children, illness, or disability). Even in such situations, however, significant proportions of parents are relatively uninvolved in exchanges with grandparents. For the parents, this may mean that family support is absent during times of need, particularly when grandparents have several children who may compete for aid. For the grandparents, the likelihood of family support increases significantly with number of children, especially daughters, so that grandparents are more likely than parents to get intergenerational assistance in times of need.

Ethnic differences in the incidence of intergenerational support present a quite different view of the pervasiveness of family support networks than prior ethnographic research or specialized surveys of minority elderly would suggest. The strong familism of the Mexican-American pop-

ulation is an oft-noted feature of that group. Clearly, even in populations where familism is strong, the migration experience—creating geographic distances between the generations and reducing their frequency of contact—seriously diminishes intergenerational support. Taking this constraint into account, we find few differences between the intergenerational support experiences of Mexican-Americans and the majority population. Routine intergenerational support occurs about as often among blacks as among whites. However, taking racial differences in family structure, needs, and resources into account, black families are less involved in intergenerational assistance than white families. Multigenerational black families in need often lack the financial and human capital resources to meet the needs of all generations adequately (Burton 1991). The societal changes that have weakened black communities and reduced the life chances of many black men and women have meant that many families are being placed in situations containing great demands and limited resources. The effective kin network that provided support to multigenerational, matrifocal black families in past decades appears to be of limited relevance today.

Prior research has indicated that, besides need, a variety of factors condition the likelihood of intergenerational support. These include the socioeconomic resources of each generation and frequency of contact between the grandparents and the parents. This research confirms that ongoing contact with aging parents is a key correlate of aid. Families with fewer socioeconomic resources are less involved in kin support. While need is a factor in decisions about the allocation of aid, families with fewer resources are less able to provide assistance. To the extent that socioeconomic resources are correlated across generations, those very families most needing support will be least able to provide it.

Physical distance remains a significant barrier to the exchange of aid. Child care and household assistance, for example, become impractical. Grandparental advice to parents diminishes with distance, too, even after taking into account frequency of contact. Grandparents at greater distances do not substitute financial assistance for other forms of support. For the parents, then, physical distance creates a serious barrier to the receipt of grandparents' assistance.

For the grandparents, physical distance is less of a barrier to aid. Distance is not a significant deterrent for their children (the parents); they are as likely to provide care during times of need and are even more likely to provide grandparents with monetary assistance. Thus research results suggesting that modern methods of transportation and communication have greatly diminished the effects of geographic access on the receipt of family support are truer from the grandparents' viewpoint than from the parents' viewpoint. One possible reason is that younger cohorts

of Americans are more adept at using these technologies than are their parents. Whatever the explanation, this finding produces an important qualification on the work of Litwak and Kulis (1987).

Intergenerational support in the United States involves a mixture of altruistic giving and an exchange strategy. Grandparents give support to their adult children, especially when those children are parents of a preschool-age child. Grandparents continue to give as long as they are able over their entire life course, especially to those children who bear them grandchildren. Adult children also tend to increase their support to parents in times of need. Even in the times of greatest need, nearly half of all persons receiving intergenerational support also give support, indicating the key role reciprocity may play in sustaining an exchange relationship among kin. The more prominent role of women in kin support is due to their sustained participation in exchanges between the generations. Speculatively, a woman may benefit more from a lifelong investment in an exchange strategy than a man—in the early adult years she is more likely to need assistance as a single parent and in her later years she is more likely to survive her spouse, living to a frail old age. Paradoxically, then, the very exchange strategy characteristic of high fertility populations in less developed nations may have been adapted by women to cope with the new needs of American families in the 1990s. While the evidence is consistent with this description, it is important to remember that many American families—both black and white—lack the resources for a sustained exchange of support and, in fact, do not give or receive support.

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Book Reviews

Is Anyone Responsible? How Television Frames Political Issues. By Shanto Iyengar. Chicago: University of Chicago Press, 1991. Pp. viii + 195. \$19.95.

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Shanto Iyengar examines the influence of television news on how people attribute political accountability and form political opinions and attitudes. He assumes that audience members engage in the same reasoning process as television news producers. They do not have, nor do they want, a great deal of factual knowledge about an event, let alone about the issues it raises. Rather, they want simplification or commonsense knowledge, which they pursue by reducing political issues to questions of accountability. Political opinions and attitudes follow from how they answer these questions.

Iyengar pursues his research through a series of fine-grained empirical studies conceptualized as follows. Television news is presented within either episodic (specific event context) or thematic (general issue context) frames, which bear implications for accountability. It attributes accountability through the way it presents people with particular attributes (e.g., gender, race, age, socioeconomic standing) as being victims or perpetrators of social problems. It also attributes accountability by adopting a particular political focus, for example, emphasizing how actions by responsible authorities are meeting policy objectives or, alternatively, dwelling upon controversial decisions by authorities. Iyengar analyzes how these formats of television news are presented in national newscasts that address security problems (crime, terrorism), welfare problems (poverty, unemployment, racial inequality), and the Iran-Contra affair. He studies whether audience members exposed to national newscasts about these problems attribute causal accountability (the locus of responsibility for the existence of a problem) and treatment accountability (the locus of responsibility for alleviating a problem) to societal or individualistic forces. He also researches whether there is a spillover effect in attributions of accountability and opinions from one topic area to another, and in the formation of political attitudes generally.

Iyengar uses multiple methods. Content analyses of ABC, NBC, and CBS newscasts allowed him to identify the degree of thematic versus episodic framing for each topic studied and to refine his experimental study materials so that they were consistent with what appeared in the

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news. Nine field experiments involving paid subjects recruited through newspaper advertisements form the primary basis on which Iyengar assesses the influence of television news formats on peoples' attributions of accountability and opinion formation. Secondary analyses of national survey data provide supplementary evidence concerning the effect of attributions of accountability on political opinions and attitudes regarding specific issues as well as with respect to the president of the United States.

While the questions asked are within the mainstream of research on mass media and public opinion, the methodology is creative, and there are many significant findings. The different methods complement each other. For example, the content analyses are used to build the experimental manipulations and parameters for the experimental field studies. In its methodological innovations and some of its findings, this is an important book and essential reading for specialists in media and public opinion.

Only a few of the significant findings can be pointed to here. There are important differences in how people attribute accountability in terms of the characteristics of victims or perpetrators of problems as seen on television news. For example, when news focused on white-collar crime, societal accountability was attributed most frequently, but when news focused on black violent crime, individual accountability was emphasized. Societal accountability for poverty lessened when the poor person depicted was black, and individual accountability for poverty was at its peak when the impoverished person was a black, adult single mother.

Episodic framing, especially regarding security problems, dominates television news. Episodic framing leads people to see accountability in individualistic terms (single problem, single issue, single individual responsibility, single immediate solution), rather than in terms of wider social forces and problems understood through a consistent ideological lens. For example, episodic framing of security intensifies attributions of individual responsibility and thus support for more punitive measures such as the death penalty and military retaliation against terrorism. Episodic framing of poverty reduces support for welfare programs and increases approval for politicians who advocate cutbacks for such programs. Hence television news is proestablishment. It desensitizes people to structural problems and the need for structural reform, and it relieves politicians of the need to take stronger reform actions.

Iyengar operates within the standard conception of the television audience as a "public" of isolated, atomized individuals who receive content unidirectionally and are therefore passive spectators. He ignores recent sociological research that gives emphasis to the fact that *organizational* audiences actively participate in news as part of their overall strategy of organizational communication and enacting their environments. They respond to the ways in which news communications affect organizational plans, routines, and careers, incorporate the news media into their everyday accountability work, and use the news as a power resource to influ-

ence other organizations in their environment. In other words, far from being passive spectators, organizations actively participate in and help to constitute the news-media institution.

Iyengar is not reflexive enough about the limitations of his methods. Reflexivity is important given that the main thrust of this research is the limits of television news methodology and how the discourse of television news circumscribes political understanding and positive reforms. The field experiment methodology favored by Iyengar has many of the limitations he attributes to television news. For example, it too is episodic, representing a brief moment in the life-long political socialization of the people who participated. Iyengar's depiction of how folks follow television's episodic news frames and adopt the commonsensical mentalities and sensibilities of those frames may be an artifact of his experimental designs, which are themselves episodic and frame matters in terms of fixed-choice, commonsense categories.

Furthermore, Iyengar repeatedly argues that public opinion and political behavior are less dispositional (i.e., reflect party affiliation, ideology, personality) than circumstantial (i.e., are dependent on immediate contexts, including "the winds of news coverage"). However, this finding could also be an artifact of the experimental design format. If opinions constantly shift by audience and context, this is also true of the experimental research audience and context and how it is framed by those who produce it. In essence Iyengar exposed people with a lifetime of political socialization to a breathtaking hurricane of television news, then documented how their attributions, opinions, and attitudes might have become a little bent in the process. However, he has no idea about what happened to those attributions, opinions, and attitudes once the persons he studied moved into different contexts. They might have expressed as much distance from and cynicism about the research as people usually do about television news.

In the concluding chapter Iyengar suggests how his findings support the dominant-official-ideology thesis. Episodic news frames—resulting from a combination of medium-format criteria, market considerations, and the occupational routines of journalism—fortuitously strengthen the legitimacy of officialdom by trivializing public discourse. Television news is the opiate of the masses and should itself be held accountable for the erosion of electoral accountability. It fosters an apathetic and even ignorant political environment in which politicians do not have to take significant actions to remedy security and welfare problems and the people can also avoid the burden of having to participate in and pay for substantial reforms. However, Iyengar's own data indicate that the political minds of Americans are not quite so closed, tight, and simple. While Iyengar's findings may be an artifact of the experimental research format, they are not as clearly supportive of the dominant-ideology thesis as he wishes them to be. He himself stresses that people who belong to a given "issue public" tend to have dissimilar overall opinion profiles and that there are no global opinions that allow prediction of issue-specific opin-

ions. Moreover, many of the detailed findings go in different directions, and some contradict the overall thesis. Why not celebrate the plural, fragmented, and diverse nature of political attributions, opinions, and attitudes, and be thankful they are not as precise, logical, consistent, and systematic as television news formats and experimental research formats frame them to be?

Make Room for Television: Television and the Family Ideal in Postwar America. By Lynn Spigel. Chicago: University of Chicago Press, 1992. Pp. 236. \$42.00 (cloth); \$15.95 (paper).

Christine LaFia
University of Chicago

Make Room for Television: Television and the Family Ideal in Postwar America is a cultural history about America's response to television's entry into domestic and private life during the post-World War II years from 1948 to 1955, a time when nearly two-thirds of American households purchased television sets. Spigel offers five chapters that begin with background on the history of domestic life and family amusements to summarize types of leisure activities for the middle class from the Victorian to the broadcast era. The four chapters that follow center on the way television became part of people's daily routines during the initial years it was installed in American homes. This work departs from much of the current research on television by not gliding through the initial years of television's entrance into American life. The initial years are its subject. By making seven years of the post-World War II period the focus of analysis, many questions not previously answered are addressed: How was it that Americans allowed television to enter family life during this period? How did television affect women's daily routines? Why were the sitcoms so powerful in capturing familiar social life and bringing family life into living rooms?

Chapter 2 offers a look at "Television in the Family Circle." Television watching—whether daytime programming with cooking, sewing, and homemaking advice, afternoon children's programs, or evening prime-time entertainment—became part of daily life. Spigel examines articles and advertisements in women's magazines and memos and reports from advertising executives and network programmers to identify how television became part of domestic American life and why programming content took the shape it did. This book reaches into the debate on the cooperation between print media, advertising, and television to demonstrate how television entered society's cultural arena and zoomed to the forefront by joining forces with other media. These media that make up the dominant cultural arenas for Americans supported a cultural, social, economic, and political "return to home" ideology for

American families, with men in the work force and women in the domestic sphere.

Spigel signals to readers that, contrary to popular thinking about television in the 1950s, there was public debate on television's entrance into America's domestic life. Popular magazine and newsprint articles discussed the possible effects of television viewing on family life, child rearing, and women's roles. There was much skepticism, concern, and confusion surrounding television entering private life in its initial years. This is valuable knowledge because television has now been part of our cultural repertoire for more than 40 years and is a habitual part of daily life. Most Americans watch six hours of television a day.

Spigel's examination of magazine advertisements suggests that "at least from the advertisers' point of view, television sets were both a gender and class-specific item" (p. 190, n. 7). Copies of persuasive magazine ads from the 1950s are displayed throughout the book. These offer an illustration of the aggressive techniques used by advertisers to target female viewers, particularly the white middle-class sector who were full-time homemakers.

In chapter 3, "Women's Work," Spigel demonstrates the way network executives discussed ways to insert programming into women's home-making routines so that television viewing would become a part of their days. This explains why the representation of women did not feature married women who combined family life and successful careers. Targeted consumers were women whose incomes were provided by white male breadwinners. These provided them with the purchasing power needed to buy products advertised on television. Television programs and advertisements offered women glimpses of other women at home watching television as a part of neighborhood life.

Spigel does not examine media content exclusively to account for the meaning of television in American society. Instead, she considers the technological invention of television and how it altered private life in middle-class America. She observes that "television's capacity for transmitting sight as well as sound would give its programs a sense of credibility that radio lacked, while its intimate privatized address would create a more compelling simulation of reality than film ever could" (p. 138).

Television is appealing to Americans because it reconstructs family life in ways that resemble real life but offers intimacy, immediacy, and spontaneity. This is discussed in chapter 5, "The People in the Theater Next Door," in which Spigel examines television and its emphasis on domestic life by showing how the sitcom "grew from a curious new contraption to a familiar cultural form" (p. 136). Television content echoed values and messages of family harmony at a time when a cultural movement for a back-to-family life was in motion. In contrast, earlier television formats borrowed from theater, vaudeville, and film to offer variety programs oriented more to settings outside domestic life.

This book contains over 550 well-prepared notes (pp. 189–225) that

offer reading suggestions ranging from early books about women and consumerism (Christine Frederick, *Selling Mrs. Consumer*, 1929) to more current readings on television (H. Baehr and G. Dyer, *Boxed In: Women and Television*, 1987). It is somewhat difficult to sort through them in the absence of a bibliography at the end of the text. Nonetheless, these notes represent an unusually rich compilation of magazine citations, broadcasting memos, books, and contemporary reading materials. They offer suggestions for further reading about television in American culture from the postwar period to the present.

Spigel's treatment of television during the early years contrasts strongly with much of the current research on television and offers mass media researchers, cultural sociologists, and those interested in American cultural history an exemplary piece of work. It widens the debate on television and offers a window into a brief but critical time in history, a time when tremendous social forces welcomed television into domestic life and encouraged women to remain at home.

Shared Pleasures: A History of Movie Presentation in the United States. By Douglas Gomery. Madison: University of Wisconsin Press, 1992. Pp. xxi + 381. \$15.95.

Eric Schaefer
Emerson College

Motion pictures have generally been approached from two distinct vantage points: as individual texts and through their audience. Yet the site where the text meets the audience has been largely ignored. Film exhibition has been the subject of several florid books on movie palaces, and there have been microstudies of towns or individual exhibitors. But clearly a gap in the scholarship on motion pictures has existed. With *Shared Pleasures: A History of Movie Presentation in the United States*, Douglas Gomery attempts to remedy the situation. Gomery's two-decade commitment to the economic history of the motion picture industry is fused with his more recent work on exhibition to offer the first historical overview of movie presentation. The book is organized in three major sections: part 1 offers a business history from the first Kinetoscope parlors to postmodern picture palaces and home video. Part 2 gives concise histories of alternative operations, including newsreel theaters, theaters for African-Americans before and after segregation, other ethnic cinemas, and art houses. The final section, part 3, is devoted to technological changes.

Although the film industry's production apparatus was centralized geographically, economically, and ideologically by the end of World War I, exhibition was characterized by its diverse and scattered nature. Theaters were found in both the largest cities and the smallest towns and ranged from massive palaces that could seat thousands to run-down halls that

held fewer than 200. The last four decades have seen even greater decentralization of the film experience as television and home video have become the primary site of motion picture viewing. This is testament to the broad range of Gomery's topic and to the inherent difficulty in writing about exhibition. The study of movie presentation presents a major methodological challenge if every theater and living room in the country can be considered a site of exhibition.

Gomery has adjusted to this problem by concentrating his efforts on the largest and most powerful movie houses, which were controlled by the major studios, and the biggest chains, which evolved after the Paramount decrees in 1948. From this vantage point he explains how a variety of monopolistic practices served as the basis for control of the fully integrated studio system and how major chains continue to dominate exhibition, both theatrically and in video. Although the central theme of the book and mode of analysis is economic, Gomery never lapses into strict economic determinism. *Shared Pleasures* is at its best in several case studies, especially when Gomery combines his economic analysis with geography and social history. For instance, his description of the rise of Chicago's Balaban and Katz chain, which eventually became the keystone of the Paramount theater empire, shows how one company gained preeminence in a major city. Even though the vast majority of theaters were owned by small chains or individuals with only one or two houses, this top-down analysis can certainly be justified since small concerns were always at the mercy of the major outfits. Also, from a practical standpoint the largest operations left the longest paper trail for the historian. However, a close examination of some rural or subsequent-run neighborhood operations and the audiences they catered to would have provided a more balanced picture of exhibition during the golden age of Hollywood.

The second section of the volume more fully captures the diversity of the filmgoing experience. Here Gomery examines theaters designed for groups differentiated by race, ethnicity, and class. In quick strokes he describes the social and economic forces that led to the development, and the eventual disappearance, of these movie houses. Throughout *Shared Pleasures* the author dispels some lingering myths, notably that the nickelodeon was a haven for immigrants and the working class. As Gomery notes, "Democracy's theaters lasted only as long as it took entrepreneurs to find a broad based middle-class patronage (a few years in most locations), one which would guarantee higher stable profits" (p. 311). He also demonstrates that television was a secondary factor in the decline of movie attendance in the 1950s, giving it less influence than has often been ascribed to it. Several major cultural products that were given a boost by their exposure in movie theaters (among them, air conditioning, popcorn, and Coca-Cola) are covered in detail. Indeed, Gomery's attention to detail is commendable and his research is marked by both breadth and depth.

Unfortunately, *Shared Pleasures* is marred somewhat by slack editing.

Needless repetition of facts and phrases occur, and a variety of typographical errors appear, all of which should have been snared by editors. Still, with *Shared Pleasures* Douglas Gomery has provided scholars and general readers alike with a solid history of motion picture exhibition in the United States. Beyond this, the book fulfills the important function of laying the groundwork for future scholarship on film exhibition, particularly in the area of social history.

Salsiology: Afro-Cuban Music and the Evolution of Salsa in New York City. Edited by Vernon W. Boggs. Westport, Conn.: Greenwood Press, 1992. Pp. 416. \$49.95.

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It has always surprised me how the rich cultures of Latin America and the Caribbean, including their immigrant communities in the United States, have received so little attention in this country from both cultural sociologists and others working in the interdisciplinary field of cultural studies. This neglect is even more glaring in work related to musical genres and their associated subcultures or communities. The publication of the anthology *Salsiology: Afro-Cuban Music and the Evolution of Salsa in New York City* is an attempt not only to give voice to "salseros" and their predecessors but to direct sociologists and other academics to the richness of "Afro-Hispanic" culture and its long and significant relationship with North American culture. This book is an excellent resource for a history of Afro-Hispanic music in New York City, the Caribbean, and Latin America. While weak in its sociological analysis and explanation, it still provides a number of examples of how cultural forms are created and disseminated across national, ethnic, racial, and class boundaries.

The positive contribution of *Salsiology* lies in the wealth of information it provides on the history of Afro-Cuban jazz. Through essays and interviews, the reader can piece together an informative history of the emergence of Afro-Cuban jazz and salsa in New York City. Vernon W. Boggs, the editor of the anthology, provides a short informative history of salsa in New York City in his two essays, "Founding Fathers and Changes in Cuban Music Called Salsa" (pp. 97-105) and "The Palladium Ballroom and Other Venues: Showcases for Latin Music in N.Y.C." (pp. 127-32). The interviews also provide a rare opportunity to read the viewpoints of individuals directly involved in an art world, from promoters, disk jockeys, and club owners to bandleaders, musicians, and dancers. Besides the focus on Afro-Cuban jazz in New York, this work also provides a glimpse at the evolution of Afro-Hispanic musical styles in the Caribbean, Latin America, and even its interaction with music in Europe and Africa.

Unfortunately, *Salsiology* provides no simple guide to the logic of its organization. An introductory chapter would have been helpful for any reader in order to navigate and sift through the large and detailed information presented. The authors also provide no detailed explanation of the sociological concepts and issues being addressed. For example, the explicit frame of reference for the anthology is the concept of transculturation elaborated by Cuban musicologist Fernando Ortiz, yet this concept is never really explained in any significant detail anywhere in the book. Jorge Duany's essay, "Popular Music in Puerto Rico," is the most sociologically oriented essay and provides a number of insights on the effects of race, class, and urbanization on popular music (pp. 71-89). Basically, the sociological analysis and conclusions in *Salsiology* are in the hands of the reader willing to do the work.

One aspect of this work worth highlighting is the large number of examples a careful and sociological reading can find of musical innovation and change and its relationship to geography, race, class, urbanization, and migration. This work is constantly describing the variety of ways in the 19th and 20th centuries that different Afro-Hispanic musics changed and interacted with other musics, both Afro-Hispanic and others. As far as I can tell from the book, the concept "transculturation" does not actually apply to many of these examples, since it seems to be confined to either the process in which music of a low-status group slowly assimilates up through a class and racial/ethnic hierarchy, or more vaguely, to the general diffusion of African musical elements to America started by the diaspora. For example, African slaves in Cuba incorporated Iberian musical elements into their music, as did slaves throughout colonial America. Is this transculturation? Is it reverse transculturation? Is it acculturation? I believe that the history of musical "articulation" in Afro-Hispanic music and its influences on other music is too complex to fit the linear and restricted practice suggested in the first definition of transculturation. Dick Hebdige's books, *Subculture: The Meaning of Style* (New York: Methuen, 1979) and *Cut 'n' Mix* (New York: Methuen, 1987), also suggest the inadequacy of this first definition for properly addressing all the complicated musical articulations constantly going on in urban centers around the world. In the second definition, transculturation only applies to one specific culture and its diffusion. What concept or concepts apply to the large number of other instances of cultural diffusion or syncretism? Maybe transculturation should best be redefined as the general process of articulation of diverse musical elements from different national cultures, and from there one can show the ways in which race, class, gender, or market (or colonial/neocolonial) relations affect this process. Also, as Peter Manuel suggests, maybe a focus on cities as the loci between foreign musical sounds and national/folk sounds might be useful (*Popular Musics of the Western World* [New York: Oxford University Press, 1988]).

Sociologists interested in the study of popular music should find this

book useful and enjoyable to read. A cultural sociologist interested in finding new theoretical or substantive insights into the general study of popular art or culture, however, will probably be disappointed in this anthology.

Men, Masculinity, and the Media. Edited by Steve Craig. Newbury Park, Calif.: Sage Publications, 1992. Pp. 271. \$19.95.

Lauren J. Pivnick

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While a significant body of scholarship has addressed the relationship between gender and the mass media in recent years, this research, as pointed out by editor Steve Craig, has been primarily confined to the exploration of portrayals of women and femininity in popular culture. *Men, Masculinity, and the Media* is a welcome corrective. Collected here are articles that range widely in topic as well as in theoretical and analytical complexity, from straightforward content analyses of images of masculinity in comic books to feminist poststructuralist explorations of the male gaze in prime-time television programming. The strength of the volume is in both the range of theoretical orientations included and the representation of scholars from a variety of disciplines including sociology, communications, political science, and the humanities.

The articles in the volume share an emphasis on gender as a social construction, their analyses focus on the ways in which multiple *masculinities* are constructed, challenged, and, generally, reconstructed through the mass media. The volume is divided into five sections. Part 1, "Past Study on Men and the Media," begins with Fred Fejes's review of existing empirical research on masculinity in media, describing men's portrayal in television, advertising, film, music, and pornography, as well as outlining the major theoretical and methodological orientations that have historically characterized this research. In the second piece in this section, Diana Saco reviews early statements drawn from the feminist poststructuralist and semiotic analyses of popular culture that have, in many circles, come to eclipse more traditional empirical methods. These introductory articles are helpful in mapping the "territory" of contemporary media studies concerning masculinity.

Two noteworthy articles are to be found in part 2: "Case Studies of Media and Masculinities," which explores the role of media as an agent of gender socialization, and Lance Strate's analysis of the lessons of masculinity embedded in beer commercials, which offers an incisive look at the male worlds of work, leisure, friendship, and intimate relationships presented in advertisements, where, the author concludes, the "myths of masculinity and femininity" (p. 92) are almost universally upheld. Also of interest in this section is "Metal Men and Glamour Boys" (S. Denski and D. Sholle), which explores the meanings of heavy metal

music for its primarily adolescent male (though increasingly female) audiences, while also addressing some perplexing gender contradictions in the androgynous presentations of the subgenre of "glam metal."

The selections in part 3, "Representations of Men's Relationships," explore television images of men as friends, fathers, sons, and war buddies. In her historical overview, Lynn Spangler finds few examples of significant emotional bonds among male characters in prime time with the interesting exceptions of working-class heroes Ralph Kramden and Ed Norton and their rural counterparts Sheriff Andy Taylor and Deputy Barney Fife, whose tender friendships challenge the action/adventure formula characterizing early Westerns or more recent programs such as "Magnum P.I." and "The A-Team." Venise Berry's analysis of television portrayals of African-American fathers and male teens seeks to explore audience perceptions of the veracity of these roles, but is weakened by her failure to address the differing historical contexts of the two shows used for comparison, the 1970s situation comedy "Good Times" and the 1980s "Cosby Show."

The most consistently strong pieces are found in part 4, "Men, Media and the Gender Order." In her discussion of images of men and masculinity in advertising, Diane Barthel suggests that advertisers seek to invest products aimed at male consumers with gendered symbolism in part to counteract the presumed femininity of the act of consumption itself. David Croteau and William Hoynes's piece "Men and the News Media" catalogs the clear overrepresentation of men as media "experts" on programs such as ABC's "Nightline" and examines the role of journalistic conventions of objectivity in shaping the news. Don Sabo and Sue Curry Jansen provide an excellent overview of recent literature examining masculinity and commercial sport and offer an analysis which, unlike many of the other selections in the volume, incorporates a critical economic perspective. They conclude that, "mediated sport is . . . a nexus of patriarchal ritual that reproduces hegemonic forms of masculinity and femininity as well as competitive values and achievement ideologies that are closely tied to class ideology" (p. 173).

The final section, part 5, "Reading Mediated Masculinity," brings together pieces that explore the process of media interpretation or "reading" by audiences. Authors in this section conclude that multiple masculinities may be presented to male television viewers in order to reach wider markets (R. Hanke), that programs featuring attractive male characters may subtly play on the sexual desires of both female and male audiences (C. Steinman), and that the globalization of U.S. media has had a powerful effect on conceptualizations of gender abroad (J. Hearn and A. Melechi).

Men, Masculinity, and the Media makes an important contribution to our understanding of the role of mass media in reinforcing traditional concepts of gender identity and traditional bases of male dominance. While the range of media forms addressed here is impressive, the volume would have benefited from more extensive discussion of recent prolific

forms such as MTV. Overall, however, it is a welcome addition to this growing field.

Women's Two Roles: A Contemporary Dilemma. By Phyllis Moen. New York: Auburn House, 1992. Pp. xii + 172. \$45.00 (cloth); \$16.95 (paper).

Linda D. Molm
University of Arizona

In the past 50 years women's labor-force participation has changed dramatically, but the societal context in which women's work roles are embedded has not. Men's work and family roles remain much the same, the structure of occupations continues to be based on the structure of the traditional family, and public policy in the United States is still largely one of governmental noninterference. This unevenness in social change has created one of the great dilemmas of modern life, not only for women but for society.

In this book, inspired by Alva Myrdal and Viola Klein's 1956 classic, *Women's Two Roles: Home and Work*, Phyllis Moen examines how trends since World War II have created increasing conflict between work and family spheres and discusses the consequences for women, families, and social institutions. She notes that Myrdal and Klein's book was written at a time when women's work roles were expected to be intermittent during the life course, and that their solutions are now out of date for the increasing number of women who work continuously, including those with small children and infants. She seeks more contemporary answers to the problem of how to mesh family and work roles.

Moen begins her analysis by describing trends in women's labor-force participation and related demographic and social changes (e.g., marriage, fertility, educational attainment, wages). She then discusses the consequences of these changes for women (chap. 3); for marriage, families, and children (chap. 4); and for employers, unions, and government (chap. 5). While much has been written on the issue of combining work and family roles in recent years, Moen's use of data from multiple time periods provides a distinctive perspective. Particularly in chapters 3 and 4, she addresses most issues (e.g., how women's employment affects their well-being or their children's sense of self-worth) by comparing several different categories of people across several different points in time. This wealth of data makes the book a valuable resource for selective information gathering.

Not surprisingly, given Moen's background in social structure and personality and life-course analysis, she places particular emphasis on indicators of the psychological and physical well-being of women and their husbands, measures of children's development, and attitudes toward work. This emphasis on individual-level survey data at times seems to undermine her central thesis, that the problem is societal rather than

individual. However, she continually returns to that theme and supplements the individual-level analysis with information on day-care facilities, governmental policies, and corporate initiatives.

In the final chapter Moen discusses a variety of solutions, finally advocating (not surprisingly) one that she calls the "Life Course" solution. This approach involves reconfiguring the life course in ways that would provide more options and flexibility, with the aim of reducing the heavy work loads experienced at particular times of life. She suggests that these might include cutting back on working hours while children are young, with changes in employment policies to remove the costs that such part-time employment currently entails, and then continuing paid work beyond the usual time of retirement. While Moen proposes these changes for both men and women, she does not describe what would bring about such a major change in men's roles. Nor does she address the economic consequences for families of two part-time workers during the child-rearing years. Without far more fundamental societal change, it seems likely that such flexibility would simply produce more individual solutions.

These are very complex problems, and the short length of this book—only 133 pages of text—limits what Moen can accomplish. There is little opportunity in this amount of space to go beyond description to more in-depth theoretical analysis. Some relevant topics, such as the difficulty of coordinating job searches and moves and the consequences for women's careers, are omitted altogether, and others are discussed quite briefly. As a result, it is somewhat difficult to know the most appropriate audience for the book. The topic, length, and level make it an ideal candidate for an advanced undergraduate course on gender, but undergraduates would have a difficult time with the amount and complexity of the descriptive statistics. Professionals and graduate students would find it a useful overview but a fairly superficial one. As it stands, the book's strengths are Moen's clear statement of the societal nature of the problem and the need for societal solutions and the wealth of trend data she skillfully compiles. Given the importance of the issue and Moen's expertise in the area, I wish she had extended the book by about 100 pages.

Women, Family, and Utopia: Communal Experiments of the Shakers, the Oneida Community, and the Mormons. By Lawrence Foster. Syracuse, N.Y.: Syracuse University Press, 1991. Pp. xx + 353. \$37.95 (cloth); \$16.95 (paper).

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Utopian social experiments can touch a sensitive societal nerve precisely because they take on central cultural dilemmas. One of the pervasive

dilemmas, as Sigmund Freud argued in *Civilization and Its Discontents*, centers on the problematic relationship between sexuality and social life. In *Women, Family, and Utopia*, historian Lawrence Foster builds on his earlier book, *Religion and Sexuality* (Oxford University Press, 1981), by examining 19th-century resolutions of the sexual question in three utopian religious movements—the Shakers, the Oneida community, and the Mormons. Foster shows how sexuality is linked to social organization, theology, and the intimate experiences of rank-and-file participants. He provides an important touchstone for the continuing debate about patriarchy. And he brings to life utopian counterpoints that resonate deeply with the cultural contradictions of gender and heterosexuality in the late 20th century.

I doubt that many people today would be comfortable with the solutions that Foster describes. Yet our era is marked by an odd admixture of liberation and constraint that has its parallels in the social ferment of the antebellum 19th century. Foster employs an interpretive and empirical historical method to get at three utopian cultural logics at work in that period. Most broadly, he argues that the alternative utopian responses were part of an emergent “Victorian” culture increasingly oriented toward social control of sexuality.

The Shakers had the most negative coding of sexuality. English founder Ann Lee had experienced four difficult childbirths and lost her children in their early years. She “became increasingly terrified of all sexual intercourse, avoiding her bed as if it were ‘made of embers’ ” (p. 22). Finding little support in England for her campaign against passions of the flesh, the woman who was to become “Mother Ann” moved to America in 1774, separated from her husband, and began to attract a following. The Shakers practiced strict celibacy coupled with an ecstatic form of ritualized dancing which must have offered some release from the otherwise libidinally repressive ethos. As Foster shows, their extreme resolution of the sexual question grew out of a theological condemnation of lust, and it required confessional mechanisms for the cleansing of carnal sin that fed into group social control within a tight hierarchical organization. In part because the strict separation of the sexes required leaders of both sexes, women achieved a certain degree of political equality, but the same principle led to a highly gendered division of labor that maintained women’s traditional lines of work.

Oneida was similarly founded through what Foster terms a “prophetic” confrontation with sexuality. Yankee minister John Humphrey Noyes is depicted as a shy man with an interest in controlling social interaction in ways that would allow unconventional development of his own sexual experience. In his struggles with how sexuality might assume a “Godly” form, Noyes rejected the “special love” of couples in favor of “complex marriage”—based on males’ coitus interruptus and members of both sexes taking on various sexual partners. Sexual expression was detached from individualism and made a basis of group love (and commitment). Males may have experienced frustration with complex

marriage, Foster notes, but women "evidently found the practice an improvement" (p. 84). With carefully controlled procreation, communal child rearing, and the absence of any rigid sexual division of labor, women experienced unusual freedom. In the face of recent feminist critiques of Noyes, Foster hastens to add that women at Oneida lived under the auspices of Noyes's paternalistic authority, an authority that channeled sexual expression partly on the basis of social status within the community (p. 96).

Noyes's paternalism may seem benevolent compared to 19th-century Mormon polygamy. Founder Joseph Smith seems to have had personal sexual motives for advancing polygamy, but these do not explain the practice. Many Mormons—men and women—resisted what they viewed as an abhorrent sin. But polygamy integrated female converts and built male kinship, loyalty, and power through in-law relations and childbirths by plural wives. The practice amounted to a giant sexual pyramid scheme, linking men's status with the accumulation of wives and Mormon relatives, offering more than one woman the status privilege of marriage to elite men. This was patriarchy with a religious face. But Foster rejects outright feminist dismissals of patriarchy as too simplistic, since "in some instances" women derived "self reliance and independence" (p. 190).

Foster's work will find its critics—for its arguments and its antitheoretical bias. The book under review would have benefited from the perspectives of Lévi-Strauss, Foucault, and sociological variants of rational choice theory, to mention just a few resources. Subsequent inquiry thus may yield more analytic leverage than Foster has achieved. But theorists need to think about concrete situations. *Women, Family, and Utopia* deserves a wide audience: it offers an important empirical benchmark for theoretical and utopian consideration of the sexual question.

How Fascism Ruled Women: Italy, 1922–1945. By Victoria De Grazia. Berkeley and Los Angeles: University of California Press, 1992. Pp. 384. \$29.95.

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Victoria De Grazia has written a challenging, fascinating history of the ways that the Italian fascist state sought to mobilize women. The book draws imaginatively on diverse primary sources and on a burgeoning, but still limited, Italian historiography. By embedding her study within the broader theoretical debates and comparative historiography of women in the interwar years, De Grazia has written a book that should command the attention of anyone concerned with how women emerged as political and social subjects in advanced industrial societies. The breadth of the text and the boldness of the endeavor can be seen in the

multiple meanings ascribable to "rule." De Grazia uses it not only to encompass direct political representation but also in the Foucauldian sense concerning how women's behavior was disciplined in both the public and private spheres.

Fascism had an essentialist vision of women—it was concerned with their role as mothers. A declining birth rate threatened Mussolini's ambitions to make Italy a grand imperial power. More liberated social roles for women threatened traditional cultural authority. However, convincing women of their duty to have larger families required mobilizing women into the public sphere, a politics that risked increasing their desire for broader political and social participation. Unraveling this contradiction in its many forms is the central enterprise of this book. The book covers a broad range of women's experience, including girlhood, motherhood and maternity, family life, education, leisure, work, and political culture.

The fascist state sought, not always successfully, to intervene in all aspects of public and private life. Although the state had crushed overt political opposition by 1925, it was not the only influence over Italian society. The Catholic church played a critical role concerning issues of morality and appropriate female roles. Popular culture provided a competing vision of what it meant to be a "modern" woman. The state was not without means, particularly those that would force women back into a maternal role. It banned birth control information and contraceptives, taxed bachelors, limited women's work opportunities, and gave prizes for large families. It exerted all forms of moral suasion to convince women, as official propaganda pronounced, to become "the bearers of numerous children," "the mothers of soldiers," and "the procreators of the race" (p. 73).

This book is particularly attentive to the effects of class, urbanization, and generation on women's experiences. Some of the most intriguing material concerns the interaction between middle-class women acting as social service workers and those in more "humble" households whose lives were to be improved. De Grazia shows a fine eye for detail, particularly in elucidating the meaning of state rituals designed to bring women into public life. Especially noteworthy are the fusion of state and family demonstrated by Mussolini's request that women sacrifice their wedding rings for the Ethiopian war effort, and the way in which Mother's Day was celebrated on December 24 to coincide with the Roman Catholic cult of the Virgin Mary.

Though the most innovative materials are those that deal with this expanded Foucauldian concept of "rule," the sections on women as explicitly political protagonists are not slighted. There is a carefully nuanced history of the diverse wings of feminism, their repression or absorption by the regime, and the reemergence near the end of the regime of an "oppositional familism" that resisted fascism's entreaties to participate in its political project. The book concludes dramatically with a discussion of World War II and of women's return to direct political

activism in the movement of armed resistance to fascism. Militarization is treated as a logical conclusion of fascist pronatalist policies. As De Grazia writes: "Militarization joined together household and nation; it submerged the individual in the collective, it treated soldiering and mothering as complementary and equally meritorious operations of society. Fertility redeemed death; reproduction compensated for destruction" (p. 279).

My only critique concerns the absence of a more theoretically sustained conclusion. A summation of how social class shaped women's various experiences, or a weighing of the diverse impacts of church, fascist, and consumer culture on women's lives, or a more explicit comparison to France, Germany, and the United States might enable the reader to better grasp the totality of women's experiences. As it is, the chapters stand as intriguing, but relatively self-contained essays. More theoretically inclined readers will have to look to the (excellent) index and footnotes to develop their own syntheses. This minor difficulty should not be seen as diminishing a superb effort that demonstrates the value of a wide-ranging intellectual inquiry for elucidating women's roles and dilemmas, and that makes accessible the Italian experience to the English-speaking world.

Crafting Selves: Power, Gender, and Discourses of Identity in a Japanese Workplace. By Dorinne K. Kondo. Chicago: University of Chicago Press, 1990. Pp. xiii + 346. \$55.00 (cloth); \$16.95 (paper).

Crested Kimono: Power and Love in the Japanese Business Family. By Matthews Masayuki Hamabata. Ithaca, N.Y.: Cornell University Press, 1990. Pp. x + 191. \$17.95.

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During the late 1970s and early 1980s, two Asian-American graduate students from Harvard were in Tokyo, studying two very different kinds of Japanese family businesses. Dorinne Kondo, an anthropologist, located herself in a downtown blue-collar district, working side-by-side with artisans and part-time women workers in a small, family-owned confectionery. Meanwhile, Matthews Hamabata, a sociologist, was socializing with the wives and children of large-scale family enterprise owners in expensive restaurants and fashionable bars in neon-lit uptown Tokyo, lending an attentive ear to his high-society informants' stories of their families. Kondo and Hamabata "held hands," as Kondo puts it (p. x), overcoming the trials of fieldwork, sharing information and insights. The result of their extensive field experiences are the two fine ethnographies here reviewed. Kondo's *Crafting Selves* and Hamabata's *Crested Kimono* both provide us with vivid pictures of dimensions of contemporary

Japanese social life often obscured by popular images of Japan as a homogeneous middle-class society.

Despite their high visibility in the Japanese media, Hamabata's informants are all but invisible in the social science literature of contemporary Japan. He certainly made the right choice when he decided early in his fieldwork to bypass the executives (who are willing to offer nothing more than the standard spiel on Japanese business that Hamabata learned in graduate school) and to turn instead to their wives and children, who talk to him about the kind of intimate personal experiences—death, marriage, household succession, love—that significantly influence what goes on in the board meetings held in Tokyo's high-rise office buildings. He recounts the stories with an eloquence that can move the reader to laughter or tears. In these stories, we see his informants not as automatons serving the hegemonic family enterprise, but as individuals who struggle to achieve personal goals, driven by "vengeance, passion, anger, ambition, nurturing care, envy, all of the exalted and sullied aspects of human nature and human emotion" (p. 162). These feelings are, he argues, "given form by, and in turn shape," the culture and social structure of family. Among the members of elite families, what is personal is often political.

As his writing strategy, Hamabata chose to intermesh these stories with explications of cultural and social frameworks. Here, the author confirms, rather than reveals. He identifies the classic features of the Japanese family/household presented by his sociological precursors (mostly in the context of village studies), such as ancestor worship, hierarchical organizations, household successions, the importance of female networks, and shows them to be at work in today's elite metropolitan household.

Crested Kimono makes for delightful reading, but a word of caution is in order for the reader unfamiliar with Japan. First, Hamabata does not make clear the demographic and structural position of these elite families in the larger matrix of Japanese society, and his account is decidedly ahistorical. Second, he fails to account for the diversity of individual actions. Add to this his uncritical use of "the Japanese," and his ethnography imparts the illusory sense that what is described in *Crested Kimono* is "the Japanese way of life."

The social scientist's curiosity begins where Hamabata leaves off, for we find here many interesting sociological phenomena that are left unconceptualized in this ethnography. For example, Hamabata discusses the classic dichotomy of form and feeling as if form and feeling are in a harmonious, reciprocal relationship. In the individual stories he relates, however, we see people struggling with disharmony and conflicts between structural demands and personal feelings. The form/feeling duality embodies in itself ambiguity, the potential for rupture, the inevitability of change. I hope Hamabata will eventually present the theoretical aspects of his work with upper-class business families, since his material suggests many conceptual possibilities.

By contrast, Kondo's *Crafting Selves* is both richly contextualized and theoretically ambitious. Kondo's ethnography is informed by the post-structuralist recognition of the multiple and shifting nature of meaning and subjectivity and of the ways in which human agents and their identities are constructed within relations of power. Though she is rather prolix in her poststructuralist theorizing, her descriptive narration is nothing less than beautiful, capturing the sense of the crisp working-class Tokyo dialect and the vibrant atmosphere of workplace interactions. Her keen eye for complexity catches the minute details of human dramas to a degree unusual in even the best ethnographies.

The confectionery she describes is family-owned, with 30 full-time employees and seven or eight part-time workers. The owner, Mr. Sato, while trying to "impart the family flavor" (p. 177) through such activities as an annual company trip and a potato-digging outing, is nonetheless a shrewd business man who hires a management consultant to expand his business, and who maintains tight control over his employees by installing a surveillance camera. Vignettes and anecdotes from the shop floor that Kondo meticulously weaves into the text give an immediacy to situations in which workers contest management's manipulation and imposition of meanings in a way that allows them to maintain pride in their work. As Kondo points out, however, there are poignant ironies operating at every level of company life: for example, by evoking the familiar idiom of company as family and infusing it with a meaning that differs from management's, workers resist the power of management, but in so doing paradoxically legitimize and reproduce the idiom, and thereby their own positions in the factory.

Distinctions among workers are drawn along the line of gender, between male artisans and female part-time workers. Inequality takes the concrete form of differences in salary, benefits, and job security, and more subtly in everyday interpersonal relations. Her analysis of the construction of gendered work identity is particularly illuminating and can be applied to deepen our understanding of the conditions of gender inequality in which many Japanese women find themselves today.

Kondo states that the central project of her book is to attempt to "deessentialize" the Western notion of self as totalizing subject. For her, selves are "sites for the play of multiple discourses and shifting, multiple subject positions" (p. 44). Although she does not downplay the ability of humans to act upon the world, her inclination to give analytical primacy to discourse leads us to wonder if the hollow, wire mannequin pictured on the cover of her book is not in fact a visual metaphor for her vision of selfhood. This inclination overshadows the very human qualities of her informants so sensitively portrayed in her ethnographic narrative. These people come across as persons who are not only able to manipulate their identities and reflect upon their performance, but who also possess integrity and a distinctive sense of who they are. One source of confusion is Kondo's use of two discrete concepts, identity and self, almost interchangeably; surely a notion of self must include something beyond a

bundle of fragmented identities, such as time-depth (biography) and inner depth (personality). Those of us who study selfhood cross-culturally should share Kondo's insistence on deessentializing the Western notion of self. Indeed, her argument compels us to question many of our assumptions. Yet despite her sensitivity to the danger of Orientalizing Japanese subjects, her formulation of self is unwittingly caught in the pitfall of reinforcing the prevailing notion, often contrasted with the Western "self," that the Japanese person lacks a strong sense of self.

Such criticisms aside, however, both of these books represent valuable contributions, and I have found them to be useful in classes on Japanese family, work, gender, and class. Kondo's *Crafting Selves* can also be productively incorporated into a class on ethnographic writing.

Political Life in Japan: Democracy in a Reversible World. By Takako Kishima. Princeton, N.J.: Princeton University Press, 1991. Pp. xvi + 142. \$27.95.

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This study aims to illuminate the worldview behind the daily activities of Japanese politicians and to demonstrate that real power in postmodern politics inheres in the liminal margins of an existing order rather than in direct confrontation with that order. It is delightful and baffling in equal measure. Takako Kishima combines ethnography with phenomenology to address a key topic in political sociology—which may account for a book by a political scientist being reviewed by an anthropologist for a sociology journal.

The phenomenology is a formulation of power that derives from a three-dimensional worldview of "mutually contrasting principles." This owes much to Peter Berger's *The Social Reality of Religion* (London: Faber and Faber, 1969) and subsequent proposals by the Japanese sociologist Chizuko Ueno. The world is conceived as a dynamic tension between nomos, the normative and institutional order of society; cosmos, the symbolic order that gives nomos meaning; and chaos, the loss of meaningfulness and structure (pp. 20–22). The key to this dynamic is at the margins of nomos-cosmos and nomos-chaos, liminal domains where "the flow of ordinary time is halted and the utilitarian, norm-governed nomos is overpowered" (p. xii). Marginal beings and liminal states have ambivalent potential for destruction and creativity; thus, when they intrude into nomos they raise inconsistencies and challenge orthodox meaning. The order of nomos must be reformulated by incorporating elements of the liminal challenge. This process stirs people's reflexivity, and their questioning of nomos opens the possibility of genuine change. The reflexivity thus activated is "the core of power of any human being" (p. xiii).

This is what leads Kishima to ethnography: one must look at people's

daily behavior to see how and when they have such liminal experiences. The surprising turn of her study is to pursue this in a political culture with a reputation for secular rationalism and, moreover, to focus on its soberest characters—the conservative politicians at the top of Japan's ruling Liberal Democratic Party (LDP). In this, she relies on her own fieldwork as well as interviews; she was an observer-intern in the office of one of these LDP politicians.

The core of her book (chaps. 2–4) is a series of case studies of liminality. She shows, for example, the operations of the Finance Committee of the lower house of the Diet. Much of its contentious work is orchestrated by a canny committee chair, who uses field trips, group socializing, and other shared “liminal experiences” to forge legislative compromises between the LDP and opposition party members. Another case deals with a Diet member's headquarters during the legally limited 15-day national election campaign. For the entire two weeks, the Diet member, his family, staff, and supporters stage a festive, communal live-in at a Buddhist temple in his district, out of which they operate in mobilizing various support groups. Kishima borrows from Marshall Sahlins's work on Pacific rulership to liken the politician to the “stranger-king,” who is domesticated by the community so that his potentially dangerous powers are curbed and his regenerative force is turned to community benefit. The campaign is thus a ritual reenactment of this liminal homecoming, by which local voters can exert their influence over the politician.

In chapter 3, Kishima has an extended analysis of Prime Minister Nakasone's tearful report in late 1983 of a key private meeting he had just had to attempt to persuade ex-Prime Minister Tanaka to resign his Diet seat after he had been found guilty of taking bribes in connection with the Lockheed scandal. Nakasone and Tanaka return in chapter 4 along with an extended profile of one of the Diet's most colorful members, Ichirō Nakagawa. Nakagawa is famous for cultivating and flaunting an irreverent outsider character. In 1973, for example, he joined other brash young LDP members of the Diet to form a Young Storm Society with a widely publicized blood-oath ritual. Such politicians as these, Kishima argues, are not charismatic leaders, imposing alternative visions through compelling personal leadership. Rather, their actions more unwittingly help to expose the limitations and contradictions of the existing order and so spark people's reflexivity—their awareness of the possibilities of and their desire for alternatives. They are tricksters (pp. 77–78), unable to *transform* but able to *deform* an institutional order by questioning the taken-for-granted. To Kishima, such marginal figures and liminal moments, with their consciousness-raising capacity, are the only means to effective resistance in a postmodern society such as today's Japan (p. 59).

The argument seems on target and off-the-wall at the same time. It takes the ludic seriously, penetrating the formal facade of LDP decorum to reveal moments of genuine consequence ignored in other studies. For example, given one-party LDP dominance, Diet politics is surprisingly flexible, and there is more opposition party influence than one might

expect. Kishima's account of the Finance Committee's *modus operandi* helps explain this.

Yet the book's poststructuralist phenomenology of power remains an unconvincing assertion, not a demonstration. In light of four decades of entrenched LDP control (however accommodating of coopted opposition), deeply corrupt party financing, and repeated failures to enact electoral reforms, the power of tears, laughter, and temple live-ins still pales before that of faction bosses, corporate contributions, and right-wing thugs.

Rhetoric and Ritual in Colonial India: The Shaping of a Public Culture in Surat City, 1852–1928. By Douglas E. Haynes. Berkeley and Los Angeles: University of California Press, 1991. Pp. xi + 374. \$49.95.

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In his book, Douglas Haynes asks four defining questions: Why did India's elites, when they had viable alternative idioms (including Gandhian ones), appropriate Britain's liberal-democratic idioms? Why, at the same time, did communal (e.g., Hindu and Muslim) identities and conceptions of justice emerge when other identities and conceptions of justice had been, and continued to be, available? Why did the central arenas of political debate become confined to these two idioms (the British liberal-democratic and the communal Hindu-Muslim)? And how did the language of democracy, despite its seeming advocacy of the equality and rights of the underclasses, exclude the underclasses from full-fledged political participation?

In answer to these four questions, Haynes focuses on the city of Surat in western India between 1852 (when Surat was incorporated as a municipality) and 1928 (after Gandhi's noncooperation movement had expired). Haynes draws on two different sources: the American ethnohistorical school of Indian history that focuses on British efforts to comprehend and categorize Indian society and a Gramscian model of dominant-class (i.e., British) cultural hegemony "negotiated" with local elites.

In chapter 4, Haynes describes Surat's pre-British-raj social organizations, lifestyles, and idioms of authority related to Hindu, Jain, Muslim, and Parsi primarily endogamous descent groups. More important than these primarily endogamous descent groups in Surat, Haynes argues, were cultural and guild-like *mahajans* directed by eminent merchants (*sheths*) that generally crossed caste and sect lines and established and enforced business practices.

In chapters 5–11 Haynes traces how, during three-quarters of a century, a "public culture" (conventions and political discussion and debate revolving around originally British notions of public opinion and the public good) gradually took shape in the city of Surat. Rejecting the notion that the spread of liberal democracy is an inevitable result of

cultural contact with the "West." Haynes shows how initially Surat's "natural" and hereditary group leaders (as identified by the British) tried to establish stable social relations through demonstrations of personal loyalty and tribute to their British political overlords as they had with Mughal and other previous external political overlords. Initially these demonstrations fit remarkably well with British imperial rituals.

With the establishment of Surat's municipal council in 1852, Surat's "natural" leaders, whom the British appointed to the municipal council, won British approval by donating considerable sums of money to such public projects as schools and fire-, famine-, and flood-relief programs. Surat's limited municipal elections also generated a new class of English-speaking political leaders. These leaders, while fully accepting the hegemonic colonial perspective that there was a single path of social evolution with Britain far in the lead and India only entering that path, nevertheless introduced contentiousness into municipal council proceedings using British political idioms.

British policies during World War I generated inflation, bank collapse, mill failures, and an economic crisis for Surat, aggravated by Surat's need for higher municipal taxes. This economic crisis eliminated the merchants (*sheths*) as intermediaries with the British, leaving as intermediaries the English-educated politicians now advocating home rule within the British hegemonic perspective. Not until Gandhi launched his nationwide noncooperation campaigns were the residents of Surat provided a clear counterperspective, one steeped in Hindu religious metaphors which challenged the presumed superiority and desirability of Britain's colonial culture, including its parliamentary politics and its forms of education and economic development. Some of the key Gandhians who emerged in Surat were men of rural origins, many with little English education, speaking for formerly unrepresented sectors of Surat's population. Throughout the early 1920s these Gandhians and their followers defined and dominated politics in Surat. By 1928, however, the Gandhian noncooperation movement had collapsed, and the English-educated politicians had reemerged in Surat, now focusing their energies on Hindu-Muslim communal politics that eventually shaped the founding of Pakistan and continue in India today.

Despite Haynes's imprecise use of such terms as "public culture," "inner politics," and "outer politics," he presents well-argued answers to his four defining questions. He also provides extraordinarily rich details of the speeches, newspaper articles, and documents whose words and phrases defined the changing relationships between the politically active residents of Surat and their British colonizers during three-quarters of a century.

Thinking Through Cultures: Expeditions in Cultural Psychology. By Richard A. Shweder. Cambridge, Mass.: Harvard University Press, 1991. Pp. 404. \$37.50 (cloth); \$15.95 (paper).

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Richard Shweder, an anthropologist, argues for the importance of cultural psychology, an emerging interdisciplinary field devoted to the study of how "cultural traditions and social practices regulate, express, and transform the human psyche" (p. 73). While *Thinking Through Cultures*, a collection of Shweder's most important previously published essays, has a range of themes, sociologists will profit most from Shweder's presentation of cultural psychology's promise.

Comparisons between different societies have always advanced sociological understanding. That Weber, Marx, and Durkheim attempted such comparisons is one reason that we keep returning to these thinkers. The case Shweder makes for cultural psychology is persuasive because it is based on empirical comparisons of the thinking and reasoning of Americans in Chicago and Hindus in Bhubaneshwar, India.

Much of the book aims at identifying "ethnic divergences in mind, self, and emotion" (p. 73). One example is Shweder's description of the difference between how Americans and Indians understand the person. While the Oriyas of east India conceive of the person as concrete, context-specific, and situational, Americans tend to abstract a concept of the inviolate personality free of social role. While the Oriyas have a "socio-centric conception of the individual-social relationship" (p. 152), Americans have an egocentric conception that focuses on the individual.

By examining Oriyas with different levels of education and socioeconomic status, Shweder can conclude that the "concrete-relational style of Oriya social thought" is "unrelated to variations in cognitive skill, intellectual motivation, available information, and linguistic resources" (p. 129). Rather, Shweder argues, it is culture per se that affects Oriya ways of thinking. Shweder argues that every human being's mental life is "mediated by the system of meanings in which it is embedded" (p. 20).

Sociologists will be especially interested in Shweder's account of how the sociocentric conception of the individual develops through common interactions. Shweder uses persuasive examples which show that "people categorize the world the way they do" because of their participation in "social practices, institutions, and other forms of symbolic action," including talk "that presuppose or in some way make salient those categorizations" (p. 156). In doing so, Shweder clarifies such concepts as "practices" that are often slippery in sociological theory.

Cultural psychology ultimately promises to contribute to an understanding of how sociocentric and egocentric understandings of the individual-social relationship influence not just cognition, but social institu-

tions as well. Shweder presents cultural psychology as focusing on how "psyche and culture . . . live together, require each other, and dynamically, dialectically and jointly make each other up" (p. 73). If cultural psychology emphasizes how psyche and culture are inextricably intertwined with the social structures that shape the possibilities for human interaction, it has the promise of contributing to a better understanding of the link between micro and macro structures.

Shweder argues that because the prevalence of certain types of emotions and understandings of what constitute the person varies from place to place, "different psychological generalizations" may be "appropriate for the different semiotic regions of the world" (p. 20). Shweder persuasively shows, for instance, that the prevalence of certain metaphors of the individual-society relationship influences cognition and emotions. By focusing on how, according to Shweder, differences in emotions and ways of thinking are only tendencies, however, the reader may conclude that Shweder overstates the case against the psychic unity of humankind. While Americans "describe the people they know in abstract, context-free terms" 46% of the time, for instance, even Oriyas use such descriptions 20% of the time (p. 146). While a particular understanding of the person is more prevalent in the United States than in India, people everywhere have access to "second languages," which while less familiar, elaborated, and rich, are nonetheless accessible. (On "second languages" see Robert Bellah, Richard Madsen, William Sullivan, Ann Swidler, and Steven Tipton, *Habits of the Heart* [Berkeley and Los Angeles: University of California Press, 1985].)

While Shweder notes that sociological work on the social construction of realities has contributed to the development of cultural psychology (p. 363), he does not devote much attention to relevant work by sociologists such as Berger, Cicourel, Garfinkel, Goffman, Hewitt, and Zerubavel. While some of the anthropological debates in which Shweder is engaged will be of little interest to many sociologists, for sociologists interested in cognitive sociology, culture, symbolic interactionism, phenomenology, and ethnomethodology, this is not a book to be missed. *Thinking Through Cultures* charts a new discipline exploring the interconnection between culture and psyche and is a reminder of the fruitfulness of comparative work.

Science as Practice and Culture. Edited by Andrew Pickering. Chicago: University of Chicago Press, 1992. Pp. 474. \$65.00.

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Andrew Pickering has brought together in this volume the latest work on the sociology of scientific knowledge. This collection highlights two major perspectives on studies of science: science as knowledge and sci-

ence as practice. The book is divided into two sections—part 1, “Positions,” and part 2, “Arguments”—and has an excellent introduction by the editor. Pickering clearly lays out the historical development of the sociology of scientific knowledge, both the American Mertonian tradition and the newer approaches influenced by British and Continental thinkers. His summary provides an excellent backdrop for the arguments of part 2 which is made up of contributions from participants in the major debates in this field. Michael Lynch lays out a fundamental critique of the Mertonian approach as well as showing linkages between Bloor’s interpretation of Wittgenstein and the ethnomethodological, constructionist approach. Various authors, including Bloor himself, as well as Bruno Latour and Michael Callon, respond to critiques of their work. This inter- and intradisciplinary dialogue demonstrates how scientific knowledge is constructed, maintained, and becomes part of the culture. These theoretical essays are the foundations for the empirical research presented in part 1. I might have reversed the order, but the book works well either way.

Part 1 consists of case studies of researchers studying other researchers. These in-depth analyses of laboratories, experiments, scientific discoveries, past and present, uncover the taken-for-granted, everyday life and culture of science. Just to touch on a few points made in most of the essays, rather than the usual myth of the solitary scientist, the essayists present a picture of collaboration. Experiments and discoveries as well as language and disciplinary boundaries are the result of collective practices that require the “coordination and management of work across multiple and divergent actors, social worlds, meanings, and uses in producing science” (p. 169).

According to Karin Knorr Cetina, laboratories are not isolated environments but reflect the varied aspects of the field as well as remaining linked with the external world. She studies the laboratory worlds of social science and natural science to bring “to the fore the full spectrum of activities involved in the production of knowledge” (p. 115). Each discipline shapes the form and content of the laboratory. Ultimately, according to Knorr Cetina, experimental reality is “a *technology of representation*” (p. 124). Moving to a postmodernist stand, she carefully shows through research on molecular genetics how “experimentation deploys and implements a technology of intervention” (p. 126). In this field, there is no assumption that anything corresponds to “natural events” or some abstract notion of “reality.” But the actual processes are often transformed into positivist language that hides or mystifies research as it is actually practiced. Knorr Cetina calls this a “reconfiguration” which she deconstructs through the use of semiotics as well as by careful observation of laboratory research in the traditional mode.

Using cancer research as an example, Joan Fujimura illustrates how laboratory research (the workplace) interfaces with the multiple social worlds. To clarify how the interface works, she makes use of the concept of “standardized packages, which facilitates both collective work by

members of different social worlds *and* fact stabilization" (p. 176). A semiotic perspective is also needed here to understand the transformations of disparate disciplinary modes into a common understanding of, in this case, oncogene theory. The researchers install "their theories, inscriptions and materials into . . . ongoing lines of research" that have already been set up and stabilized and then "these collective constructions are packaged together" as representations "to enroll other researchers, biological supply companies, the National Cancer Institute, the American Cancer Society, members of Congress, and the Nobel Prize Committee" (pp. 203–4).

Pickering and Adam Stephanides make a foray into the history of mathematics to "do a careful anthropological study" of the process of modeling to demonstrate the dialectical relationship between resistance and accommodation that results in the acceptance of one way of doing algebra and geometry over another. Foregrounding choice, chance, and contingency in the relationship between two different approaches to mathematics, the authors illustrate the culturally situated aspect of even something thought to be so abstract and beyond historicity as mathematics (p. 164).

One minor disappointment is the lack of acknowledgment of the feminist research in this area which has been going on for some time. Those familiar with this rich body of literature may find some of the theoretical material to be somewhat commonplace. I would recommend this book not only for those interested in and teaching courses in the sociology of scientific knowledge but even for those teaching courses in the broader area of the sociology of knowledge. I also think that this book could be a valuable tool for teaching research methods. The essays in part 1 are exemplary in their formulations of research aims and methods.

Questions about Questions: Inquiries into the Cognitive Bases of Surveys. Edited by Judith M. Tanur. New York: Russell Sage, 1992. Pp. 306. \$34.95.

Stanley Presser
University of Maryland

From one point of view, the survey is simply a tool used to make discoveries about the social world. In order to make these discoveries, however, a survey creates its own world. As Matilda White Riley put it, "an interview, and to a lesser extent even the self-administration of a questionnaire, always establishes a new and temporary social system" ("Sources and Types of Sociological Data," in *Handbook of Modern Sociology*, edited by Robert Faris [Chicago: Rand McNally, 1964], p. 1005).

Methodologists have long been aware that features of this temporary social system shape the data produced by surveys. It is an article of faith

among survey methodologists that improvements in both data production and interpretation can come from understanding the interactions that take place in the survey.

Since language, memory, cognition, and persuasion play central roles in survey interaction, it might seem only natural that methodological research would borrow heavily from the cognitive sciences that study these matters. In fact, however, most of the work done by survey methodologists is uninformed by theoretical insights from those who study topics such as understanding language, recalling the past, or complying with influence attempts.

In the past decade, this has begun to change. Aided by Murray Aborn's methodology program at the National Science Foundation, a movement has developed to apply cognitive science to solve survey measurement problems. One of the movement's leading proponents is Judith Tanur. This book, an outgrowth of the Social Science Research Council's Committee on Cognition and Survey Research, is divided into five main sections: meaning, memory, expression, social interaction, and government applications.

In the meaning section, Herbert Clark and Michael Schober review linguistic principles that organize conversations (e.g., accumulation and grounding) and show how these ideas can be used to interpret survey question wording and context effects. Despite the fact that "everyday conversation is unmediated, extemporaneous, and interactive [whereas] survey interviews are mostly mediated, predetermined, and noninteractive" (p. 26), it is interesting to note how many of these "method" effects would be expected to occur in everyday conversation.

In the other paper on meaning, Robert Groves, Nancy Fultz, and Elizabeth Martin deal with a key survey dilemma: the same words may be interpreted differently by different people. They show how responses to follow-up questions that probe what a respondent had in mind when answering a question can illuminate answers to the item.

The book's longest section is on memory. Robert Pearson, Michael Ross, and Robyn Dawes review the evidence for the thesis that "memory is often a construction in which images of the past and present are combined with inferences drawn from implicit theories about self and society" (p. 67). The results of experiments reported in chapters by Elizabeth Loftus et al. and Robert Abelson et al. demonstrate the difficulties of overcoming the forces leading to misreporting about the past. A number of manipulations based on cognitive theories did not produce the expected improvements in accuracy, and the one manipulation that did work (though in only one of two cases) stemmed not from cognitive theory but from the intuition of an experienced survey researcher.

The section on expression contains two chapters on attitude measurement, one by Jon Krosnick and Robert Abelson on the indispensability of strength measures, and the other by John Dovidio and Russell Fazio on indirect approaches designed to overcome socially desirable forms of responding. Both chapters draw on the findings of an extensive set of

laboratory experiments in making recommendations for survey procedures. These will prove challenging for survey researchers, as they pose major implementation difficulties.

The sole chapter in the social interaction section is a reprint from the *Journal of the American Statistical Association* of Lucy Suchman and Brigitte Jordan's "Interactional Troubles in Face-to-Face Survey Interviews." Based on an analysis of interviewer-respondent interactions, it argues that the standardized scripting of the interview violates conversational norms and impairs data quality. Although the paper deals with an important tension in the survey interview, it is not clear from the authors' examples that the answers that emerge from "interactional troubles" are typically in error.

Cathryn Dippo and Janet Norwood's "Review of Research at the Bureau of Labor Statistics," which composes the final section, makes clear how vital many of the problems reviewed in the preceding chapters are to the production of economic indicators in the United States. It also shows that the federal statistical establishment has been in the forefront of efforts to apply cognitive science to survey measurement.

Taken together, these chapters provide a good sense of the range of survey problems investigated by the cognitive movement, the methods and ideas it draws upon, and the results it has yielded. What then of the contribution of the movement? It would be hard to improve on the answer given by Tanur in her introduction: "We have not yet . . . been able to use cognitive theories . . . to shape survey innovations or to predict the results of using innovative variations on established procedures. . . . The movement has so far raised more questions than it has answered. Several of the empirical findings remain contradictory. But in these contradictions lie the seeds for a better understanding of cognitive processes and improved means of asking questions in surveys" (p. 11).

The Professional Quest for Truth: A Social Theory of Science and Knowledge. By Stephan Fuchs. Albany: State University of New York Press, 1992. Pp. xviii + 254. \$54.50 (cloth); \$18.95 (paper).

Andrew Pickering
University of Illinois

Arguments that science is "socially constructed" have vexed the disciplines for the past couple of decades. Within sociology, one standard response has been straight denial, and thus a reaffirmation of traditional Mertonian sociology of science. Another response, which seems to be gaining ground, is to accept the claim but then to subsume it within traditional sociology. This is Stephan Fuchs's strategy. He concedes the "sociology of scientific knowledge" (SSK) thesis of social construction, but faults SSK for having no "theory." What he means by this is that SSK is not interested in understanding *variations* across disciplines,

fields, or specialties in their modes of social construction. This obliges him to be somewhat disingenuous in his discussions of David Bloor, one of the founders of SSK, who has traveled just this path, developing Mary Douglas's anthropological "grid-group" theory as he goes. But Fuchs wants to develop a rather different theoretical framework centered more securely within mainstream sociology.

Fuchs's guiding idea is to put organizational theory to work in exploring differences in the "cognitive styles" of different fields, both within science and without. Here he follows in the footsteps of Richard Whitley's *Intellectual and Social Organization of the Sciences* (Oxford: Clarendon Press, 1984), but while Whitley was there concerned with relatively fine patterns in social structure and knowledge production Fuchs is after a different fish. His primary aim, I think, is to displace major epistemological debates from the terrain of philosophy onto that of sociology. The persistent theme of this book is that realist/objective and relativist/hermeneutic epistemologies should be understood as products of distinct kinds of social structure. "Strong fields," such as physics—well ordered and integrated and centrally disciplined organizationally—understand themselves as producing objective knowledge about an accessible real world. In other words, they produce "facts." "Weak" fields, such as sociology—Fuchs's concluding and sole substantive chapter is devoted to precisely this point—are self-consciously relativistic and hermeneutic, and produce "conversation" (no bad thing, perhaps, according to the book's last sentence).

Since issues of objectivism and relativism are not, according to Fuchs, just "pseudoproblems" of SSK (p. 101), but are key foci of contemporary academic-political discourse, this is a very strong and potentially important claim. But, from the perspective of anyone who has tried seriously to engage with these issues, it is also a pretty dull and dubious one. As do all such sociological reductions, it has the effect of trivializing fascinating topics. "Oh, so you're a relativist. . . . From a weak discipline are you?" Well maybe, but what about my *arguments*?

I can think of several ways of destabilizing the overt message of this book. One would be simply to challenge Fuchs's characterization of the technical substance and social structure of such fields as physics and sociology (and literature, for that matter). The former is not nearly as monolithic as he supposes; I do not think that sociology as a discipline is well described as "conversational." Fuchs sometimes seems to be making up a world to exemplify his theory. Another way would be to point to problems Fuchs has with causation. Much of his review of organizational theory has a technological determinist slant: social organization takes the form that best accommodates the specific properties of the technology it envelops. This line of thought tends to the conclusion that the social structures of different fields follow from their "cognitive styles" rather than the reverse. Toward the end of the book, as a dutiful Durkheimian, Fuchs seems to recognize this problem, and seeks to recoup what appear to be technological variables as social ones (chap. 7). The recuperation

is not very convincing in itself, however, and is made less so when the notion of *choosing* social organizations and cognitive styles starts to surface (p. 207).

As a potential line of flight from this flip-flopping of causal arrows it is interesting to think about a subsidiary accounting strategy to be found here and there in *The Professional Quest for Truth*. At various points Fuchs slips, incongruously but apparently deliberately, into the vocabulary of the "actor-network" theory of science, technology, and society developed by Michel Callon, Bruno Latour, and John Law. When Fuchs writes that "natural order coemerges with social order" (p. 75), for example, he adopts a characteristically noncausal and irreductive formulation from the actor-network. Fuchs never comes to grips with the differences between the actor-network approach and SSK, and when he tries to he disavows what is truly interesting and original about the former, its "extended symmetry" which treats human and nonhuman actors (and all combinations thereof) as analytically on a par (chap. 1, n. 13). It seems to me, though, that something like the actor-network approach, rather than SSK, is what Fuchs needs if he is to avoid representing serious debates as sociological epiphenomena.

Milestones and Millstones: Social Science at the National Science Foundation, 1945–1991. By Otto N. Larsen. New Brunswick, N.J.: Transaction Publishers, 1992. Pp. xvi + 285. \$32.95.

Stephen Cole
SUNY at Stony Brook

Otto N. Larsen, who served in an administrative capacity at the National Science Foundation (NSF) in the 1980s, provides a detailed historical account of the funding of the social sciences by their most important federal patron. The book concentrates on the debates over whether and how the NSF should fund social science, how much funding the various social science disciplines received, and attitudes of politicians toward the funding of social science research by the NSF. Larsen reports data on four variables, all measured by budgetary figures. "Perceived scientific need" is defined as the amount of money requested for a particular discipline in the president's budget. "Actual political power" is determined by comparing the ratio of money appropriated by Congress to money requested by the president; the higher this ratio the more political power. "Prospective societal impact" is the total amount of money actually spent on a discipline. Finally, "comparative scientific status" is the proportion of total NSF funds allocated to a social science discipline or disciplines.

The book, organized by decades, provides much useful descriptive information on changes in the fate of the social sciences at the NSF over the 46-year period studied. As a historical description of the politics within the NSF and in the federal government as they affected the social

sciences at the NSF this book can be strongly recommended. As a piece of sociology it has three primary problems. First, the book is entirely descriptive with little or no analysis of the many sociological problems raised by the interesting data presented. For example, Larsen points out that funding for the social sciences peaked in the 1960s and declined in the 1970s and 1980s. There is no detailed or systematic analysis of why this occurred. We are told that by 1979, prior to the election of Reagan, the political climate had changed, but we are not given a clear idea of how and why this influenced the funding of the social sciences.

Second, Larsen tends to place most of the blame for the relatively low status of the social sciences on bias and ignorance rather than on real shortcomings in these disciplines. As is evident from Larsen's data the social sciences have had very low comparative scientific status. Through 1989 less than 3% of all NSF expenditures were allocated to the social sciences. In the 1960s this proportion approached 6%, but in the 1980s fell to about 2%. As this book makes abundantly clear the social sciences are held in quite low repute by large numbers of natural scientists and politicians, and by the public at large. Today, unfortunately, this is particularly true of sociology. Some members of our profession see our low status as the unjust result of ignorance as well as politically motivated bias. Others among us see our low status as at least in significant part a legitimate reaction to our own failings. Larsen occasionally gives voice to those expressing the latter view. For example, the eminent sociologist Herbert Costner, who served as head of the Division of Social Sciences from 1976 to 1979, in analyzing the difficulty faced by social sciences at the NSF "placed a heavy burden on social science itself" (p. 119). Costner, writing to Larsen, said, "I am discouraged and disgruntled at the state of social science research" (p. 127). But, in general, Larsen tends to assume that the reason for our relatively low prestige is ignorance and political opposition. For example in the preface he urges social scientists to take natural scientists to lunch and talk about their work, assuming that this will increase support among natural scientists for the social sciences. Many of us who have worked closely with natural scientists, however, have had the experience that greater exposure does not invariably increase understanding or sympathy. Larsen pays little attention to the many eminent social scientists, including such past presidents of the American Sociological Association as Lewis Coser and Stanley Lieberman, who have been strongly critical of the way in which at least one social science is practiced. Putting the blame for reduced funding by the NSF (or the closing or shrinking of many sociology departments) on bias and ignorance is likely to deflect attention from how we might improve our discipline to make it more worthy of public support.

Third, Larsen has ignored many of the most interesting questions that could be raised about the social sciences at the NSF. For example, there is no systematic analysis of what type of proposals were submitted and what type received funding. How, if at all, did the NSF shape the development of knowledge in the social sciences through its funding decisions?

There is virtually no discussion of the output from NSF grants. What is the relationship between the availability of funding and production of new knowledge in disciplines such as sociology? There is no analysis of the relationship between the amount of money available and "proposal pressure." Is level of funding for the social sciences in any way influenced by the number of proposals submitted? When funding levels dropped, did the number of proposals fall off? And if so, what type of sociologists self-selected out of the competition for funding? Finally, and perhaps most striking, there is virtually no discussion of the NSF peer review system, a topic that has stirred considerable interest among both the disciplines and the Congress. Is the NSF peer review system universalistic? Does it discourage innovative research? Any analysis of the social sciences at the NSF remains seriously incomplete without a consideration of these topics.

Organizational Communication. By Peter K. Manning. New York: Aldine de Gruyter, 1992. Pp. xi + 249. \$39.95 (cloth); \$19.95 (paper).

Peter M. Hall and Ron Busselman
University of Missouri

Peter Manning has conducted imaginative, conceptually diverse, and consequential studies of dramaturgy and communication in organizations during his productive career. Now he has written an advanced textbook on organizational communication from an interpretive perspective which amalgamates and elaborates the concepts of that program and also provides a paradigm for field research. But this volume is much more than a text. The author uses the occasion to move the analytical agenda into the postmodern period and examine its implications for organizations and social relations. He also presents aspects of a postmodern ethnography congruent with that theorized context.

Manning employs dramaturgy, semiotics, loose coupling, and discourse as elements of his perspective to focus on the relationship between ubiquitous ambiguous information and equivocal communication in formal organizations, internal and external contexts, and rules and practices used to reduce uncertainty. Research exemplars are reviewed to illuminate multiple interpretive strategies for studying organizational communication including semiotics, narrative analysis, textual exegesis, and discourse analysis. The author then turns to his own research on police organizations and nuclear regulatory agencies to give more substance to his perspective.

The strength of the book is its excellent demonstration of the vitality, sophistication, and power of the interpretive paradigm. Manning skillfully reveals the benefits of using different approaches (dramaturgy and semiotics). He gives unusual clarity to the concept of loose coupling and uses it with subtlety. Attention to language and discourse significantly

extends symbolic interaction and helps detect relationship between structure and process. The author continuously illustrates the indispensability of context and the imperative for comparative analysis. More specifically, Manning discusses how information technologies and the distribution of information in organizations affect uncertainty. He illustrates how cultural practices provide the means for a working consensus but also sustain paradox. Analysis of rhetorical strategies reveals the achievement of short-lived resolutions that reinforce organizational credibility but also retain continuing dilemmas. Manning is quite sensitive to the double edge of symbolic management that, like political impression management, is easily used but eventually becomes a sign of weakness. Overall, Manning is convincing in showing the reader why an interpretive maxim, "You had to be there; it could have been otherwise," is prophetic.

The postmodern adventure is distinctly less satisfying. While explicitly constructing ideal types of postmodern society and postmodernism, and calling for empirical analysis and writing with some qualifiers, Manning, nevertheless, appears to accept the perspective as manifest. For example, "the knowledge society" as a countertrend is discounted and dismissed, leaving us with only postmodernity. Thus, history loses its relevance, "reality" dances with hyperreality, the self vanishes, uncertainty and deception increase, and media is the driving societal force. It can easily be argued that this vision manufactures discontinuities and constitutes hyperboles. This is reminiscent of the mass society models of the 1950s which were later empirically discredited. The postmodern emphasis overlooks the utility of paradigms that explain contradiction and relations between power and truth. Linking postmodernism to symbolic interaction downplays fundamental differences and current writings on their problematics. Nowhere does Manning evaluate the perspective or its capacity to fulfill a critical role.

Manning, to his credit, suggests that organizational field research is required to study ambiguity, deception, and signwork, local knowledge and lived experience, and sociopolitical production of information and credibility. He also wants an examination of the mass media's role in creating value and deferring reflection. He is correct that research is needed. Too much of postmodernism is armchair assertions, grand theorizing, and texts on texts. The central contention about mass mediated communication and its effects demands attention. Do people in all their diversity see the same movies and give the same monolithic readings that sociological deconstructors do? Past research would tell us no. Are people as unaware and unreflective or as constituted by language as some theorists would suggest? The interpretive paradigm would demonstrate otherwise.

According to Manning, postmodern ethnography should be a reflective and reflexive text which includes its constitutive processes and sensemaking of self and others. It should be open to representational variety, inclusive of voice, responsive to indeterminacy and unpredictability, and substantially contextual. Contemporary interpretive ethnographers do

not have to agree with either a correspondence theory of truth or post-modern deconstruction and confessions to carry out those suggestions. Most are in keeping with current theory and practice. Indeed, the author's proposed well-founded research agenda can be undertaken with the interpretive tools at hand. Moreover, we can still utilize pragmatic theoretical and empirical practices in truth seeking. In that effort, Peter Manning has made a valuable contribution to organizational analysis, the interpretive paradigm, and the necessary ongoing critique of our discipline and our scholarly activity.

The New Institutionalism in Organizational Analysis. Edited by Walter W. Powell and Paul J. DiMaggio. Chicago: University of Chicago Press, 1991. Pp. vii + 478. \$65.00 (cloth); \$24.95 (paper).

David H. Kamens
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Culture is back in organizational analysis, and this volume reflects the variety of ways that institutional theorists are reconstructing—and using—the idea of culture. Many of this book's segments were presented as papers at a conference funded by the ASA's Problems of the Discipline program. However, Walter W. Powell and Paul J. DiMaggio wisely decided to expand the agenda of the book, beyond the conference papers, to one that would introduce a wider audience to the "new institutionalism" in the field of organizations. Accordingly, the authors augment the series of papers written especially for this volume with reprints of some of the classics of the field. Overall this volume offers a comprehensive introduction to institutional lines of argument.

The book is divided into an introduction and three main sections. The introduction by Powell and DiMaggio masterfully contrasts the old institutionalism of the 1950s, associated with the work of Selznick and others, with the new institutionalism of the 1980s (table 1.1 [p. 13] is an extremely helpful summary of the contrasts). The first section, entitled "The Initial Formulations," reprints four well-known papers written in the late 1970s and early 1980s. It includes the ground-breaking 1977 Meyer and Rowan paper on "Institutionalized Organizations," the authors' paper on the "Iron Cage Revisited," a paper by Zucker on "The Role of Institutionalization in Cultural Persistence," and a piece by Scott and Meyer on "The Organization of Societal Sectors." These papers were responsible for initiating the new institutionalism as an intellectual movement. The rest of the papers in this volume will help bring readers up to speed on where the field is heading.

The second section, mislabeled "Refining Institutional Theory," consists of five papers that actually explore the diversity of views among practitioners of the new institutionalism. Jepperson's lead essay attempts to rigorously define the domain covered by institutions and institutional

effects. Scott's paper examines the current variety of institutionalisms. Powell's essay helps explode some of the myths about the limitations of the new institutionalism, such as the charge that it cannot deal with change or technical efficiency considerations. Jepperson and Meyer present a rich, theorized account of polity types and the societal effects of these types on the amount and form of formal organizing. This essay is central for understanding the new institutionalism and its capacity to reintroduce society and history into the analysis of organizations. Last, Friedland and Alford attack rational choice and other theories that presume primordial social units (e.g., individuals, the state) with exogenous preferences or interests. They develop an alternative that focuses analytic attention on the social construction of interests and the contradictions that different institutional logics generate (e.g., the logic of family as exchange value vs. the logic of family and human needs, p. 257).

The third section presents six papers that investigate change in diverse populations of organizations in various settings. The organizational populations include art museums, nonprofit organizations, community colleges, and industrial firms. Most are situated in the United States, though at different historical periods, but two papers transcend this geographical boundary and report on voluntary, nonprofit organizations in Canada and business firms in Japan, Taiwan, and South Korea (chaps. 15, 16). These papers are useful both for their specific findings and as illustrations of the lines of research that the new institutionalism has generated.

Taken together, these papers develop a thorough exposition of the new institutionalism. First, as the empirical papers in the third section make clear, the analytical focus is on populations of organizations, much as in population ecology. The difference is that analytic attention is on the vertical links between these populations and key agencies of authority, often but not exclusively nation-states. Singh et al. (chap. 16), for instance, find that Canadian voluntary associations gained authority over "youth" as a result of a change in governmental priorities. They show that this vertical linkage altered the population dynamics of organizational foundings and death rates.

Second, the new institutional analysts view organizations as agents, who have been delegated public functions. Resources and authority are delegated to organizations but sovereignty over particular domains lies outside organizations, in all sorts of occupational groups and professions created and authorized by modern educational systems and in the nation-state itself. This notion has important implications for organizational change. Fligstein (chap. 13), for example, shows that change in the occupational backgrounds of CEOs is associated with the acceptance and diffusion of new organizational structures. The point he makes is that each occupational group has a different image of organizations. When its members gain power, they enact substantive models of organization. Even "iron cages" evolve.

Third, as a result, environments penetrate organizations and infuse organizational roles with meaning and cognitive content. This often re-

sults in a good deal of ambiguity regarding organizational purposes and activities. Brint and Karabel's (chap. 13) study of junior colleges illustrates this point. Although the authors do not choose to emphasize it, the history of junior colleges is one marked by a continuing search for a viable mission and a niche in the structure of higher education. While the authors view this process as one of goal displacement, it is more plausible to think of junior colleges and their administrators as making continuous efforts to reduce ambiguity through a variety of strategies, once their initial hope of becoming the bottom rung of higher education was dashed by historical developments following World War I.

Fourth, the new institutionalism focuses on mechanisms that produce homogeneity or diversity across populations of organizations and isomorphism with environments. DiMaggio and Powell's well-known essay and Meyer and Rowan's path-breaking paper carefully delineate processes that generate isomorphism. DiMaggio's paper (chap. 11) on American art museums shows the role of professionals in developing new organizational models for both museums and supporting organizations (see also Galaskiewicz's paper on corporate-nonprofit relations, chap. 12). Orru et al. (chap. 15) describe the interplay between firms, families, and states that produces isomorphism in firm structure within each society (Taiwan, Japan, and South Korea) along with substantial national variation between these populations of firms.

In sum, this book represents a significant addition to the literature on organizations. Not only does it make accessible the key papers delineating the new institutionalism, it also prefaces these pieces with an introduction that is a conceptual tour de force. The volume will be useful in advanced classes on organizations and to anyone interested in understanding modern organizations. It will also be an important reference book for administrators and professionals in a wide variety of fields who will need to become acquainted with these new lines of thought.

Human Services as Complex Organizations. Edited by Yeheskel Hasenfeld. Newbury Park, Calif.: Sage Publications, 1992. Pp. xi + 400.

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The state of organizational theory and research with respect to human services has greatly changed since publication of Yeheskel Hasenfeld's *Human Services Organizations* in 1974. His new volume updates the literature on environment, technology, leadership, job satisfaction, working conditions, professionalism, and effectiveness and covers new issues or approaches including empowerment, gender, nonprofit organizations, and human services as moral work.

Hasenfeld's early chapter on theoretical approaches summarizes new directions in population ecology, political economy, and institutional the-

ory as well as recent critiques of the rational-legal and human relations schools. Handler's chapter on dependency and discretion contains an excellent review of power. Glisson presents an insightful critique of the technology and structure literature. In her review of empowerment, Gutiérrez refers to the growing work in community psychology by, among others, Rappaport, Zimmerman, and Prestsby. McNeely covers interesting recent developments in job satisfaction, and Schmidt tries to categorize prevailing approaches to studying executive leadership.

In contrast to Hasenfeld's earlier volume which contained the essential literature available in 1974, *Human Services as Complex Organizations* includes recently published pieces as well as several specifically written for this volume. The result is a somewhat uneven overall product, as one reads journal articles, condensations by authors from larger bodies of their theoretical or empirical work, case studies from a dissertation, and an abridged chapter from an award-winning book.

But this does not mean the volume is boring in style or content. Hasenfeld is interested in exploring the incongruities inherent in human services organizations, and he has assembled a variety of perspectives. The reader can discern and participate in a potential dialogue as chapters offer different definitions of legitimacy, explore the empowerment of clients versus staff versus organizations, examine the role and status of women as clients, workers, or administrators, indirectly debate the nature and application of politico-economic approaches to nonprofit and government agencies, and ask whether variables such as technology and structure are independent or dependent.

The chapters on women are particularly interesting. Gottlieb discusses feminist practice technologies, empowerment, and bridging the gap between individual and societal sources of and solutions to problems. Dressler's historical data on the division of social work labor based on gender is insightful, although the explanation could be broadened to include the religious and capitalist as well as patriarchal roots of social work. Hyde's descriptions of feminist health centers are fascinating, but I do not think they fully fit her hypotheses or conclusions.

Several of the chapters involved applied research. Gronbjerg's on funding strategies and adaptation of nonprofit human-service organizations compresses many findings in a growing research area and may encourage readers to further pursue this topic. D'Aunno's ideas on multiple constituency and institutional and goal approaches used to compare effectiveness were interesting, but his data were not altogether convincing. On the other hand, Grusky and Tierney provided some much needed data gathered from different stakeholders—agency directors, case managers, clients, and families—on evaluating the effectiveness of eight county mental health systems, although the presentation was a bit condensed.

Hasenfeld is in the School of Social Welfare at UCLA and several chapters are heavily oriented toward social work. Not unexpectedly, sociologists who have published on social welfare organizations and professions such as DiMaggio and Powell, Aiken and Hage, Freeman and

Hannan, Pfeffer and Salancik, and Perrow were often cited while others who have written more broadly on organizations and professions, such as Blau, Friedson, French and Raven, and Herzberg were, for me, conspicuously absent.

In particular, I believe that disciplinary differences account for my problems with the definition and use of the terms legitimacy and institutionalization in Tucker, Baum, and Singh's chapter on institutional ecology, and with the definitions of profession and diagnosis in Kirk and Kutchins's chapter, although they had the misfortune to be placed after the excerpt from Abbott's *The System of Professions*.

The volume has an index, often missing in such readings, which enables the reader to find cross-references to terms, methods, and major references. In fact, I read this in a hopscotch fashion, following topic lines dictated by my own research interests, and found the variety of perspectives and mix of theory, literature review, and research stimulating and helpful.

Choosing Crime: The Criminal Calculus of Property Offenders. By Kenneth D. Tunnell. Chicago: Nelson-Hall, 1992. Pp. x + 191. \$18.95.

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How people make decisions to commit criminal acts is a growing area of research and theory. This area of criminology is usually labeled "rational choice." Unfortunately, the word "rational" has multiple meanings, often implying careful thinking and sensible decisions by somebody's standards. Yet the new rational choice criminologists do not mean that and say so. For example, Derek Cornish and Ronald V. Clarke (*The Reasoning Criminal: Rational Choice Perspectives on Offending* [New York: Springer-Verlag, 1986]) treat offenders as goal-oriented, but note that their goals are not necessarily general ones. Thus, a 14-year-old boy may engage in vandalism because he enjoys the sound of smashing glass, which seems "irrational" to the middle-aged store owner whose glass he smashes. Offenders may think but not necessarily with great care, "satisficing" (see Herbert A. Simon, *Administrative Behavior* [New York: Free Press, 1976]) rather than "maximizing" in the older utilitarian sense. Moreover, the old-time utilitarian implication that punishment is the key to crime prevention is not embraced by new rational choice criminologists, who are strongly inclined to "situational prevention" or "designing out crime." Although Cornish and Clarke are well aware that the word "rational" does not do the job, they and many other scholars have been struggling for a better one. I prefer the term "illegal decision analysis," which leaves the hows and wherefores as empirical questions.

Kenneth D. Tunnell's book does just that. It is based on interviews

with 60 offenders in Tennessee's prisons and county jails in 1986 and 1987. It asks them what they were thinking when deciding whether to take illegal action. Their answers are reported verbatim and at length. This author describes trees better than the forest, but that does not invalidate the book. For example, the last chapter—"What Shall Be Done?"—includes many suggestions that seem to be based more on ideology and faith than on the findings of this study. On the other hand, the critique of U.S. toss-them-in-jail policies is consistent with the reports by offenders that they do not think about punishment while they are making criminal decisions and that they know they have very little risk of being caught and punished. The author correctly sees the futility of enhancing punishment in a modern society with extensive crime opportunity. Such reports by offenders and conclusions by criminologists are not new and therefore not a major contribution (although replication is not to be sneezed at).

The author does add something important to the literature: he shows that offenders see themselves as making their illegal choices in very informal ways, based on intuition more often than cogitation, on casual and brief discussions rather than extensive ones. Several quotations are given to support this conclusion. They are consistent with other recent work yet are phrased in a different way and hence add to our knowledge.

In generalizing from this perfectly good finding, the author—as do so many others—falls into the semantic bog of "rational choice," which he rejects because offenders are seldom thinking about punishment. Yet he quotes his own offenders as avoiding illegal action when police or nosy neighbors are watching. These quotations are completely consistent with what the *new* rational choice criminologists are talking about. This author refers to "situational rationality" and notes that offenders "still may have been acting in a logical fashion, even though they did not consciously think of alternatives to their specific crime" (p. 71). This is the essence of new rational choice theory.

This research in 1986–87 preceded the publication of important ethnographic work by Paul F. Cromwell, N. J. Olson, and D. W. Avary, *Breaking and Entering: An Ethnographic Analysis of Burglary* (Newbury Park, Calif.: Sage, 1991), who determined that incarcerated burglars tend to portray themselves as more cogitating and less intuitive than Tunnell's sample. Indeed, Cromwell et al. felt that vanity was pushing burglars to give too many reasons and to paint themselves as smarter than they really were. Therefore, they tried another research method: finding a sample of unincarcerated burglars. They drove around with them one at a time, asking such questions as, "Would you break into *that house right now?*" and "Why this house and not that one?" These questions evoked good responses: burglars gave a few simple and sensible reasons based on immediate threats. Perhaps the only difference between Tunnell and Cromwell et al. is that the latter were better able to probe a burglar's meaning when he said, "I just had a feeling that was the

wrong place to break into." Incarcerated offenders may tend to elaborate too much or too little. If the researcher can get closer to the criminal situation, perhaps offenders will present a more revealing indication of their thinking. What the evidence increasingly points to is that offenders are neither empty-headed nor meticulous, have some goals and some means, but do not think very far ahead.

The Rebirth of Private Policing. By Les Johnston. New York: Routledge, 1992. Pp. ix + 251. \$66.95 (cloth); \$18.95 (paper).

Pamela Irving Jackson
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Citing the microsociological focus of most police research, as well as the oversimplification and overemphasis on the control of criminal elements in police history, Les Johnston offers a book that parts with these facets of both. *The Rebirth of Private Policing* is a "must read" for scholars interested in the origins of policing, its links to social structure, or its prospects for the future. The book is comprehensive in scope, exploring in depth the links between the current "rebirth" of private policing and its historical origins in vigilante and moral enforcement groups, discussing the competing ideologies of public and private provision, considering the current and potential impact of privatization on public policing, and detailing current and long-range developments in private policing. The book ends with two well-formulated chapters of conclusions; one pursues new directions in the sociology of policing, and the other considers the relationship between privatization and social control.

It is no accident that *The Rebirth of Private Policing* was written by a British scholar, in light of Britain's exploration of privatization in many service areas in recent decades. (Johnston is deputy director of the Centre for Police and Criminal Justice Studies at the University of Exeter.) But the book is filled with so many examples and policy applications relating to North America that U.S. scholars will find it compelling.

One of the first points that Johnston makes is that private policing is not a novel idea, that, in fact, when viewed in historical context, it represents a variant of the cheap and speedy criminal justice that develops regularly in response to the disorder engendered by transition. In chapter 1, Johnston examines the history, structure, and organization of criminal justice along the dimensions of public versus private, formal versus informal, and central versus local. Private prosecutions and felons' associations, vigilante groups, security companies, and other forms of self-policing are the foci of a myriad of telling historical details buttressing his argument that local elites have often organized self-help organizations to manage antisocial behavior until centrally provided authorities were sufficiently mobilized to respond to the new circumstances,

or where policing proved ineffective. In the latter case (as in cohesive, police-resistant communities), police themselves have sometimes worked through informal community networks.

Chapter 2 compares the ideological supports for both the public and the private provision of goods and services. Johnston notes that "the case for public provision . . . rests upon the view that private markets produce certain inevitable consequences (inequality, injustice, waste, . . . etc.) merely by virtue of being private . . . [and that the case for private provision rests] upon a simple reversal of that same assumption" (p. 28). Despite the debate inherent in conflicting approaches to state-market relations (as put forward in conceptualizations Johnston refers to as the minimal and the limited state, and the rugged individualism of public-choice theorists), it has generally been assumed, Johnston states, that "policing is an inherently public good, whose provision has to reside in the hands of a single monopoly supplier, the state" (p. 24). This view prevails, he points out, regardless of private initiatives that have been evident throughout history. After examining the current ideological underpinnings of both sides, Johnston states that "justification for public and private provision of goods and services has to be seen as a political problem to which there can be no general answer, either in terms of the inherent nature of goods or in terms of the allegedly essential qualities of the public or private sphere" (p. 43). He also terms the public/private dichotomy unrealistic in terms of the reality of service provision.

Chapter 3 relates the privatization debate directly to policing. Liberalization (where responsibility for providing and financing services remains with the state, while nonstate agencies are involved in their delivery) and marketization (where consumers purchase services with allowances or vouchers made directly to them by the governmental unit) are two of the four forms cited that seem to have the most direct relevance to the discussion of policing. He also argues that informal and voluntary modes of provision—ignored in most analyses—are "especially significant in the policing context" (p. 51). Decentralization of financial administration and civilianization are discussed as having emerged from what he calls "the privatization mentality" of recent decades (p. 51), and he warns that neither is a panacea. As future prospects he offers three areas of current development: load-shedding through commercial and voluntary provision of police services (some of which are usurped from police by vigilante and neighborhood watch groups), liberalization through police authorities' contracting out services (including duties in the less controversial areas of maintenance and repair, as well as operational areas such as the "clamping and storage of illegally parked cars," p. 60), and income generation through fees charged for special services (such as policing public events and provision of private security). The development of commercial forms of policing is evaluated from several perspectives including its impact on police public relations and the possibility of the development of a two-tier system of policing, where those with the ability

to pay choose private policing with "the public police function being restricted to control of the urban 'dangerous classes' " (p. 69).

Chapters 4–8 discuss specific forms of private policing. Johnston outlines the current structure, control, and activities of a broad area he refers to as the private security sector (which includes private prisons), describes systems of hybrid policing currently in effect today, discusses community solutions through "responsible citizenship" (which include special and volunteer police, neighborhood watch groups, and citizen patrol groups), and considers autonomous self-policing, such as vigilantism and street and subway patrols.

Johnston draws all of the elements of his analysis together in the concluding chapters, in which he considers the relationship between a spatial continuum of policing (with local, regional, national, and supranational levels) and another continuum based on the sectoral forms of policing (with public, quasi-private/hybrid, and private forms), and predicts that new questions of accountability will arise from the development of a new military-industrial complex and its impact on European policing. Describing the "fragility and ambiguity of the public-private divide" (p. 217), Johnston also argues in the final chapter that the concept of social control "is untenable" (p. 221). With these underpinnings, his final arguments concern the explosive nature of some forms of private policing in the absence of political agreement concerning basic principles of social justice.

There is little doubt that *The Rebirth of Private Policing* will add considerable excitement to current discussions of both privatization and the tenability of existing modes of public police provision. It may also help to foster some of the genuinely comparative, macrosociological work on policing that Johnston claims is conspicuously missing from the literature.

Western Times and Water Wars: State, Culture, and Rebellion in California. By John Walton. Berkeley and Los Angeles: University of California Press, 1992. Pp. xx + 378. \$27.50.

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Few local conflicts have fired the public imagination or generated as much literature, popular and scholarly, as the Owens Valley water wars. The case has become an epic of western development and a powerful American metaphor of the triumph of urban over rural civilization. If ever there was a case that was arguably overstudied it is this. Notwithstanding, John Walton's *Western Times and Water Wars* formidably demonstrates there is both more evidence and more meaning to be churned from this western water drama.

Western Times and Water Wars is a significant book, both for what it contributes to scholarship on the American West and what it adds to social movement theory and comparative social history. Undeterred by what is now a gorge of fiction and nonfiction writing on the Owens Valley water conflict, Walton makes an original contribution to its historiography by tracking its process from a pioneer era of national incorporation through its present-day welfare state manifestations. His thesis, however, is fundamentally directed at social theory, and only incidentally at western historiography. In brief, collective action theorists have failed to generate an empirically accurate, comprehensive, and coherent explanation of the emergence, evolution, transformation, and impact of social movements. This he seeks to rectify.

Drawing mainly on social movement theory in the resource mobilization and moral economy traditions, Walton offers a theoretical synthesis of extant theory emphasizing the role and transformation of the state and the influence of local culture in shaping the character of social movements (he dismisses rational choice theory as an empirically flawed and reified explanation of collective action with little relevance to the Owens Valley case). Walton shows how locally situated actors in the Owens Valley, acting in a shifting and malleable arena of state power and drawing on active cultural values and traditions, combined to define and defend their interests against the forces of external domination and, in the process, incubate the elements of intergenerational protest which, despite state transformation and the failure of earlier movements, persisted to shape successive social movements and eventual movement success.

Empirically, Walton's argument hinges on his ability to demonstrate the linkage between forms of collective action across three broad historical periods: an expansionary era of state incorporation, the Progressive Era, and the modern era of the welfare state and its stepchild, the contemporary environmental movement. This he accomplishes convincingly with a virtuoso display of historiographical methods. Walton shows how dependent development of frontier society and the failure of fragmented modes of collective resistance to state incorporation led directly to new community-oriented strategies of resistance to further incorporation—in the form of the City of Los Angeles's appropriation of the valley's water and property rights. These strategies, even in failure, generated the symbolic, affective, and solidary resources that enabled a new generation of activists to appropriate the incipient national environmental regime of the welfare state era to their advantage.

Walton's arguments for the explanatory importance of state power in a formative era of state incorporation, for the need to examine the full range of state authority in relation to local actors, for the interactive role of local communities in shaping the legitimate complex of state power, and for the enduring role of culture as both collective memory and symbolic resource for mobilizing resistance to state power, are valuable contributions to rectifying what Walton sees as the statics of current theorizing on social movements. For Walton, social movements are fun-

damentally dynamic, contextualized, multilevel, interactive events which unfold over time and may only be comprehended as such.

In sum, Walton's book succeeds at three levels. As historiography, it generates new data which significantly illuminate the social dynamics of Owens Valley's long-standing dispute with Los Angeles, culminating in the environmental movement. As social theory, it makes a powerful case for the role of state and culture in driving the dynamics of social movements. As methodology, it strengthens the case for tightly focused, multi-level studies of critical cases that draw out the shortcomings of broader theoretical formulations and suggest further avenues for analysis and refinement. If there is a fly in the ointment it is only this: the author could have drawn on the rich literature on public organizations and contemporary policy-making to strengthen his case against a monolithic conception of the state in the analysis of social movements. Let us hope that University of California Press releases this book soon in paperback so that it may, as it richly deserves, quickly find its way to the bookshelves of an emerging generation of students and social theorists.

Community versus Commodity: Tenants and the American City. By Stella M. Čapek and John I. Gilderbloom. Albany: State University of New York Press, 1992. Pp. xviii + 326. \$19.95.

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Community Service Society of New York

The fascinating analysis of Santa Monica's tenants' movement offered in *Community versus Commodity* is a model of qualitative research. It provides an intelligent insider's view of an important contemporary social movement.

In chapter 1 Čapek and Gilderbloom review the status of rent control and tenants' movements. They introduce Santa Monica as a progressive ideal to be compared with Houston, the antithesis of regulation. One of the main hypotheses is proposed in this section: that activism in the 1960s was a precursor to social movements, including the tenants' movement, in the following decades. The second chapter offers a thorough review of social movement theories.

Chapters 3, 4, and 5 contain the authors' contribution to understanding social movements via the case study approach. These chapters detail the emergence of a tenants' movement in Santa Monica, one which ultimately contained a broad coalition of the elderly, the poor, and younger activists.

Chapter 4 describes how movement activists won over the "hearts and minds" of Santa Monicans by utilizing particular symbols and vocabulary, and by framing both the movement and its opposition in their own terms. Čapek and Gilderbloom also highlight some of the movement's problems (e.g., the debate between those wanting more democratic

involvement and those wanting more highly specialized campaign technology). The transition of the tenants' movement into a progressive movement after a strong rent-control law was passed in 1979 is recounted in chapter 5.

Houston's housing crises and other problems are discussed in chapter 6. Repeatedly, the authors emphasize that free enterprise has reigned supreme in Houston. In contrast to Santa Monica, they insist, Houston's lack of a cadre of sixties' activists prevented any grass-roots organizing there.

The last chapter reviews the study's findings and conclusions. It incorporates a discussion of the roles rent control can play in solving a housing crisis and as an organizing strategy. Recommendations are made for more progressive housing programs such as community-based housing and homesteading.

As is promised in the foreword, the sections on Santa Monica are nearly flawless. Each aspect of the movement is described and the appropriate theoretical model brought to bear. For Čapek and Gilderbloom, the presences of Tom Hayden, of an elderly New Yorker with extensive knowledge of rent control, of a strong black church group, and of sixties' activists were all "resources" available to the tenants' movement, thus validating resource mobilization theory.

These resources alone, however, would not have been enough to rally large numbers of Santa Monica residents. Here, the authors recognize the need for micromobilization theory and the notion of framing to explain the movement's success. The tenants' movement activists neatly framed the rent control battle as a classic David versus Goliath struggle, exposing the major outside financial interests behind the landlords. Additionally, the tenants' movement used senior citizens as a legitimating symbol for rent control and spoke of the need for "economic democracy." Via the manipulation of symbols, the tenants' movement gained the support of homeowners and "yuppy" renters. Continued control of the community's political will allowed tenants' movement activists to achieve political office and to foster changes beyond mere rent control.

Slightly over 10 pages of the book are devoted to two brief exercises through which the authors try to prove their main hypotheses with cross-tabs and ordinary least squares regression. A wise editor would have eliminated these pages.

The first quantitative exercise compares 1983 survey data from Santa Monica with 1987 survey data from Houston. The data are used to determine what is different about residents of the two cities which would lead to rent control in one and not the other. Given that the Santa Monica movement was fully developed by 1983, it seems that the Santa Monica data may be skewed.

The second quantitative exercise compares the characteristics of 161 cities (which are not listed), to determine what global factors cause some cities to have rent control and others not. From this regression analysis, the authors conclude that the distance a city was from major antiwar

activity in the late sixties is a significant predictor of rent control. A crucial flaw in this argument, however, is that rent-control laws in some Eastern cities (likely to be in the sample) preceded the antiwar movement. Rent-control laws in New York City and nearby Nassau and Westchester counties were first passed in 1943. Furthermore, rent-control laws in New Jersey and other areas near New York City are viewed by housing experts as "spillover" from the New York City law—a possibility not considered in Čapek and Gilderbloom's analysis.

Overall, the study is a masterful combination of historical information, fieldwork, and theoretical analysis. Minus the quantitative sections, it is worthwhile reading for urban scholars, social movement scholars, and methodologists.

Toxic Work: Women Workers at GTE Lenkurt. By Steve Fox. Philadelphia, Pa.: Temple University Press, 1991. Pp. xii + 178.

Life and Death at Work: Industrial Accidents as a Case of Socially Produced Error. By Tom Dwyer. New York: Plenum Press, 1991. Pp. ix + 318. \$45.00.

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One of the jobs I held during an extended hiatus from the academic world was as a material handler in a ceramics tile factory. A young employee who shoveled dry ceramic compound into bins became the laughingstock of the department by insisting on wearing an elaborate safety mask, causing old-timers to guffaw, "I worked there for twenty years and it never hurt me." I joined the general hilarity, even after I worked his job for a week sans mask and had the mucus in my nose turn the color and consistency of the La Brea tar pits. Significant industrial health and safety issues are often obscured in workday habits; exposing the dangers hidden in the "ordinariness" of work life is the common task of these two books.

Toxic Work by Steve Fox describes a protracted confrontation between an electrical components manufacturer in Albuquerque and a large group of its employees over the effects of chemicals used in production. The work force—mostly Hispanic women—was routinely exposed to a wide range of potentially toxic solvents, acids, adhesives, and solders. While employees had discussed common medical problems such as excessive menstrual bleeding and "chemical colds," it took a chance encounter between a GTE worker dying of cancer and local attorney Josephine Rohr to begin a systematic investigation. Rohr found abnormally high occurrences of hysterectomies, neurological dysfunctions, and cancers. The book details her attempts to find legal redress for the employees.

Fox makes clear the complexities in understanding and resolving

claims of work-related illness. Rohr filed charges for more than 200 employees, and she clearly doubted some of them herself. The case histories she took from her clients were often murky and hard to assess. Even though she was convinced that in the main the illnesses recounted were work related, she knew that causation is difficult to prove. The company relied on those ambiguities in its defense, arguing that individual factors such as smoking or obesity were sufficient explanations for the illnesses. One of the merits of the book is that we see both the unclarity of raw industrial health data and the various ways in which companies are able to use their vastly superior resources to defend against claims of toxic exposure. GTE requested large numbers of depositions in an effort to delay, being in a much better position to absorb the costs than the single crusading lawyer and her relatively poor clients. The governing laws also worked to limit the company's liability; it was necessary to prove total disability to have any standing. Rohr eventually felt compelled to accept a settlement that disappointed many of the employees. Even in the act of settling, GTE was able to engage in damage control for themselves and other companies. The terms of the agreement were secret, all relevant documents were destroyed, and witnesses sworn to secrecy. Fox points out how this diminishes the value of the GTE case for future legal use; his term is "white collar hush money."

The book reads like a fast-paced movie script. The reader sees the story as Josephine Rohr did, bit by bit, but with a developing picture of underlying injustice. At times Fox skimps a little on documentation and at other times cites questionable sources (such as *Time* magazine on legal matters). The conclusion—calling for a return to "communitarian values" which supposedly marked earlier periods—seems a little forced. But the drama of the conflict and the sensitivity with which Fox presents it override those weaknesses and make this study useful for courses concerned with the relation of law to social justice.

In *Life and Death at Work*, Tom Dwyer assembles a massive amount of historical and case-study material on industrial safety. He reviews the history of accident theory, describing it as a shifting effort to place responsibility on the victims. He formulates and tests his own "sociological theory" of industrial accidents: they are produced by the social organization of work. Dwyer identifies three "levels" of work relations: rewards, command, and organization. The greater the weight of one of these levels in work relations, the greater the likelihood of accidents produced at that level. The author claims that his theory represents a "break with dominant tradition in industrial accident research" and an advance in understanding the relation between accidents and work organization (p. 151).

The presentation of his argument is flawed in several respects. Some important components are established largely through assertion. He describes the period of the late 1960s as a "rupture," a break with several hundred years of practice in industrial safety. But he spends only two pages defending this extravagant claim. Certainly (speaking of that pe-

riod in this country) the creation of the Occupational Safety and Health Administration (OSHA) was an important act. But the enormous differences in OSHA practices between the Carter and Reagan eras belie Dwyer's depiction of the late 1960s as "crossing the Rubicon" in industrial safety (p. 79). And it is at least debatable that the "participatory management" movements of the 1980s have altered industry *more* than the reforms of his "rupture" period. This approach—unwarranted generalization from insufficient evidence—mars much of the book. One running theme, for instance, is that safety devices, engineering controls, and institutional channels for resolving safety conflicts do more to undermine workers' autonomous understandings than they contribute to safety. He takes this to be self-evidently true.

Dwyer's "sociological theory" of accidents is similarly flawed by unnecessary abstraction and overconceptualization. His three levels seem to be chosen almost at random. One subdivision of the "command level" is "voluntary servitude," which—even if we tolerate the asserted validity of such a contested concept—would seem to be the polar opposite of command, not a subdivision of it. His use of these levels is often simply to insert them into sentences which carry no important information, telling us, for example, that tired workers produce accidents "at the [rewards] level" (p. 195). The supporting empirical evidence is also not convincing. He seems to say at one point that we should expect fewer accidents on second shifts because of relaxed management practices. When two of the three case studies he presents show increased second-shift accidents, he solves the discrepancy by adding epicycles to the theory. A statistical test is based on important variables whose values are determined by subjective and undescribed "observational data." The central finding—that if management does not pay attention to safety and employees have no input, accidents will increase—does not seem to add much to our stock of knowledge about accidents. He undermines even that finding by pointing out without elaboration that earlier versions of the sociological theory failed significance tests.

Dwyer's opaque writing style compounds some of the problems mentioned above. He uses the passive voice constantly, which results in inevitably awkward and convoluted sentences, and needlessly creates neologisms for a single further use. He also anthropomorphizes inanimate subjects: theories attempt, ruptures question. These are not merely stylistic annoyances. They are part of an approach that dresses commonplace observations up in more portentous clothes than they warrant. If I were still working and happened on this book, I would learn a great deal about the history of debates about industrial safety, but very little that would help me understand how to make work safer.

Work and Democracy in Socialist Cuba. By Linda Fuller. Philadelphia: Temple University Press, 1992. Pp. xx + 274. \$44.95.

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Rosary College

Work and Democracy in Socialist Cuba is an interesting and well-researched study that offers a significant contribution to the comparative sociology of work. Linda Fuller's skillful utilization of field and secondary data has produced an objective and comprehensive portrait of post-1959 Cuban work life, one that challenges the interpretations of Jorge Domínguez (*Cuba: Order and Revolution* [Cambridge, Mass.: Belknap, 1978]) and other established Cubanologists whose analyses invariably exude antipathy to Cuban socialism.

Fuller frames her analysis around seven general aspects of democratic work relations including the locus and process of decision making in production as well as the degree of multiplicity in institutional forms which structure workers' participation. The identification of factors that commonly impede workplace democracy provides the basis for subsequent discussion of the Cuban context.

Her point of departure is the role played by Cuban labor unions following the early 1970s when major reforms promoted their revitalization. In moving from a passive role as economic "transmission belt" of the state to an active one as a force advocating workers' rights and greater participation in economic policy-making, unions necessarily walked a path filled with contradictions, largely due, as Fuller shows, to their vested interest in cooperating with the revolutionary state's accumulation strategy. By avoiding the temptation to dismiss Cuban unions as simply partners in a corporatist civil society, she breaks into fertile territory in understanding the complexity of post-1970 Cuban socialism.

As Fuller turns to consider the Cuban Communist Party (PCC), she focuses on its growth, composition, and internal structure in assessing the party's capacity to affect social relations at the point of production. It is important that she observes how non-party members likewise affect party activities and concludes that the existence of a single, vanguard party (as opposed to two or more competing electoral parties) is not in itself a decisive issue. More salient is the degree to which working people can influence the party (parties) in question. The availability of multiple channels of participation for Cuban workers, including the PCC itself, provides avenues for workplace democracy frequently absent in other countries, and Fuller explicitly cites this in distinguishing the Cuban experience from Eastern European-style socialism.

The study then examines measures undertaken in the mid-1970s aimed at consolidating cost accounting in Cuban firms and thus decentralizing economic decision making. Fuller asserts that a measured amount of firm autonomy is a necessary but insufficient step toward democratizing the workplace under socialism, reaffirming that an overly decentralized econ-

omy incapable of strategic planning as well as an overly centralized, command economy both tend to disempower workers. As she details the evolution of workers' participation in Cuban economic planning, she shows it to have increased throughout the 1970s and 1980s as a result of union strength, arriving at a point where it has, despite its numerous shortcomings and setbacks, far surpassed other socialist countries.

An important barometer of workplace democratization is the degree of workers' participation in formal conflict resolution. Fuller describes the tripartite, state-worker-management grievance commissions (*Comisiones de Reclamacio'n*) formed in 1962 which evolved into work councils (*Consejos del Trabajo*) by 1965 when state and management representation was eliminated in all but case appeals. By 1977, unions had acquired jurisdiction even over appeals and for a time administered all workplace grievance procedures. In the early 1980s, however, the Cuban Council of State revoked union jurisdiction over all disputes specifically involving work discipline, and returned it to management. This dramatic move responded to widespread concern that workplace indiscipline had reached intolerable levels. Fuller points out that the measure was enacted with union approval but only following intense debates.

In the final two chapters, Fuller analyzes the changing interrelationships of unions, management, and the PCC, from the early period of union revitalization up through the 1980s rectification campaign. The weight of evidence suggests that unions grew increasingly stronger after the first decade of the revolution, winning substantial gains in workplace democracy through the 1980s and acting as a brake on the state during periods in which mounting economic pressures threatened to reverse democratization reforms. Throughout the post-1970 period, unions remained loyal to the PCC's revolutionary project, and Fuller accounts for this by reference to the particular historical factors that shaped Cuba's organized labor movement.

Fuller's study does not explicitly set out to offer theoretical advancement in the sparse literature of workplace democracy. She nevertheless establishes the pivotal role that unions acting in a socialist context played in furthering workers' participation. This helps temper the judgments of many theorists of workplace democratization whose infatuation with QWL (quality of work life) schemes leaves them excessively critical of unions. Moreover, her presentation of rich historical data convincingly demonstrates the differences between Cuba's socialism and Eastern Europe's now defunct variety. In sum, Linda Fuller has produced an excellent analysis of Cuba which is long overdue and forms essential reading for students of development and labor studies alike.

Managing Modern Capitalism: Industrial Renewal and Workplace Democracy in the United States and Western Europe. Edited by M. Donald Hancock, John Logue, and Bernt Schiller. New York: Praeger, 1991. Pp. 384. \$55.00 (cloth); \$19.95 (paper).

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University of East London

This collection of essays is the outcome of the collaboration of a group of American and European authors in a comparative study of industrial democracy in the postwar period. In its first part, it offers an account of the broad basis of industrial policy for the five nations examined (United States, Britain, France, the former West Germany, and Sweden). Its second section provides case studies of the progress (or lack of progress) of industrial democracy in each of these countries. And in the final section, it provides a more prescriptive discussion of policy issues and prospects. The project is broadly sympathetic to the ideals of industrial democracy—the authors include some of its internationally most important advocates, such as Rudolf Meidner—but throughout the approach adopted is critical and scholarly. The authors recognize that the period of greatest potential for industrial democracy initiatives lies a decade or two back, and some of the most interesting essays examine the reasons for the failures or partial failures of the projects of those years.

Particularly interesting from this point of view are the reflections by Bernard Brown on *autogestion* in France and by Rudolf Meidner on Sweden's wage-earner fund experiment. Brown describes how the *Loi Auroux*, initiated by the socialist government in 1981 to increase workers' voice in industry, became transformed by determined employers influenced by Japanese methods into a strategy for participative management, by means of "quality circles" and the like, effectively outflanking and dividing the trade unions. While this has been important, Brown argues, in bringing French management methods up to date, it has done little to challenge the power of capital. Meidner's essay explains how the wage-earner fund initiative was in part a response to an unintended consequence of Sweden's "solidaristic" wage policies. Restraining wages in profitable firms had the effect of generating excess profits, and the wage-earner funds were a device for creaming these profits off for the workers' collective benefit. The policy has failed, however, not only because of the opposition of capitalists, but also because of its lack of appeal to workers, especially in the profitable sectors which would be likely to generate the largest funds. It was deemed inconsistent with the egalitarian and collectivist purpose of the wage-earner funds to distribute these profits to individual workers, and thus a gap opened between the trade unions, who had custody of the funds, and their members. Meidner's solution to this problem, to spend the proceeds of the funds on less divisive goods such as pensions, education, and the like, seems more like a

reinvention of the welfare state than a step toward industrial democracy as such.

These two interesting narratives reflect a broader picture of individual interests triumphing in the past two decades over attempts to uphold collective goods. John Hillard and David Coates describe the dismantling of Britain's rather unsuccessful neocorporatist institutions during the 1980s, and Jutta Helm outlines a similar drift of policy in Germany, although one that has still left German workers in a more powerful position than most others in Europe. But Catherine Ivancic and John Logue are able to point hopefully to the development of employee share ownership schemes (ESOPs) as a growing sector in the American economy, an approach to industrial democracy that goes with the individualist American grain.

These various accounts make it clear how far the changed balance between forces in the last decade or so has put the advocates of industrial democracy on the defensive. Globalization has exposed national economies to fierce competition, giving capital many options if it does not like what governments do. Most national governments have moved to the right, so that the political support crucial for industrial democracy initiatives has vanished. The fact that economic democracy seems a logical concomitant and extension of political democracy (some powerful philosophical statements of this position are quoted in the book) does not count for much in face of these harsh realities. The arguments that are made for the comparative advantage of more consensual forms of industrial relations are convincing, but while the Japanese industrial model has its relative attractions, economic democracy it is not.

This book offers a thorough review of its topic and makes a valuable contribution to comparative economic and political studies. Most of the essays are competent and accessible sources of information on both the broader and narrower issues of industrial democracy in these different nations, and they bring out differences of national context clearly. Sociologists might like to see a more unified theoretical argument, but that is perhaps not to be expected from a transnational project of this kind. Its various chapters suggest that industrial democracy will only again become a major issue in Western societies if there is a return to rapid economic growth and full employment. Under these conditions, workers' demands and needs have to be taken more seriously, but that may be one reason why these conditions have for the time being disappeared.

Emptying Their Nets: Small Capital and Rural Industrialization in the Nova Scotia Fishing Industry. By Richard Apostle and Gene Barrett. Toronto: University of Toronto Press, 1992. Pp. 396. \$65.00 (cloth); \$24.95 (paper).

Terry Amburgey
University of Kentucky

Emptying Their Nets is an examination of industrial differentiation in the Nova Scotia fishing industry, but the implications of the research reported by the authors extend beyond the fishery. The description of the different types of small capital and their relationships to markets, communities, and resource conditions contributes not only to political economy but also to organization theory, as does the question of whether there is an inexorable trend of accumulation and concentration. Richard Apostle and Gene Barrett show that, when embedded in a network of kinship and community ties, small firms can survive and even thrive while larger corporations are languishing.

The research reported in the book was derived from a six-year study of fishing and fish processing in Nova Scotia. The study involved a variety of surveys and case studies of plant managers, plant workers, fishing captains, fish brokers, and family members. The results of the research are used to illustrate the factors that allowed small capital to avoid subsumption by large capital—the artisanal nature of small firms, personal connections, strong family and community ties, and resource-related factors such as the characteristics of different species of fish and different harvesting technologies.

The first chapter of the book contains a brief overview of three important perspectives in political economy: dualist theory, dependence theory, and differentiation theory. The next two chapters provide a review of the historical development of fishing. The authors argue that there have been four successive phases in the process of capital accumulation but that, even in the post-1945 phase, concentration has been inhibited.

The authors then turn to a contemporary description of the industrial and locational structure of fish processing in chapters 4 and 5. Using both a survey of plant managers and a financial survey, they develop a tripartite typology of small, “competitive,” and “large” plants and discuss the substantial differences in their operating characteristics.

Chapters 6–9 deal with markets and market structure. The discussion of markets begins with the final product market and the characteristics of different product segments. Chapter 7 describes the port market and the importance of informal ties between harvesters and buyers. Chapters 8 and 9 also deal with the port market but emphasize the effects of technology as well as kinship and community.

Chapters 10–13 cover the labor market and community structure. Chapter 10 illustrates the importance of surplus labor in determining both the location and choice of technology in processing plants. Chapters

11 and 12 discuss the role of both social and economic ties that link the harvesting and processing sectors. Chapter 13 describes household and workplace strategies with particular emphasis on the role of the family.

Chapter 14 describes the role of populist ideology in the small capital sector. Apostle and Barrett argue that the differentiated structure of fish processing supports the conservative populism that is the dominant ideology in rural Nova Scotia. Moreover, they argue that it contributes to the high level of political alienation and fragmentation found in the fishery.

The concluding chapter describes how the results of the various studies inform the three theoretical perspectives that guided the research. One important point raised by the authors is that both the dependency and differentiation perspectives suffer from gender blindness. They argue that patriarchal relations pervade both the household and the processing plant, and that while it may provide a "family" atmosphere it can also provide greater control over labor.

One of the strong points of this book is the wide variety of information used to illustrate the nature of fishing and fish processing. At one point the authors describe the financial characteristics of fish-processing plants, at another they provide personalized accounts of the struggle to "get by." However, the variety of topics and methods also contributes to one of the book's weaknesses—the material is not always well integrated and some sections are better developed than others. Most readers would probably prefer to have greater depth to the discussion of communities and their social economy and fewer financial ratios.

Apostle and Barrett start with a preference for the differentiation perspective and use it to guide their efforts. Their work provides substantial support for the differentiation perspective, but it also provides support for dependency theory. Although several forms of small capital are flourishing, it remains to be seen if they will share the same fate as the small-boat draggers described in the book.

Rural Poverty in America. Edited by Cynthia M. Duncan. New York: Auburn House, 1992. Pp. xxvii + 296. \$49.95 (cloth); \$22.95 (paper).

Thomas A. Hirschl
Cornell University

This book features research supported by a Ford Foundation initiative designed to focus the attention of social science researchers on the issue of rural poverty. The authors represent a variety of social science disciplines, and each chapter covers some aspect of rural poverty. The table of contents lists 13 chapters on topics such as American Indians, African-American mother-only families in the rural South, and farm workers.

Five important findings may be deduced from this volume. First, Cynthia Duncan asserts in the introduction that the rural poor population is both substantial and understudied, and estimates that over nine million

rural residents are in poverty at the time of writing. Adams and Greg Duncan find, that of these nine million, four million are "long term poor," defined as persistently poor over a 10-year period. They also note that nonmetropolitan rates of long-term poverty are significantly higher than the corresponding metropolitan rates. Extremely high rates of rural poverty are particularly notable in certain communities. Snipp and Summers identified one Arizona Indian reservation with a family poverty rate of 78.1%.

Second, by far the most reiterated empirical finding identified by the book is the rural poor's labor force attachment. Deavers and Hoppe point out that nearly one-quarter of the rural poor belong to families with two or more workers in the labor force. Similarly, in a study of labor force income, Gorham identifies the wage/salary level necessary to lift a family of four over the federal poverty level. Workers who earn less than this are defined as "low earners," and it is estimated that as a percentage of the total paid labor force, rural low earners increased from 32% to 42% between 1979 and 1987, while urban low earners increased only from 23% to 29%. These and other findings are cited as evidence that increasing spatial disparity within the United States is a particular feature of increasing social inequality.

Third, the demographic composition of the rural poor differs markedly from its metropolitan counterpart. After accounting for the substantial African-American population residing in the rural South, the rural poor are disproportionately white vis-à-vis the central-city poor. In addition, the rural poor are disproportionately elderly and are more likely to belong to husband-wife families.

Fourth, the rural poor are undergoing significant sociodemographic transformations. As is the case for the poor nationally, rates of female family headship are on the rise, and many children spend significant periods of childhood in single-parent families. Approximately half of the long-term rural poor identified by Adams and Greg Duncan are children, and many of these belong to families with weak labor force attachment. The dynamics of the American Indian population identified by Snipp and Summers are particularly interesting. During the 1970s, they find substantial increases in Indian educational attainment and major declines in rates of family poverty. However, much of the decline in poverty can be explained by demographic change (reduced fertility and family size) rather than by increased personal income. Thus it is apparent that the Indian population did not greatly benefit from the high levels of social spending that occurred during that decade.

Finally, the book presents an informative and wide-ranging set of policy discussions and prescriptions. Snipp and Summers provide a valuable description of how U.S. policies have directly and indirectly affected American Indians since the first whites arrived in North America. O'Connor identifies the various social science rationales of 20th-century antipoverty policies, and then summarizes the broad efficacy of these policies from the New Deal to the present. In a chapter on empowerment

of the poor, Suitts argues that history has tended to disfranchise the Southern rural poor, but that significant progress has been made since the civil rights movement. Data on the voting behavior of congressmen representing the rural South and on the number of African-American elected officials are analyzed to assess this argument. Finally, Greenstein and Shapiro outline the various government policies and programs that, if organized in concert, would greatly ameliorate and potentially eliminate rural poverty.

A central theme of the book is that politics is the primary obstacle to eradicating rural poverty. Deavers and Hoppe assert that 2.5% of the gross national product is spent on programs designed to aid the poor and imply that this is simply not enough. O'Connor argues that antipoverty policies have been undermined by lack of funding and that this is traceable to a characteristically weak political will in Washington, D.C.

This book has merit both as a research resource and as a teaching tool. It contains useful literature reviews, and the empirical analyses make original contributions to understanding the spatial configuration of poverty in the United States. As a teaching tool, it is a valuable resource for undergraduate courses dealing with inequality and is also appropriate for graduate-level courses. For social scientists interested in rural poverty, this book is a must.

Separate Societies: Poverty and Inequality in U.S. Cities. By William W. Goldsmith and Edward J. Blakely. Philadelphia: Temple University Press, 1992. Pp. xviii + 247. \$49.95 (cloth); \$19.95 (paper).

Kirk E. Harris

Legal Assistance Foundation of Chicago

Race and class divisions in American society have grown with amazing persistence and at a disturbing pace. These divisions are a result of the changing economic dynamic that now reflects the demands of a global economy. The confluence of several factors has functioned to undermine the economic integrity and tatter the social fabric of inner-city communities. Among the factors impinging on the ultimate survival of impoverished neighborhoods are the lack of effective government policy that limits the mobility and unproductive use of corporate capital, diversion of resources away from ensuring a healthy, adequately housed, literate, and well-trained populace, hypereconomic and social segregation of poor inner-city communities from employment opportunities, shrinking opportunities for well-paid blue-collar work, depressed wages among those in blue-collar employment, and declining purchasing power among the poor and working class. At present, insufficient response by governmental entities at all levels has generated a set of conditions that promises to replicate the experiences of Los Angeles in other cities throughout the United States, as urban unrest constitutes the disjointed retort of poor

urban communities against their economic and social oppression. William Goldsmith and Edward Blakely seek to highlight the economic and social dislocation in urban centers and rearticulate the debate in a way that strikes at the substantive unfairness and intolerable conditions existing in American urban centers.

Goldsmith and Blakely's book opens with a foreword by Harvey Gantt, a planning practitioner in the 1960s, the first black mayor of Charlotte, North Carolina, in the 1980s, and an unsuccessful reform-oriented senatorial candidate, who as a professional and politician has been very concerned about urban and social problems. Gantt sets the tone for this book, which effortlessly addresses the interests and concerns of policymakers, urban planning practitioners, and academics alike. Gantt provides a unique introduction and insight into the national policy debate on the urban crisis. Race-based politics and questions of redistribution, which Gantt barely touches on, reflect a national struggle that rages over what has been viewed as the excesses of the welfare state. In the decades since the 1960s, neoconservative opinion has placed the bulk of the criticism for these excesses on inner cities and their residents.

Goldsmith and Blakely's book is extremely timely in that it seeks to document the distorted logic of national economic and social policy and refute neoconservative claims that poverty is a function of the expansion of the welfare state, which encourages welfare dependency and discourages work. In fact, Goldsmith and Blakely point out that many of the poor are working poor. They argue that neoconservatives use welfare as a scapegoat, when in reality a poorly adjusted and sluggish economy fostered by right-wing policies of deregulation and unbridled corporate decision making are at the heart of the problem. In specific terms, the authors believe that short-term, profit-driven corporate decision making which treats the American worker as a disposable commodity is the core of the problem. The authors posit that this short-term thinking has left the United States lagging behind other industrial nations in productivity and has left American workers unemployed and in poverty. Additionally, the authors vividly illustrate that the persistence of poverty in urban centers is largely a function of the gross income and wealth inequities present in American society.

In combination with the discussion of poverty trends for the overall population, Goldsmith and Blakely clearly and skillfully draw out the racial implications of these trends. Goldsmith and Blakely argue that structural changes have a disproportionate impact on minority communities which must contend with unstable low-wage jobs, higher credentialing requirements for employment, horrendous levels of unemployment and underemployment, and systematic segregation and discrimination which run rampant in the private sector, and are in many instances facilitated or left unchecked by governmental policies. While firmly committed to the structural view of poverty, the authors are by no means structurally deterministic in their viewpoint. They are committed to the view that communities have the potential to offset these structural forces.

Their belief is that local communities can shape their response to general economic and corporate restructuring with assertive and planned local action.

Finally, not unlike William J. Wilson (*The Truly Disadvantaged: The Inner City Underclass and Public Policy* [Chicago: University of Chicago Press, 1987]), the authors call for industrial, educational, and family support policies that have universal appeal. But in contrast to Wilson, Goldsmith and Blakely advocate a "bottom-up" approach to their policy considerations and have a keener and more sophisticated appreciation for the political implications of their proposals. Goldsmith and Blakely are optimistic, as am I, about the possibility of citizen actions at the local level turning the tide on prevailing national trends. However, I believe they underestimate the problematic racial and class divisions at the local level that may cause local action in the most distressed inner cities to falter. All in all, this book is a must read for anyone who is serious about being informed or joining the debate on the urban crisis.

Education and Society in Hong Kong: Toward One Country and Two Systems. Edited by Gerard A. Postiglione. Armonk, N.Y.: M. E. Sharpe, 1992. Pp. xiv + 314. \$45.00.

Stephen Lung Wai Tang
Chinese University of Hong Kong

The signing of the Joint Declaration between Britain and China over the future of Hong Kong in early 1985 marked the beginning of a new era in Hong Kong's development. Its constitutional link with Britain will terminate on June 30, 1997, when it becomes part of China as a special administrative region (SAR) with a high degree of autonomy. Despite promises by both parties to keep changes in Hong Kong to a minimum, most residents there think otherwise. The effective operation of China's invisible hand in a multiplicity of critical domains, clearly reflected in its manipulation of changes in Hong Kong's formative democratic processes and in its penetration into key economic sectors, has further reinforced the skepticism of many that Hong Kong will be able to enjoy much autonomy after 1997. The period leading to 1997 has been defined by both key power players, Britain and China, as a transition period, but with grossly divergent connotations. While recognizing it as a period of decolonization, Britain has tried its best to seize the opportunity to reposition itself economically and politically, whereas China has made no pretense of its enthusiasm to take over many privileges that Britain had reserved for itself during its colonial heyday. Thus, the transition could be taken as an illustration of the opposing faces of decolonization and reversion. In the process, however, the more dynamic development-induced changes would often, unfortunately, be taken as merely epiphenomenal.

Now, half-way through the transition period scholars in different fields are interested in finding out how it really fares. Ming K. Chan and Gerard A. Postiglione have dedicated a series to the subject of "transition to 1997"; *Education and Society in Hong Kong* is part of that series. The book is edited by Postiglione with 13 invited papers written mainly by scholars from the schools of education at the two Hong Kong universities and focuses on the interface between education and society in Hong Kong as part of the dynamic political process toward "one country and two systems." The editor defines the role of education in the new era as follows: "Education is expected to play a part in cementing the reunification. New educational reforms have the potential to act as vehicles for negotiating social transition, as well as instruments for resisting decolonization" (p. 3). The volume arranges the papers into five groups as follows: education and the social context, education and politics, stratification, educational issues—language and labor, and finally, comparative perspectives—one country, two educational systems.

While seeking to address the critical role education is playing in the transition, the volume falls short of its aim to do so with academic rigor and relevance. The quality of its papers varies significantly, with some quite thorough and well documented and others falling between casual observations and journalistic commentaries. Postiglione's excellent "The Decolonization of Hong Kong Education" succeeds in putting the problems of education in the transition period in a nutshell, and Sweeting's "Hong Kong Education within Historical Processes" provides an integrated overview of the development of education as a social and political institution in Hong Kong. Morris's "Preparing Pupils as Citizens of the Special Administrative Region of Hong Kong" meticulously documents changes in the civic education curriculum in synchronization with the growing demands for a new citizenship since the signing of the Joint Declaration.

While Cheng's "Educational Policymaking in Hong Kong: The Changing Legitimacy," Leung's "Education in Hong Kong and China: Towards Convergence," Pierson's "Cantonese, English, or Putonghua," and Shive's "Educational Expansion and the Labor Force" are no doubt dealing with crucial subjects, they fail to fully articulate the issues and forcefully differentiate with substantive data the relative effects of decolonization, reversion, and growth-induced changes. Li's "On the Characteristics, Strong Points, and Shortcomings of Education in Hong Kong" reads as a dogmatic misrepresentation of Hong Kong education.

The volume also suffers from problems of relevance by its inclusion of some interesting but barely pertinent topics. Cheung and Lee's "Egalitarianism and the Allocation of Secondary School Places in Hong Kong" succeeds in articulating the crucial political debate on educational opportunities, but does not match the theme of the volume. Mak's "The Schooling of Girls in Hong Kong: Progress and Contradictions in the Transition" and Bray's "Hong Kong Education in an International Con-

text: *The Impact of External Forces*” turn out not only to lack relevance but to be weak in argument and coherence.

In sum, as a loosely organized collection of invited papers, the volume provides some valuable knowledge and observations on the role of education in Hong Kong’s transition to a future SAR under China, but adds very limited theoretical enhancement to the field.

The Crisis of Socialism in Europe. Edited by Christiane Lemke and Gary Marks. Durham, N.C.: Duke University Press, 1992. Pp. 253. \$39.95.

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University of Massachusetts

The Crisis of Socialism in Europe is one of an increasing number of books inspired by the unforeseen and astonishing developments in what used to be the Soviet bloc. Several motives prompt the appearance of such volumes: the desire of specialists to come to grips with the collapse of Communist systems, the urge to stake out new positions by those who had earlier misinterpreted the condition and prospects of Communist societies (usually by crediting them with far more legitimacy and stability than they possessed), and the wish of some scholars to share their incomprehension of the unraveling of Communist states with their colleagues and seek solace for their inability to predict these developments in the company of kindred spirits. Last, we must not omit the desire to salvage the ideals and promises of socialism from the rubble of disillusionment and discredit that has been heaped upon them in every country where an attempt was made to realize the precepts of Marx and his followers.

The position concerning Marxism on the part of the contributors to this book is perhaps best summed up by Konrad Jarausch: “As a dogma justifying the dictatorship of a party, Marxism is discredited. As a critical perspective on social inequality, Marx’s ideas continue to offer stimulating insights” (p. 231).

The volume here reviewed differs from other similar enterprises in that the contributors seek to determine the prospects of socialism in both the former Soviet dependencies of Eastern Europe and the rest of Europe. It appears to be their cautious hope that the collapse of “existing socialism” need not completely undermine the Marxist critiques of capitalism (e.g., “Purged of its Stalinist perversions and even some of its Leninist legacies . . . the Marxist vocabulary can still function intermittently as a critique of market abuses” [p. 235]).

To the extent that the essays in this collection share a common perspective it is the desire to draw conclusions from the unraveling of Communist (or “existing socialist”) states that allows the authors to retain hope in the revival or survival of the socialist ideal. Most of the contributors wish to separate these ideals from what had transpired in the Communist systems.

The volume begins with a substantial introduction by the editors addressing "The Fate of Socialism in Europe." Other pieces deal with the socialist tradition (G. Eley), "Revolution and Counter-Revolution in Eastern Europe" (N. Naimark), the crisis and future of socialism in Eastern Europe (S. Wolchik), the social and political implications of the 1990 elections in Hungary (Szelenyi and Szelenyi), the general prospects of social democracy (W. Merkel), the West German Left (A. Markovits), and left-wing politics and ideology in Western Europe (H. Kitschelt). The postscript by K. Jarausch probes once more the prospects of socialism and social democracy under the new historical conditions. He concludes that they should persist because of their historical contribution to preserving the health and vitality of capitalism.

This book excludes analysis of developments in the former Soviet Union. Specialists on specific Eastern European countries are underrepresented (only the Szelenyis belong to this category) while five of the contributors are specialists on contemporary German politics and society. Several of the contributors share a concern with the future of social democracy both in Western Europe and in the former Communist countries, an interesting departure from the indifference that used to be shown by Western social scientists toward such movements and parties.

The reader unfamiliar with conditions in Eastern Europe both before and after the collapse of Communist systems can learn a good deal from these well-informed essays. But one can take issue with numerous specific points and suggestions. For instance, Eley seems to think that left-wing movements in the West, such as the British Campaign for Nuclear Disarmament or the student movements of the late 1960s had an affinity with the revolutions in Eastern Europe in 1956 and 1968 (pp. 44–45)—a dubious proposition. Naimark believes that the Soviet-type socialist systems of Eastern Europe emerged for reasons additional to the arrival and prolonged presence of the Soviet forces and the handful of native party activists who came with them returning from exile (e.g., pp. 63–64); the Szelenyis equate the legitimacy of the current Hungarian government (which came into power with an election in which only two-thirds of the population voted) with the total illegitimacy of the Communist government which was in power at the pleasure of the Soviet occupation forces and "legitimated" by one-party elections (p. 131).

The question rarely confronted in these essays that now increasingly requires an answer is, Why it has been so difficult to realize the Marxian ideals? What has it been in the theories of Marx and his followers that so stubbornly defied implementation—assuming for the moment that the Communist states now defunct were mere parodies of what Marx had in mind, that they had nothing whatsoever in common with Marxian ideals and ideas—in itself a questionable proposition that deserves further inquiry. Of course the alternative is to suggest that Communist states did realize *some* if not all of what Marx had in mind, that theory and practice were not *totally* apart.

A problem of the Western Left (which also includes some students of

the former Communist states), as pinpointed by one of the contributors, Andrei Markovits, is that it seeks to salvage what it regards as the positive features of the now defunct systems while concurrently arguing that they had nothing to do with the true ideals of socialism. As to the desire for rescuing these ideals, the postscript says it all: "At the bottom of this longing seems to be a . . . need for a utopian vision of society that makes present day imperfections bearable and life meaningful" (p. 220).

Schumpeter: A Biography. By Richard Swedberg. Princeton, N.J.: Princeton University Press, 1992. Pp. vii + 293. \$24.95.

Robert Holton

Flinders University

There are comparatively few intellectual biographies of genuine sociological interest. Richard Swedberg's new study of the Austrian social economist Joseph Schumpeter stands among the very best. Swedberg's achievement centers on the skill with which he threads together the rich and complex tale of Schumpeter's theoretical development with an intimate account of his subject's personal life—his hopes, fears, loves, and obsessions. This enables him to identify a fascinating paradox in Schumpeter's life, namely the sense of personal failure that beset a man who, by any estimation, had made a major contribution to economic theory for nearly five decades.

Schumpeter is probably best remembered among sociologists for *Capitalism, Socialism, and Democracy*, first published in 1942. This major work brought together a number of Schumpeter's fundamental interests. These included the formulation of a dynamic theory of economic evolution incorporating processes of innovation and business cycles, the prognosis for capitalist entrepreneurship in an epoch of rationalization and socialist advance, and, above all else, the construction of a social economics that linked together the fields of economic theory, economic history, statistics, and economic sociology. Schumpeter's argument, drawing on Max Weber's earlier work, was that capitalism while economically robust could not survive the rationalization process, which would, among other things, routinize the principal source of innovation and economic dynamism, namely entrepreneurship. Managerialism was taking over from entrepreneurship, and the bourgeoisie was losing its vitality and becoming defeatist.

Unlike the more conventional Austrian neoclassical economists, Schumpeter believed that socialism more closely reflected the logic of rationalization, or put another way, he believed that rational socialism was possible. However, it remained unclear exactly how far socialism would be consistent with democracy, since socialism remained, in his view, culturally indeterminate. This point anticipates the politically divergent forms of postwar socialism, ranging from absolutism to social

democracy. Just as capitalist economies took much of their political character from the surrounding social structure (a point taken up by Barrington Moore), so Schumpeter suggests will socialist economies be influenced by the surrounding social structure and the range of autocratic, aristocratic, and proletarian features reflected in it. For Schumpeter social structures influence the fate of economic systems far more profoundly than Marx realized.

Swedberg shows the long intellectual route by which Schumpeter reached the position he eventually adopted. By means of a thorough and judicious review of Schumpeter's published texts, unpublished letters, and personal papers, together with the testimony of colleagues and friends, Swedberg pieces together a fascinating account of how the most promising young economist in pre-World War I Austria survived personal and financial crises in the 1920s and moved to Harvard to promote a wider and more sociological approach to economic analysis. This account is further enhanced by a bibliographical listing of all Schumpeter's writing and by the reprinting of a number of his letters and aphorisms.

A sense of personal failure and an impending preoccupation with death haunted Schumpeter through his Harvard days. This owed much to the devastating impact made by the tragic deaths of his mother, wife, and newborn son all within a few months during 1926. Nonetheless his creativity returned. Between 1932 and his death in 1950 he wrote *Business Cycles* (1939), *Capitalism, Socialism, and Democracy* (1942), and the bulk of the posthumously published *History of Economic Analysis* (1954).

One of the major intellectual frustrations felt by Schumpeter was his failure to produce a major contribution to the theory of money. He also felt himself to have been increasingly upstaged by successive works by John Maynard Keynes during the late 1920s and 1930s. Swedberg's portrait of an intellectual whose ideas and ambitions flew ahead of his capacity to realize them in print is surely one that will strike a chord with most scholars, perhaps even more so among the more prolific.

The reasons for Schumpeter's sense of failure also relate very closely to the insuperable difficulties in living up to the aristocratic vitalism within which his personal ambitions were embedded. "Early in life I had three ambitions," Schumpeter once remarked, "I wanted to be the greatest economist in the world, the greatest horseman in Austria, and the best lover in Vienna. Well, I never became the greatest horseman in Austria." On another occasion Schumpeter added two more ambitions, namely, political success and recognition as a connoisseur of art. Swedberg's biography does justice both to Schumpeter the showman and Schumpeter the intellectual.

Schumpeter's most striking intellectual legacy is his courageous attempt to move beyond the static and rather desiccated view of economic life presented by the mainstream proponents of neoclassical orthodoxy, while not abandoning economic theory or abstraction. The principal merit of this excellent book is the painstaking way in which Swedberg traces Schumpeter's evolution as a social economist able to build substan-

tive intellectual bridges between economics and sociology. In today's intellectual climate of imperialist designs on each other by both economists and sociologists, Schumpeter's plea for a free trade in ideas and for interdisciplinary synthesis is well worth heeding. Swedberg has done a magnificent job of bringing Schumpeter's achievement back to center stage.

The End of History and the Last Man. By Francis Fukuyama. New York: Free Press, 1992. Pp. xxiv + 418. \$24.95.

John A. Hall
McGill University

Ideas gain importance in times of uncertainty. Just as the onset of the Cold War led to fame for George Kennan, so its demise accounts for the attention given to Fukuyama's "The End of History," an article published in the *National Review* in the summer of 1989. The phrase is derived from Kojève's interpretation of Hegel: history is seen as ending only in the sense that it is no longer possible to imagine any radically alternative future. The world in which we live—whose leading representative is not, as Hegel had it, Prussia but rather the United States—is it, there is no other. The fact that the United States combines liberalism and capitalism allows Fukuyama basic optimism, and this is massively enhanced by the belief that evolutionary forces exist that ensure that other societies will eventually converge to this single point. This bold thesis is not unknown to sociologists, but recent events have made it well worth considering again, not least because Fukuyama's optimism is by no means uncritical and total.

The first way in which the book expands on the article is by the specification of two mechanisms that drive the historical process. The cognitive revolution of modern science is held to have gained its salience because of the presence in northwestern Europe of competing states always eager to adopt the new technology. A related and central part of the argument stresses that capitalism is by far the most efficient economic mechanism known to man. A proper obeisance is paid to modernization theory in this section of the argument, but Fukuyama sensibly and interestingly distances himself from the view that commerce automatically gives rise to political liberty.

Nonetheless, a second mechanism does guarantee the emergence of freedom. Fukuyama again draws on Hegel—or, as he freely admits, on a particular interpretation of Hegel—to argue that human psychology contains an innate drive to gain recognition from others, a drive which can only be satisfied fully in a society of equal citizens. Given the pattern of recorded history, this is of course an extraordinary claim, and it leads Fukuyama to produce some curious intellectual history. It is important to stress that he is a product of a Straussian cénacle at the University of

Chicago. Accordingly, he tends to deride the British empiricist tradition as narrow dogmatism positing a singular materialist motivation for humanity. That was rarely so, as Fukuyama half admits when noting that the Scottish moralists—among others—favored commerce essentially for the entirely instrumental reason that money making would control the violent political passions that they knew all too well.

The second way in which the book goes beyond the initial article is through detailed consideration of possible counterargumentation. Fukuyama seeks to downplay the view that geopolitics may interrupt benign tendencies of social evolution, as they did in 1914. His views on international relations are generally interesting, and he naturally makes much of Kant's claim, as interpreted for us by Michael Doyle, that liberal societies tend not to fight each other. Nonetheless, there are problems for his case in this area that he does not fully consider: the United States remains wedded quite as much to an heroic as to a trading strategy in international affairs, and this—rather than the possibility that Japan may move from deference to resentment—may well prove to be a fundamental source of instability to the post-Cold War world polity. But Asian economic development does cause a terrible wobble in the argument. Fukuyama's concern with noneconomic sources of motivation leads him to appreciate cultural sources of the work ethic—as well as to be frequently insightful about religion and nationalism. But Singapore, South Korea, and Taiwan may represent an alternative to that combination of capitalism and liberal democracy which is supposedly our historical terminus. Differently put, there is little reason to believe that the writing is on the wall for this particular version of capitalism plus authoritarianism. Equally, social democracy is a historical option quite far removed from the current political practices of the United States.

While it would be easy to poke holes in this book by accusing it of mere American triumphalism, it would be unfair to do so. It is bold, lucid, and careful, and it is clearly the product of genuine thought and worry. There is much truth in the argument: there can be no doubt, for instance, that the collapse of state socialism does mean that the obvious alternative historical project has finally lost its appeal. Nonetheless, sociologists should be able to do rather better than Fukuyama. Most obviously, the emphasis on an innate drive for recognition is horribly vague. A measure of reciprocity in social relations is a historical norm, as Barrington Moore has shown, but this is normally satisfied at the domestic or communal rather than the national level. We need to explain the differential emergence of modern citizenship—and are in fact becoming rather good at so doing. Still more important, sociological research must lead to much skepticism toward the scorn shown for “merely instrumental” societies. There really is no evidence to suggest that normal Americans live impoverished lives, finding it impossible to associate with each other. More generally, the United States remains the most Durkheimian of societies, even in its individualism.

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CONTENTS OF VOLUME 98

Articles

- 1388 ALBA, RICHARD D., and JOHN R. LOGAN. Minority Proximity to Whites in Suburbs: An Individual-Level Analysis of Segregation
- 308 AMENTA, EDWIN, BRUCE G. CARRUTHERS, and YVONNE ZYLAN. A Hero for the Aged? The Townsend Movement, the Political Mediation Model, and U.S. Old-Age Policy, 1934–1950
- 1259 ANSELL, CHRISTOPHER. See PADGETT
- 628 AXINN, WILLIAM G. See THORNTON
- 30 BEARMAN, PETER S., and GLENN DEANE. The Structure of Opportunity: Middle-Class Mobility in England, 1548–1689
- 511 BELL, ELEANOR O. See LIEBERSON
- 280 BRANTLEY, PETER. See FLIGSTEIN
- 131 CAMARIGG, VALERIE. See GLASS
- 997 CAMPBELL, CAMERON. See PRESTON
- 1039 CAMPBELL, CAMERON. See PRESTON
- 308 CARRUTHERS, BRUCE G. See AMENTA
- 755 CLEMENS, ELISABETH S. Organizational Repertoires and Institutional Change: Women's Groups and the Transformation of American Politics, 1890–1920
- 1428 CLOGG, CLIFFORD C. See HOGAN
- 873 COHEN, LAWRENCE E. See VILA
- 1020 COLEMAN, JAMES S. Comment on Preston and Campbell
- 30 DEANE, GLENN. See BEARMAN
- 368 DIETZ, THOMAS. See FREY
- 1351 DOHRENWEND, BRUCE P. See LINK
- 1428 EGGEBEEN, DAVID J. See HOGAN
- 105 FIREBAUGH, GLENN. Growth Effects of Foreign and Domestic Investment
- 280 FLIGSTEIN, NEIL, and PETER BRANTLEY. Bank Control, Owner Control, or Organizational Dynamics: Who Controls the Large Modern Corporation?

- 152 FREEMAN, LINTON C. The Sociological Concept of "Group": An Empirical Test of Two Models
- 368 FREY, R. SCOTT, THOMAS DIETZ, and LINDA KALOF. Characteristics of Successful American Protest Groups: Another Look at Gamson's *Strategy of Social Protest*
- 555 GERHARDS, JÜRGEN, and DIETER RUCHT. Mesomobilization: Organizing and Framing in Two Protest Campaigns in West Germany
- 131 GLASS, JENNIFER, and VALERIE CAMARIGG. Gender, Parenthood, and Job-Family Compatibility
- 721 GOULD, ROGER V. Trade Cohesion, Class Unity, and Urban Insurrection: Artisanal Activism in the Paris Commune
- 1094 GRIFFIN, LARRY. Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology
- 597 HAGAN, JOHN. See MCCARTHY
- 628 HILL, DANIEL H. See THORNTON
- 1428 HOGAN, DENNIS P., DAVID J. EGGEBEEN, and CLIFFORD C. CLOGG. The Structure of Intergenerational Exchanges in American Families
- 451 JONES, STEPHEN R. G. Was There a Hawthorne Effect?
- 368 KALOF, LINDA. See FREY
- 1033 LAM, DAVID. Comment on Preston and Campbell
- 1351 LENNON, MARY CLARE. See LINK
- 511 LIEBERSON, STANLEY, and ELEANOR O. BELL. Children's First Names: An Empirical Study of Social Taste
- 1351 LINK, BRUCE, MARY CLARE LENNON, and BRUCE P. DOREN-WEND. Socioeconomic Status and Depression: The Role of Occupations Involving Direction, Control, and Planning
- 1388 LOGAN, JOHN R. See ALBA
- 597 MCCARTHY, BILL, and JOHN HAGAN. Mean Streets: The Theoretical Significance of Situational Delinquency among Homeless Youth
- 799 MCDANIEL, ANTONIO. See MORGAN
- 799 MILLER, ANDREW T. See MORGAN

- 799 MORGAN, S. PHILIP, ANTONIO MCDANIEL, ANDREW T. MILLER, and SAMUEL H. PRESTON. Racial Differences in Household and Family Structure at the Turn of the Century
- 1259 PADGETT, JOHN, and CHRISTOPHER ANSELL. Robust Action and the Rise of the Medici, 1400–1434
- 469 PETERSEN, TROND. Individual, Collective, and Systems Rationality in Work Groups: Dilemma and Market-Type Solutions
- 67 PETERSEN, TROND. Payment Systems and the Structure of Inequality: Conceptual Issues and an Analysis of Salespersons in Department Stores
- 829 PODOLNY, JOEL M. A Status-based Model of Market Competition
- 1320 PORTES, ALEJANDRO, and JULIA SENSENBRENNER. Embeddedness and Immigration: Notes on the Social Determinants of Economic Action
- 799 PRESTON, SAMUEL H. See MORGAN
- 1039 PRESTON, SAMUEL H., and CAMERON CAMPBELL. Reply to Coleman and Lam
- 997 PRESTON, SAMUEL H., and CAMERON CAMPBELL. Differential Fertility and the Distribution of Traits: The Case of IQ
- 555 RUCHT, DIETER. See GERHARDS
- 340 SCHUMAN, HOWARD. See STEEH
- 1320 SENSENBRENNER, JULIA. See PORTES
- 1 SEWELL, WILLIAM H., JR. A Theory of Structure: Duality, Agency, and Transformation
- 237 SHALIN, DMITRI N. Critical Theory and the Pragmatist Challenge
- 340 STEEH, CHARLOTTE, and HOWARD SCHUMAN. Young White Adults: Did Racial Attitudes Change in the 1980s?
- 628 THORNTON, ARLAND, WILLIAM G. AXINN, and DANIEL H. HILL. Reciprocal Effects of Religiosity, Cohabitation, and Marriage
- 873 VILA, BRYAN J., and LAWRENCE E. COHEN. Crime as Strategy: Testing an Evolutionary Ecological Theory of Expropriative Crime
- 1044 WARNER, R. STEPHEN. Work in Progress toward a New Paradigm for the Sociological Study of Religion in the United States

- 1134 ZHOU XUEGUANG. *The Dynamics of Organizational Rules*
308 ZYLAN, YVONNE. See AMENTA

Book Reviews

- 1239 ANDERSON, JEFFREY J. *The Territorial Imperative: Pluralism, Corporatism, and Economic Crisis*. Johan P. Olsen
- 1233 ANDERSON, KAY J. *Vancouver's Chinatown: Racial Discourse in Canada, 1875-1980*. Peter Kwong
- 1512 APOSTLE, RICHARD, and GENE BARRETT. *Emptying Their Nets: Small Capital and Rural Industrialization in the Nova Scotia Fishing Industry*. Terry Amburgey
- 952 ASHER, RAMONA. *Women with Alcoholic Husbands: Ambivalence and the Trap of Codependency*. Norman K. Denzin
- 1187 AYRES, IAN, and JOHN BRAITHWAITE. *Responsive Regulation: Transcending the Deregulation Debate*. Anne M. Khademian
- 692 BAGGULEY, PAUL. *From Protest to Acquiescence? Political Movements of the Unemployed*. Richard Lewis
- 195 BAINBRIDGE, WILLIAM SIMS. *Goals in Space: American Values and the Future of Technology*. Joseph Tatarewicz
- 186 BAUMEISTER, ROY F. *Meanings of Life*. Pierre Hegy
- 420 BELL, CATHERINE. *Ritual Theory, Ritual Practice*. Philip Smith
- 933 BERGER, IRIS. *Threads of Solidarity: Women in South African Industry, 1900-1980*. Sonya O. Rose
- 1222 BERNARD, THOMAS J. *The Cycle of Juvenile Justice*. Mark D. Jacobs
- 1173 BLAU, JOEL. *The Visible Poor: Homelessness in the United States*. Peter H. Rossi
- 1466 BOGGS, VERNON W., ed. *Salsiology: Afro-Cuban Music and the Evolution of Salsa in New York City*. Paul D. Lopes
- 1195 BOHMAN, JAMES. *New Philosophy of Social Science: Problems of Indeterminacy*. Michael G. Flaherty
- 956 BONALD, LOUIS DE. *On Divorce*. Robert Alun Jones
- 1200 BOURDIEU, PIERRE. *Language and Symbolic Power*. John D. Kelly
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- 986 WERNER, EMMY E., and RUTH S. SMITH. *Overcoming the Odds: High Risk Children from Birth to Adulthood*. Janet L. Lauritsen
- 216 WHITE, JAMES M. *Dynamics of Family Development: A Theoretical Perspective*. Valarie King
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- 1225 ZWEIGENHAFT, RICHARD L., and G. WILLIAM DOMHOFF. *Blacks in the White Establishment? A Study of Race and Class in America*. Elijah Anderson

BOOK REVIEWERS

- PATRICIA A. ADLER, 175
 PETER ADLER, 175
 TERRY AMBURGEY, 1512
 EDWIN AMENTA, 393
 ELIJAH ANDERSON, 1225
 ROBERT W. AVERY, 412
 KURT W. BACK, 233
 JAMES A. BECKFORD, 958
 MABEL BEREZIN, 1218
 JULIA BERNARD, 963
 MARGARETA BERTILSSON, 672
 DENISE D. BIELBY, 943
 FRANK BONILLA, 208
 CHARLES L. BOSK, 984
 PAUL BRANTINGHAM, 1220
 APRIL BRAYFIELD, 684
 DANIEL BRESLAU, 415
 JOHN D. BREWER, 666
 JULIE BRINES, 938
 RICHARD H. BROWN, 433
 JAMES BURK, 1185
 MARTIN J. BURKE, 1180
 PAUL BURSTEIN, 231
 RON BUSSELMAN, 1491
 MICHELLE D. BYNG, 652
 MICHAEL P. CARROLL, 967
 BRUCE G. CARRUTHERS, 711
 TANYA M. CASSIDY, 659
 JOY CHARLTON, 969
 KEVIN J. CHRISTIANO, 708
 CANDACE CLARK, 1210
 ELISABETH S. CLEMENS, 686
 JONATHAN M. COHEN, 654
 STEPHEN COLE, 1489
 PAUL COLOMY, 1196
 DHARMA E. CORTÉS, 1211
 THOMAS CRUMP, 1184
 THOMAS CUSHMAN, 225, 1247
 CHRISTOPHER DANDEKER, 926
 DALE DANNEFER, 417
 CHRISTIE DAVIES, 992
 RICHARD A. DELLO BUONO, 1508
 NANCY A. DENTON, 438
 NORMAN K. DENZIN, 952
 STEVE DERNÉ, 1482
 MICHELE DILLON, 1241
 WILLIE J. EDWARDS, 440
 JOSEPH W. ELDER, 1480
 GEOFF ELEY, 668
 JULIE ELWORTH, 936
 REBECCA JEAN EMIGH, 214
 CYNTHIA FUCHS EPSTEIN, 181
 RICHARD V. ERICSON, 167, 1459
 SUSAN A. FARRELL, 1483
 MARCUS FELSON, 1497
 SARAH FENSTERMAKER, 950
 WILLIAM FINLAY, 399
 MICHAEL G. FLAHERTY, 1195
 NEIL FLIGSTEIN, 1189
 SIMON FRITH, 706
 JOHN H. GAGNON, 990
 JOSEPH GALASKIEWICZ, 190
 JOHN A. GARCIA, 1176
 MADELAINE GERBAULET-VANASSE, 928
 GARY GEREFFI, 446
 ANTHONY GIDDENS, 388
 JESS GILBERT, 714
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 JEFFREY GOLDFARB, 223
 JACK A. GOLDSTONE, 391
 VICTOR V. GOLUBCHIKOV, 1249
 JEFF GOODWIN, 923
 STEVEN L. GORDON, 664
 ALLEN GRIMSHAW, 1214
 DOUGLAS T. GURAK, 210
 JEFFREY K. HADDEN, 188
 FREDERIC W. HAFFERTY, 676
 JOHN A. HALL, 396, 1523
 JOHN R. HALL, 1471
 PETER M. HALL, 1491
 DAVID HALLE, 432
 MICHAEL HAMMOND, 981
 PHILLIP E. HAMMOND, 1198
 NANCY WEISS HANRAHAN, 169
 LINGXIN HAO, 428
 KIRK E. HARRIS, 1515
 SHIRAH W. HECHT, 183
 PIERRE HEGY, 186
 JOHN HERITAGE, 171
 RICHARD A. HILBERT, 982
 JEROME L. HIMMELSTEIN, 716
 THOMAS A. HIRSCHL, 1513
 DOROTHY HOLLAND, 179
 PAUL HOLLANDER, 1519
 ROBERT HOLTON, 1521
 CHRIS HOWELL, 1251
 HSIN-HUANG MICHAEL HSIAO, 219
 DAVID M. HUMMON, 424
 AIDA HURTADO, 229
 CHERYL HYDE, 954
 LAURENCE R. IANNACCONI, 448
 KEIKO IKEDA, 1475
 KIYOSHI IKEDA, 702

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| BENTON JOHNSON, 961 | PAUL D. LOPES, 1466 | LAUREL RICHARDSON, 913 |
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| STEPHEN R. G. JONES, 201 | ARMAND L. MAUSS, 187 | SONYA O. ROSE, 933 |
| ERLING JORSTAD, 960 | DOUGLAS W. MAYNARD, 976 | TERRY J. ROSENBERG, 1503 |
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| JACK KATZ, 945 | RALPH MILIBAND, 709 | GILBERT ROZMAN, 916 |
| IRA KATZNELSON, 698 | RICHARD G. MITCHELL, JR., 173 | MICHAEL RUSTIN, 1510 |
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| JOHN D. KELLY, 1200 | STEPHEN P. MUMME, 1501 | ERIC SCHAEFER, 1464 |
| WILLIAM W. KELLY, 1478 | BEVERLY Y. NAGEL, 405 | BARRY SCHWARTZ, 1201 |
| MICHAEL D. KENNEDY, 1244 | MARIE AUGUSTA NEAL, 965 | DMITRI SHALIN, 409 |
| DAVID I. KERTZER, 973 | MARGARET K. NELSON, 948 | GI-WOOK SHIN, 1237 |
| ANNE M. KHADEMIAN, 1187 | W. LAWRENCE NEUMAN, 1174 | ROGER SILVERSTONE, 1206 |
| VALARIE KING, 216 | ALAN NEUSTADTL, 1178 | BENJAMIN D. SINGER, 662 |
| BERT KLANDERMANS, 403 | ALICE O'CONNOR, 1193 | CHARLES W. SMITH, 419 |
| MALCOLM W. KLEIN, 1224 | RICHARD OFSHE, 435 | PHILIP SMITH, 420 |
| DAVID KNOKE, 203 | JOHAN P. OLSEN, 1239 | TOM W. SMITH, 430 |
| PETER KWONG, 1233 | ANN SHOLA ORLOFF, 1167 | DAPHNE SPAIN, 437 |
| CHRISTINE LAFIA, 422, 1462 | AARON M. PALLAS, 1229 | JAMES V. SPICKARD, 193 |
| JANET L. LAURITSEN, 986 | TOBY L. PARCEL, 688 | MARJORIE E. STARRELS, 946 |
| ROBIN LEIDNER, 942 | CHAN-UNG PARK, 171 | STEPHEN J. STEIN, 700 |
| MARC LENDLER, 1505 | HAROLD PERKIN, 206 | GEORGE STEINMETZ, 1191 |
| DAVID LEVINE, 1231 | TED PERLMUTTER, 1473 | MITCHELL STEVENS, 177 |
| EMANUEL LEVY, 1208 | HARRY PERLSTADT, 1495 | |
| RICHARD LEWIS, 692 | | |

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| | | |
|--------------------------------|------------------------------|--------------------------|
| TERESA A. SULLIVAN, 655 | PETER UHLENBERG, 683 | KATHRYN WARD, 442 |
| SZONJA SZELÉNYI, 936 | WILEY LEE UMPHLETT, 974 | GEORGE W. WENZEL, 397 |
| TONY TAM, 1235 | MARTHA VAN HAITSMAN, 1169 | PETER WHALLEY, 679 |
| STEPHEN LUNG WAI TANG, 1517 | LOÏC J. D. WACQUANT, 426 | JOHN WILSON, 940 |
| JOSEPH TATAREWICZ, 195 | CATHERINE E. WALSH, 433 | DIANA WYLIE, 704 |
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| | | MAYER N. ZALD, 1182 |
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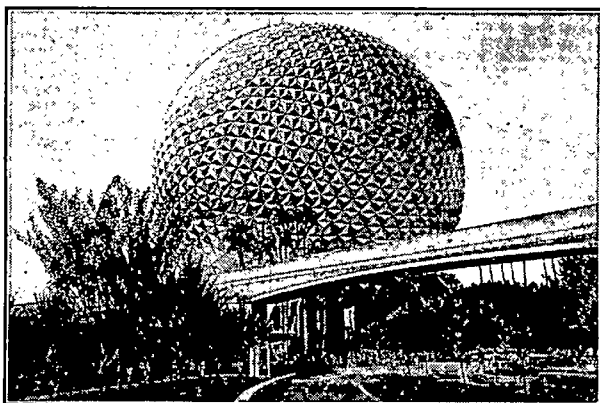
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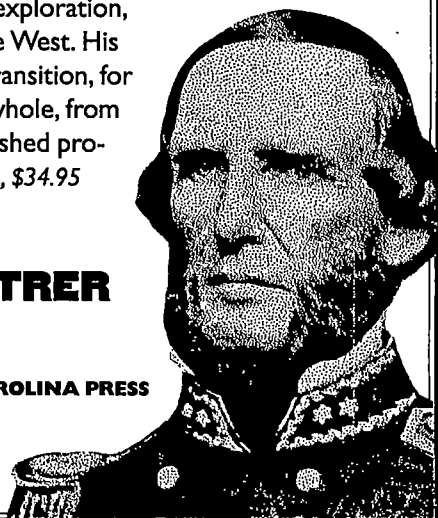
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